Geology:

With the U.S. Forest Service, Matt led investigations to determine hillslope stability of areas proposed for timber harvest in the central Oregon Coast Range. Specific activities were as follows:

- Mapped geology in the field, and used aerial photographic interpretation and mathematical models to determine slope stability.
- Coordinated his research with community members who were concerned with natural resource protection.
- Characterized the geology of an aquifer that serves as the sole source of drinking water for the city of Medford, Oregon.

As a consultant with Dames and Moore, Matt led geologic investigations of two contaminated sites (later listed on the Superfund NPL) in the Portland, Oregon, area and a large hazardous waste site in eastern Oregon. Duties included the following:

- Supervised year-long effort for soil and groundwater sampling.
- Conducted aquifer tests.
- Investigated active faults beneath sites proposed for hazardous waste disposal.

Teaching:

From 1990 to 1998, Matt taught at least one course per semester at the community college and university levels:

- At San Francisco State University, held an adjunct faculty position and taught courses in environmental geology, oceanography (lab and lecture), hydrogeology, and groundwater contamination.
- Served as a committee member for graduate and undergraduate students.
- Taught courses in environmental geology and oceanography at the College of Marin.

Matt taught physical geology (lecture and lab and introductory geology at Golden West College in Huntington Beach, California from 2010 to 2014.

Invited Testimony, Reports, Papers and Presentations:

Hagemann, M.F., 2008. Disclosure of Hazardous Waste Issues under CEQA. Presentation to the Public Environmental Law Conference, Eugene, Oregon.

Hagemann, M.F., 2008. Disclosure of Hazardous Waste Issues under CEQA. Invited presentation to U.S. EPA Region 9, San Francisco, California.

Hagemann, M.F., 2005. Use of Electronic Databases in Environmental Regulation, Policy Making and Public Participation. Brownfields 2005, Denver, Coloradao.

Hagemann, M.F., 2004. Perchlorate Contamination of the Colorado River and Impacts to Drinking Water in Nevada and the Southwestern U.S. Presentation to a meeting of the American Groundwater Trust, Las Vegas, NV (served on conference organizing committee).

Hagemann, M.F., 2004. Invited testimony to a California Senate committee hearing on air toxins at schools in Southern California, Los Angeles.

Brown, A., Farrow, J., Gray, A. and **Hagemann, M.**, 2004. An Estimate of Costs to Address MTBE Releases from Underground Storage Tanks and the Resulting Impact to Drinking Water Wells. Presentation to the Ground Water and Environmental Law Conference, National Groundwater Association.

Hagemann, M.F., 2004. Perchlorate Contamination of the Colorado River and Impacts to Drinking Water in Arizona and the Southwestern U.S. Presentation to a meeting of the American Groundwater Trust, Phoenix, AZ (served on conference organizing committee).

Hagemann, M.F., 2003. Perchlorate Contamination of the Colorado River and Impacts to Drinking Water in the Southwestern U.S. Invited presentation to a special committee meeting of the National Academy of Sciences, Irvine, CA.

Hagemann, M.F., 2003. Perchlorate Contamination of the Colorado River. Invited presentation to a tribal EPA meeting, Pechanga, CA.

Hagemann, M.F., 2003. Perchlorate Contamination of the Colorado River. Invited presentation to a meeting of tribal repesentatives, Parker, AZ.

Hagemann, M.F., 2003. Impact of Perchlorate on the Colorado River and Associated Drinking Water Supplies. Invited presentation to the Inter-Tribal Meeting, Torres Martinez Tribe.

Hagemann, M.F., 2003. The Emergence of Perchlorate as a Widespread Drinking Water Contaminant. Invited presentation to the U.S. EPA Region 9.

Hagemann, M.F., 2003. A Deductive Approach to the Assessment of Perchlorate Contamination. Invited presentation to the California Assembly Natural Resources Committee.

Hagemann, M.F., 2003. Perchlorate: A Cold War Legacy in Drinking Water. Presentation to a meeting of the National Groundwater Association.

Hagemann, M.F., 2002. From Tank to Tap: A Chronology of MTBE in Groundwater. Presentation to a meeting of the National Groundwater Association.

Hagemann, M.F., 2002. A Chronology of MTBE in Groundwater and an Estimate of Costs to Address Impacts to Groundwater. Presentation to the annual meeting of the Society of Environmental Journalists.

Hagemann, M.F., 2002. An Estimate of the Cost to Address MTBE Contamination in Groundwater (and Who Will Pay). Presentation to a meeting of the National Groundwater Association.

Hagemann, M.F., 2002. An Estimate of Costs to Address MTBE Releases from Underground Storage Tanks and the Resulting Impact to Drinking Water Wells. Presentation to a meeting of the U.S. EPA and State Underground Storage Tank Program managers.

Hagemann, M.F., 2001. From Tank to Tap: A Chronology of MTBE in Groundwater. Unpublished report.

Hagemann, M.F., 2001. Estimated Cleanup Cost for MTBE in Groundwater Used as Drinking Water. Unpublished report.

Hagemann, M.F., 2001. Estimated Costs to Address MTBE Releases from Leaking Underground Storage Tanks. Unpublished report.

Hagemann, M.F., and VanMouwerik, M., 1999. Potential Water Quality Concerns Related to Snowmobile Usage. Water Resources Division, National Park Service, Technical Report.

VanMouwerik, M. and **Hagemann, M.F.** 1999, Water Quality Concerns Related to Personal Watercraft Usage. Water Resources Division, National Park Service, Technical Report.

Hagemann, M.F., 1999, Is Dilution the Solution to Pollution in National Parks? The George Wright Society Biannual Meeting, Asheville, North Carolina.

Hagemann, M.F., 1997, The Potential for MTBE to Contaminate Groundwater. U.S. EPA Superfund Groundwater Technical Forum Annual Meeting, Las Vegas, Nevada.

Hagemann, M.F., and Gill, M., 1996, Impediments to Intrinsic Remediation, Moffett Field Naval Air Station, Conference on Intrinsic Remediation of Chlorinated Hydrocarbons, Salt Lake City.

Hagemann, M.F., Fukunaga, G.L., 1996, The Vulnerability of Groundwater to Anthropogenic Contaminants on the Island of Maui, Hawaii. Hawaii Water Works Association Annual Meeting, Maui, October 1996.

Hagemann, M. F., Fukanaga, G. L., 1996, Ranking Groundwater Vulnerability in Central Oahu, Hawaii. Proceedings, Geographic Information Systems in Environmental Resources Management, Air and Waste Management Association Publication VIP-61.

Hagemann, M.F., 1994. Groundwater Characterization and Cleanup at Closing Military Bases in California. Proceedings, California Groundwater Resources Association Meeting.

Hagemann, M.F. and Sabol, M.A., 1993. Role of the U.S. EPA in the High Plains States Groundwater Recharge Demonstration Program. Proceedings, Sixth Biennial Symposium on the Artificial Recharge of Groundwater.

Hagemann, M.F., 1993. U.S. EPA Policy on the Technical Impracticability of the Cleanup of DNAPL-contaminated Groundwater. California Groundwater Resources Association Meeting.

Hagemann, M.F., 1992. Dense Nonaqueous Phase Liquid Contamination of Groundwater: An Ounce of Prevention... Proceedings, Association of Engineering Geologists Annual Meeting, v. 35.

<u>**Other Experience:**</u>

Selected as subject matter expert for the California Professional Geologist licensing examination, 2009-2011.

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EDUCATION

UNIVERSITY OF CALIFORNIA, LOS ANGELES **B.S. ENVIRONMENTAL SCIENCES & ENVIRONMENTAL SYSTEMS AND SOCIETY** *JUNE 2016*

PROJECT EXPERIENCE

SOIL WATER AIR PROTECTION ENTERPRISE ***SANTA MONICA, CA***

AIR QUALITY SPECIALIST

SENIOR PROJECT ANALYST: CEQA ANALYSIS & MODELING

- Modeled construction and operational activities for proposed land use projects using CalEEMod to quantify criteria air pollutant and greenhouse gas (GHG) emissions.
- Organized presentations containing figures and tables that compare results of criteria air pollutant analyses to thresholds.
- Quantified ambient air concentrations at sensitive receptor locations using AERSCREEN, a U.S. EPA recommended screening level dispersion model.
- Conducted construction and operational health risk assessments for residential, worker, and school children sensitive receptors.
- Prepared reports that discuss adequacy of air quality and health risk analyses conducted for proposed land use developments subject to CEQA review by verifying compliance with local, state, and regional regulations.

SENIOR PROJECT ANALYST: GREENHOUSE GAS MODELING AND DETERMINATION OF SIGNIFICANCE

- Evaluated environmental impact reports for proposed projects to identify discrepancies with the methods used to quantify and assess GHG impacts.
- Quantified GHG emissions for proposed projects using CalEEMod to produce reports, tables, and figures that compare emissions to applicable CEQA thresholds and reduction targets.
- Determined compliance of proposed land use developments with AB 32 GHG reduction targets, with GHG significance thresholds recommended by Air Quality Management Districts in California, and with guidelines set forth by CEQA.

PROJECT ANALYST: ASSESSMENT OF AIR QUALITY IMPACTS FROM PROPOSED DIRECT TRANSFER FACILITY

- Assessed air quality impacts resulting from implementation of a proposed Collection Service Agreement for Exclusive Residential and Commercial Garbage, Recyclable Materials, and Organic Waste Collection Services for a community.
- Organized tables and maps to demonstrate potential air quality impacts resulting from proposed hauling trip routes.
- Conducted air quality analyses that compared quantified criteria air pollutant emissions released during construction of direct transfer facility to the Bay Area Air Quality Management District's (BAAQMD) significance thresholds.
- Prepared final analytical report to demonstrate local and regional air quality impacts, as well as GHG impacts.

PROJECT ANALYST: EXPOSURE ASSESSMENT OF LEAD PRODUCTS FOR PROPOSITION 65 COMPLIANCE DETERMINATION

- Calculated human exposure and lifetime health risk for over 300 lead products undergoing Proposition 65 compliance review.
- Compiled and analyzed laboratory testing data and produced tables, charts, and graphs to exhibit emission levels.
- Compared finalized testing data to Proposition 65 Maximum Allowable Dose Levels (MADLs) to determine level of compliance.
- Prepared final analytical lead exposure Certificate of Merit (COM) reports and organized supporting data for use in environmental enforcement statute Proposition 65 cases.

ACCOMPLISHMENTS

- **Academic Honoree,** Dean's List, University of California, Los Angeles *MAR 2013, MAR 2014, JAN 2015, JAN 2016*

Shawn Smallwood, PhD
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Riverside County Planning Department
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27 August 2017

RE: Palo Verde Mesa Solar Project FEIR

Dear Mr. Brady,

I write to comment on the Palo Verde Mesa Solar Project Final Environmental Impact Report (FEIR). Specifically, I write to reply to responses to my earlier comments on the Palo Verde Mesa Solar Project Draft Environmental Impact Report (DEIR). My replies are numbered consistent with the responses in the FEIR. My qualifications as an expert appear in my original comment letter of 16 November 2016.

O4-49 The response mischaracterizes my comment by suggesting I argued for a comprehensive inventory of all species. I did not make this argument. Rather, I pointed out how the DEIR's factual statement of how many species were found can be misleading when the survey effort was so slight; that is, the factual statement carries little useful information content for decision-makers and the public because it summarizes a metric – numbers of species – that was unmeasurable by the surveys that were performed. Based on a cursory effort to assess habitat, the EIR should not be presenting the illusion that a species inventory was performed. The response to my comment makes this very point – that it was not the intent of the surveys to inventory species of wildlife. But now Riverside County wants to have it both ways by claiming a species inventory was out of scope while also claiming that its species counts are accurate.

It turns out, after the burrowing owl survey report was released with the FEIR, that more species were detected than were reported in the DEIR. Among special-status species found during burrowing owl surveys, but not reported in the DEIR, were ferruginous hawk, yellow warbler, yellow-headed blackbird, and desert kit fox. It turns out, that contrary to response O4-49, the DEIR was far from accurate. Other species reported as having been detected during the burrowing owl surveys but not reported in the DEIR included at least 3 species of arthropod (grasshoppers, desert ironclad beetle, red velvet ant, tarantula), 4 species of reptile (zebra-tailed lizard, desert iguana, desert horned lizard, Great Basin whiptail), 23 species of bird (ruddy duck, Indian peafowl, Gamble's quail, ferruginous hawk, American coot, killdeer, spotted dove, white-winged dove, mourning dove, greater roadrunner, great-horned owl, lesser nighthawk, western wood-pewee, ash-throated flycatcher, western kingbird, eastern kingbird, American crow, bushtit, and yellow warbler, MacGillivray's warbler, red-winged blackbird, great-tailed grackle, yellow-headed blackbird), and 3 mammals (round-tailed ground squirrel, woodrat, desert kit fox). In other words, with just a little additional survey effort in the burrowing owl surveys, there were more species (33 species) of wildlife detected than reported in the DEIR (29 species). And this was the point I was making, but which response O4-49 dismisses as irrelevant and inaccurate. I know from experience that a reasonable survey effort would result in many more species being detected on the project site, including more special-status species, than the additional species detected during the burrowing owl surveys (Figure 1). Decision-makers and the public should be provided the findings of a reasonable survey effort and a more reliable summary of existing conditions.

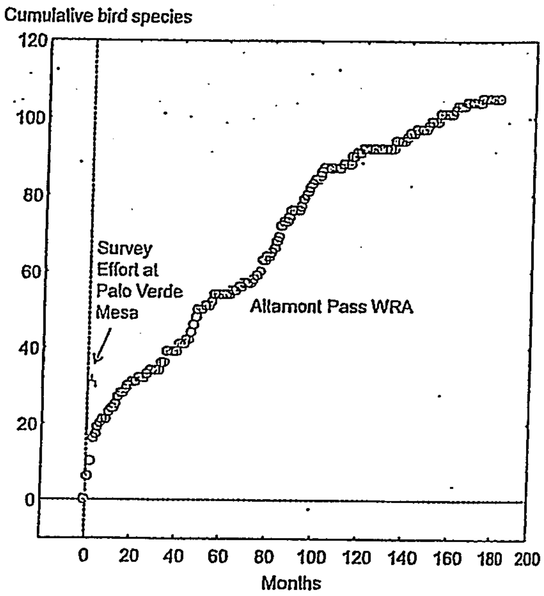


Figure 1. *Generously assuming a survey effort at Palo Verde Mesa of about 5 months (March through June 2013 for burrowing owl surveys and a couple of days of surveys for habitat assessment in October 2011), and assuming a species detection rate similar to other studies (here it is the utilization surveys in the Altamont Pass Wind Resource Area), one can expect about 15% of the diurnal species to have been detected at Palo Mesa Verde. A serious effort to characterize the species inventory at Palo Verde Mesa would have taken many months. Another acceptable approach would be to assume potentially occurring species as present and then assess impacts and formulate mitigation accordingly.*

O4-50 The response lumps two of my comments into one and proceeds to mischaracterize the first of the two comments. I commented on the lack of reporting of field methods and qualifications of survey personnel, whereas the response defends some surveys performed in August 2013. My original comment remains unaddressed.

In response to the second of my two comments in this reply, the County defends its surveys by saying that surveys were repeated in August 2013, "generally" relying on established survey protocols. However, my second comment under O4-50 was about the inappropriate timing of an October survey for detecting multiple special-status species that would not be in the area during October. The August survey faced the same problem with inappropriate timing. The response does not address my original comment that survey timing coincided with times of year when multiple potentially occurring special-status species were not present due to their migratory status.

O4-51 The response explains that species occurrence likelihood determinations were not based solely on site visits or field surveys, but also on habitat conditions and available occurrence records. This explanation would suffice had the occurrence likelihoods erred on the side of caution, consistent with the precautionary principle in risk assessment (National Research Council 1986, O'Brien 1990, Shrader-Frechette and McCoy 1992). Unfortunately, the occurrence likelihoods erred on the side of advancing the project (see my original comments) even in the face of high uncertainty over the status of many of our most rare and precious resources, i.e., species of wildlife. Project impacts on species of wildlife should not be dismissed in the absence of reasonable efforts to assess the status of those species on and around the project site. There is no more effective means of assessing the status of wildlife species than performing appropriate surveys to detect the species. No survey effort was directed toward detections of nocturnal species of wildlife, as my comment pointed out, so the County lacked any observational basis for determining whether species were present or absent.

Part of the explanation for the poor reporting of survey times was that the surveys were intended to assess habitat rather than inventorying species. This is a strange explanation because habitat is defined by species' use of the environment (Morrison et al. 1998, Smallwood 2002). The approach taken by the County appears to have been to not survey for wildlife while determining habitat suitability across a 3,400-acre project area. The only basis for determining that >90% of the project area is unsuitable for wildlife is the absurd assumption that any soil touched by a tractor-drawn disk is forever more unusable. Even the County's burrowing owl survey report refutes this absurd assumption by reporting the increasing abundance of ground squirrels and other wildlife species across the large expanse of "fallowed agriculture."

O4-52 According to the reply, the status of two species I used as examples in my comment was upgraded from 'absent' to 'low' potential to occur, but only for the purpose of clarification. The response points out that the revised status does not change determinations of project impacts on these species. In other words, the FEIR admits that the species might occur on the project site, but whether they occur on the site makes no difference to determining project impacts. Given this logic, and given the lack of minimization or compensatory mitigation measures, why bother with CEQA?

O4-53 The response goes to great length to justify not having paid any attention to the DRECP, which listed 36 special-status species as likely occurring in the project area but for which the EIR makes no mention. This is not a small number of species – **36 species**, but rather represents a glaring omission by those who prepared the DEIR. Instead of acknowledging and addressing the shortfall, the FEIR doubles down on the incompetence of the DEIR by refusing to consider the species identified by wildlife professionals as likely to occur in the project area. Ironically, the response argues that on-site information is superior to information appearing in other reports, because response O4-51 argues that on-site information is not so important, after all, and that other sources of information were indeed relied upon to make determinations of species occurrence likelihoods. It appears to me that the EIR is based on whichever reports or sources of information conveniently support species occurrence determinations made in favor of the project. After all, the DRECP is not just another of the 'many' reports the County can choose from; it was developed in support of renewable energy development in the Mojave and Colorado Desert regions of California. The DRECP was developed by wildlife biologists representing multiple state and federal resource agencies. Frankly, ignoring it in this context is unbelievable, and is certainly contrary to the spirit and intent of CEQA.

O4-54 This response characterizes my approach for assessing occurrence likelihoods as based on speculation while the approach leading to such determinations in the EIR are "good-faith projections based on available evidence..." My approach, however, preceded me by many decades – it is not *my* approach, but rather the approach of any scientist performing risk assessment in advance of management decisions. Erring on the side of caution in the face of high uncertainty is not "speculation." It is a rational, scientific approach for preventing egregious mistakes. The evidence that supposedly guides the "good-faith projections" mentioned in the response is central to deciding the merits of either approach. Lack of evidence of presence is not evidence of absence. This evidence rule is the founding justification for protocol-level detection surveys, which the County appears unwilling to use. Good-faith projections based on false evidence of absence is speculation at best and otherwise scientifically incompetent.

Response O4-54 again lumps two of my comments into one. There was overlap between the two comments, but there was also a difference. Response O4-54 simply summarized my comment about the DEIR's misleading impact conclusion based on loss of golden eagle foraging habitat, but the response did not address my comment.

O4-55 The response did not address my comment, despite the lengthy argument made. The response seized on a narrow issue around an example that I raised in my comment, and then proceeded to argue around the narrow issue. My comment was about potential attraction of wildlife to the project due to the lake effect and other factors. My comment was about the DEIR having been less comprehensive in its impacts assessment than it should have been, and having failed to acknowledge the sorts of surprises discovered at other renewable energy projects.

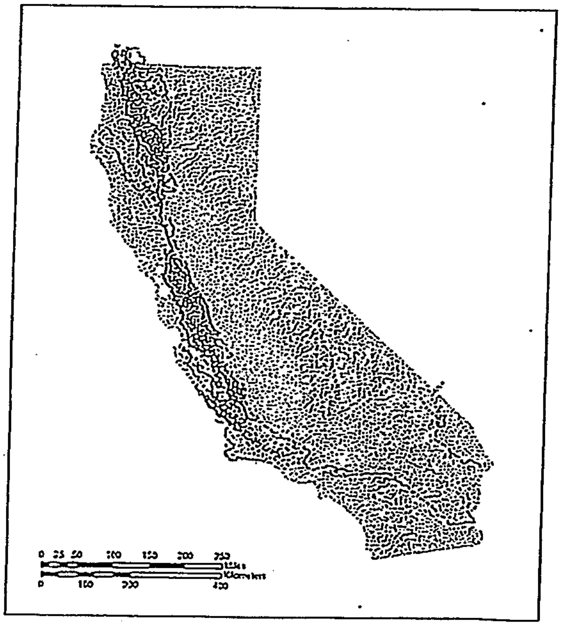
O4-57 The response provides a little more clarity to the maps in the figures I referenced, but the figures would be clearer by stating the source of the data right in the figure legend. The dates of the surveys and the purpose of the surveys should be provided in the figure legend so that the reader can more carefully interpret the map points. The reader needs to know that the mapped species locations were based on grossly inadequate survey efforts, and that many more map locations would have been depicted had protocol-level surveys been performed and had they been performed across the entire project area instead of a few peripheral (convenient) locations.

O4-58 The last sentence of this response reads, "*Therefore, the pre-construction surveys would identify the presence of any special-status species, including burrowing owls, on the project site prior to construction.*" This claim reveals either gross misunderstanding of the types of surveys needed to detect special-status species or a strong bias in favor of project approval with the least cost paid to mitigation. It is a false claim. Pre-construction 'take-avoidance' surveys are not the same as detection surveys, the former of which are intended to minimize impacts of an already-approved project and the latter of which are intended to inform decision-makers and the public by either generating evidence of absence or the numerical basis for formulating mitigation measures. If pre-construction surveys were suitable for detecting any special-status species, then why would resource agencies have developed detection survey guidelines?

O4-59 The response was to some other argument, I suppose, but not to my comment. My comment went to the information content of a statement about golden eagles on the project site, which was "*Golden eagles or their nests were not observed during surveys...*" This statement implies that golden eagles are unlikely to be affected by the project. My argument was that the cursory surveys performed at the project site were entirely unsuitable for detecting golden eagles regardless of whether they were nesting or flying around. That eagles were not observed was of no meaning to any impacts assessment. It was a misleading statement that appears intended to persuade a reader that golden eagles are so scarce as to be unaffected by the project. It lacked foundation because appropriate surveys for eagle use of the area were not performed.

O4-60 The response alleges that I provided no evidence of golden eagle avoidance of intensive human land uses. I provided Figure 1 in my comment letter, which depicts the flight path of an eagle dispersing from California's Diablo Range. This flight path provides ample evidence of avoidance, but it is not alone; my colleagues and I have seen this same pattern many times (Figure 2). After all, golden eagle studies have been performed in remote areas because it is only in remote areas where researchers can find any eagles to study. This said, however, eagles do venture onto anthropogenic landscapes when no other options are available.

Figure 2. GPS/GSM telemetry data collected from 22 golden eagles captured by East Bay Regional Park District's Doug Bell in the Diablo Range, 1 January 2013 through 1 May 2017. Each purple dot is an eagle position, which number >1,100,000 in this image. Nearly all of the positions were located in remote areas, and the exceptions were wherever golden eagles had no choice to fly over when traveling to reach other remote areas.



The response also refers me to response O4-54, but I fail to understand what O4-54 has to do with my comment about insufficient analysis of loss of golden eagle habitat.

O4-61 The response again conflates pre-construction take avoidance surveys with detection surveys. Whereas it would be helpful to prevent on-site badgers from being crushed by construction machinery, the absence of detection surveys means that no basis has been established for formulating appropriate compensatory mitigation. In other words, the intent appears to inconsequentially destroy badgers that go undiscovered during pre-construction surveys and to inconsequentially destroy foraging and breeding habitat to the extent of the project's boundary. The project needs to compensate for the impacts, but to do so it needs either (1) project-wide detection surveys, or (2) an estimate of the number of badgers affected based on the numerical capacity of the site.

O4-62 The response again conflates pre-construction take avoidance surveys with detection surveys. Pre-construction surveys would be standard practice to help prevent on-site kit foxes from being crushed by construction machinery, but the lack of detection surveys means that no basis has been established for formulating appropriate compensatory mitigation. In other words, the intent appears to inconsequentially destroy kit foxes that go undiscovered during pre-construction surveys and to inconsequentially destroy foraging and breeding habitat to the extent of the project's boundary. The project needs to compensate for the impacts.

O4-63 In response to my comment about desert tortoise impacts and the need for protocol-level detection surveys, I was referred to responses A6-14 and A6-15. These responses again conflate pre-construction take avoidance surveys with detection surveys. They are not the same thing, and they are intended for different purposes. By not performing detection surveys, the County makes determinations of low to marginal habitat suitability based on an absence of evidence approach. Protocol-level detection surveys are intended to achieve minimum standards of search effort before concluding species absence. These surveys were established by wildlife ecologists experienced with desert tortoise, in this case, so a lot of thought and expertise went into the establishment of minimum survey standards. Just as I commented on the DEIR, I reiterate that detection surveys are needed to determine presence or absence of desert tortoise, and if tortoises are present the detection surveys are needed to estimate the approximate distribution and number of tortoises on and around the project.

O4-64 Although I argued that the burrowing owl survey report(s) should be attached to a recirculated DEIR, the County attached a survey report to the FEIR under Appendix C. The response to my comment refers to survey reports as if there was more than one report, but I see evidence of only one report.

The response characterizes the burrowing owl surveys as having achieved the standards in the CDFW (2012) guidelines. I must refute this characterization. Table 1, below, lists the minimum standards appearing in CDFW (2012) as well as my assessment of how closely the burrowing owl surveys came to meeting the standards. I found that the survey report met none of the 4 standards related to minimum qualifications of survey personnel. Perhaps the surveyors were qualified, but no evidence was provided to demonstrate their qualifications. I found that the surveys failed to meet 8 of the 12 habitat assessment standards and only partly met a ninth standard. None of the breeding-season survey standards were met, mostly because the surveys were restricted to a mere 9.8% of the project area. Most of the area not surveyed was bypassed after it was arbitrarily and fallaciously determined to be unsuitable for burrowing owls. Viewing the photos of the fallowed agricultural land, and being aware of many ground squirrel burrows occurring across these several thousand acres, I find the reasoning for the unsuitable determination to be absurd. The burrowing owl is a disturbance-adapted species, and in my experience I would expect to find burrowing owls on most of the 90% of the land that was not surveyed. Finally, I found that the majority of reporting standards were not met. In summary, the burrowing owl survey effort did not meet the standards of detection surveys recommended by CDFW (2012).

O4-65 According to the CDFW (2012) guidelines, it is the surveyors' responsibility to demonstrate qualifications with burrowing owl surveys. The County's response attempts to shift the burden to me, an expert making comments, but it is up to Power Engineers to demonstrate why their survey personnel were qualified to perform breeding-season surveys for burrowing owls. Whenever I submit another paper on burrowing owls for scientific peer review, I make a point of citing my past papers partly as an aid to the reviewers in determining my experience level with the species. Similarly, Power Engineers needs to inform the readers of the EIR about the levels of experience of their survey personnel. I would be satisfied with inexperienced biologists helping with the surveys so long as they were accompanied by experienced biologists, because we all have to start somewhere, but as a reviewer of the EIR I am unable to find evidence that any of the surveyors were experienced with burrowing owls. If they were, then report accordingly.

O4-66 The response mostly repeats the same argument that I commented on originally. The only subtle difference was the last statement that it seems obvious burrowing owls would avoid areas having been recently disked. However, it is not obvious that this would always be the case. I have many times observed burrowing owls use areas that were recently disked. A developer in Davis, California disked a field desired for a housing project, despite being warned not to do so by the City of Davis. Some of the owls stayed at their burrows despite the burials caused by the disking. A few years ago the US Navy attempted to displace burrowing owls from an airfield (Lemoore) by disking, but the owls quickly re-inhabited the burrows damaged by disking. More typical of disked areas is burrowing owl occupancy of the field margins. Just this year I monitored two pairs of burrowing owls nesting along the berm supporting a fence separating two disked fields. The owls produced chicks, which later fledged, even though they lived within a matrix of disked agricultural fields. Biologists qualified to perform surveys would be aware that the burrowing owl is a disturbance-adapted species, and that it uses agricultural fields, graded lands, gravel and dirt roads, and intensively grazed range. The largest, most dense concentration of burrowing owls in the world occurs amidst intensively farmed fields in California's Imperial Valley (DeSante et al. 2007b, also see Smallwood et al. 2013). The County committed a potentially egregious error by dismissing >90% of the project area as unsuitable for burrowing owls simply because it was disked.

Table 1. Assessment of EIR consistency with CDFW's (2012) recommended burrowing owl survey protocol. Standards are numbered to match those in CDFW (2012).

andard in CDFW (2012)	Assessment of surveys performed	Was the standard met?
inimum qualifications of biologists performing surveys and impact assessments		
Familiarity with the species and local ecology	Some of the scientific literature is cited, but there is no evidence of personal familiarity with burrowing owl ecology. As an example, had Power Engineers been familiar with the species, their survey report would not have concluded that burrowing owls are active at dawn and dusk and sometimes at night; instead, it would have concluded the species is most active at night.	No
Experience conducting habitat assessments and eding and non-breeding season surveys	No evidence of experience was provided.	No
Familiarity with regulatory statutes, scientific earch and conservation related to burrowing owls	A few papers were cited on burrowing owl ecology, but no information was provided that would demonstrate knowledge of burrowing owl conservation.	No
Experience with analyzing impacts on burrowing owls	No information provided.	No
bitat assessment		
Conduct at least 1 visit covering entire site and offsite fer to 150 m	There was a visit in October 2011, but the County summarily deemed the majority (77.2%) of the project footprint as unsuitable because it was classified as fallow agricultural	No
Prior to site visit, compile relevant biological ormation on site and surrounding area	No information reported other than reviews of surveys performed at neighboring projects.	No
Check available sources for occurrence records	Other sources were reviewed.	Yes
Identify vegetation cover potentially supporting rowing owls on site and vicinity	No details provided other than dismissing most of the project footprint as unsuitable for burrowing owls.	No
) Describe project and timeline of activities	Timeline of activities was unreported.	No
) Regional setting map showing project location	Provided.	Yes

Standard in CDFW (2012)	Assessment of surveys performed	Was the standard met?
5c) Detailed map with project footprint, topography, andscape and potential vegetation-altering activities	The project footprint was mapped.	Yes
5d) Biological setting including location, acreage, errain, soils, geography, hydrology, land use and nanagement history	Some of this information was reported, though some was cursory in description.	Partial
5e) Analysis of relevant historical information oncerning burrowing owl use or occupancy	None provided.	No
5f) Vegetation cover and height typical of temporal and patial scales relevant to the assessment	Not provided.	No
5g) Presence of burrowing owl individuals, pairs or sign	Not provided, as no surveys were performed.	No
5h) Presence of suitable burrows or burrow surrogates	The increasing number of ground squirrels and their burrows was mentioned (page 7), but Power Engineers inexplicably failed to conclude that burrowing owls likely increased in occurrence along with the ground squirrels.	No
reeding season surveys		
erform 4 surveys separated by at least 3 weeks	Achieved over 323 acres deemed suitable by Riverside County, but not achieved over 90% of the project footprint.	No
survey between 15 February and 15 April	Achieved over 323 acres deemed suitable by Riverside County, but not achieved over 90% of the project footprint.	No
-3 surveys between 15 April and 15 July	Achieved over 323 acres deemed suitable by Riverside County, but not achieved over 90% of the project footprint.	No
survey following June 15	Achieved over 323 acres deemed suitable by Riverside County, but not achieved over 90% of the project footprint.	No
Valk transects spaced 7 m to 20 m apart	Achieved over 323 acres deemed suitable by Riverside County, but not achieved over 90% of the project footprint.	No
can entire viewable area using binoculars at start of ach transect and at 100 m intervals	No surveys were performed over >90% of the project area.	No
ecord all potential burrow locations determined by resence of owls or sign	No surveys were performed over >90% of the project area.	No

Standard in CDFW (2012)	Assessment of surveys performed	Was the standard met?
Survey when temperature >20° C, winds <12 km/hr, and cloud cover <75%	Mostly achieved on <10% of project area, but no surveys were performed over >90% of the project area.	No
Survey between dawn and 10:00 hours or within 2 hours before sunset	Mostly achieved on <10% of project area, but no surveys were performed over >90% of the project area.	No
Identify and discuss any adverse conditions such as disease, predation, drought, high rainfall or site disturbance	Possibly achieved on <10% of project area (high winds were reported during one survey), but no surveys were performed over >90% of the project area.	No
Survey several years at projects where activities will be ongoing, annual or start-and-stop to cover high nest site fidelity	Only one year of surveys was performed.	No
Reporting should include:		
(1) Survey dates with start and end times and weather conditions	Achieved.	Yes
(2) Qualifications of surveyor(s)	None provided.	No
(3) Discussion of how survey timing affected comprehensiveness and detection probability	No surveys were performed.	No
(4) Description of survey methods including point count dispersal and duration	None provided.	No
(5) Description and justification of the area surveyed	Provided, but justification was unsupportable, consisting merely of a County staff member requiring surveys over small areas covered by natural vegetation.	No
(6) Numbers of nestlings or juveniles associated with each pair and whether adults were banded or marked	No surveys were performed over >90% of the project area, so survey effort was incomplete.	No
(7) Descriptions of behaviors of burrowing owls observed	No surveys were performed over >90% of the project area, so survey effort was incomplete.	No
(8) List of possible burrowing owl predators in the area, including any signs of predation of burrowing owls	Only observed predator species were mentioned.	Partial
(9) Detailed map showing all burrowing owl locations and potential or occupied burrows	A map was provided showing the locations of where sign was found, but no surveys were performed over >90% of	Partial

tandard in CDFW (2012)	Assessment of surveys performed	Was the standard met?
	the project area, so no mapped results were possible over the majority of the project area.	
10) Signed field forms, photos, etc.	None provided.	No
11) Recent color photos of project site	Provided.	Yes
12) Copies of CNDDB field forms	None provided in report.	No

O4-67 The response does not convince me that the surveyors were qualified to do breeding-season surveys. Like I explained in my comment, experienced burrowing owl biologists would know how to confirm nesting without harassing the owls or having to assume nesting. And by the way, the surveyors did not assume any owls were nesting; my comment was in response to the surveyors not being able to determine whether the owls were nesting, relying on an excuse about not wanting to approach too closely and disturbing the owls. Qualified biologists wouldn't have needed to explain being unable to determine whether the owls were nesting.

O4-68 The response points out that 12 references were used to support the burrowing owl study. However, I had commented on the DEIR without having seen the burrowing owl study report because the report was not circulated with the DEIR. But even so, the burrowing owl report *does not* include 12 references of scientific studies. It includes a reference to eBird, 3 references of online data bases managed by CDFW, 2 references of the survey guidelines (same thing twice, almost), 4 consultant reports, 1 referenced source consisting of the same 312-word blurb on the internet site that I originally commented about, but this time with the author names rearranged, and only 1 scientific publication. The County's response supported my comment that Power Engineers was unqualified to perform the surveys recommended by CDFW (2012).

O4-69 The response did not address my comment. My comment was about whether Power Engineers was sufficiently qualified to perform the breeding-season burrowing owl surveys recommended by CDFW (2012). I provided evidence questioning the qualifications of Power Engineers. The response addressed the adequacy of mitigation, but not my comment.

O4-70 The response points out that the burrowing owl survey report is included with the FEIR, and includes "the suitable habitat assessment." I compared the burrowing owl survey report in the FEIR to the reporting in the burrowing owl mitigation and monitoring plan included in App. D of the DEIR. The wording was nearly the same, with most paragraphs copied from one document to the next. What I didn't see in either document was a suitable habitat assessment. The County simply divided the project area into categories of vegetation cover and land use, including 77.2% of the area characterized as fallow agriculture. Then the County declared agriculture to be unsuitable for burrowing owls, even though the vast majority of California's burrowing owls reside on agricultural landscapes (DeSante et al. 2007a).

If what appears in the FEIR is a suitable habitat assessment, then CEQA loses meaning because anyone can declare any habitat category to be suitable or unsuitable without ever examining how the species in question actually uses the environment. Habitat is defined by the species' use of the environment (Hall et al. 1997, Morrison et al. 1998, Morrison and Hall 2002), and usually we infer habitat from controlled studies comparing use and availability of portions of the environment (Smallwood 2002). Leaving habitat assessment to "good-faith," arbitrary assignments of broad land-cover categories would be exposing CEQA to bias, because too often the consultant working for the project proponent will be tempted to assign large portions of the project area to a category that can then be declared unsuitable.

There are two scientifically legitimate ways to assess habitat on a project area such as Palo Verde Mesa. The direct method would be to perform protocol-level surveys across the entire project area before declaring any of it unsuitable. The resulting distribution of burrowing owls would inform the investigators of those portions of the project area as suitable. A more careful direct approach would be to also study movement patterns of the resident owls to learn which portions of the project area they select for nesting, which portions they select for non-breeding season refugia, and which portions they rely upon for foraging and socializing. After having performed burrowing owl surveys over many years, I continue to be surprised by finding burrowing owls in environmental settings I would not have predicted based on experience or scientific literature. Simply put, one does not know what is truly suitable until one looks hard enough to find out.

The indirect method would be to categorize the project area into land-cover types that comport with habitat conditions described for burrowing owls in the scientific literature. Rather than categorizing land cover based on mapping convenience, it would make more sense to define land-cover categories based on what is known about burrowing owl habitat selection. For example, it would make more sense to delineate boundaries around areas supporting ground squirrels (Smallwood et al. 2009a,b; 2013), raised berms, or south- to southwest-oriented slopes (Smallwood and Morrison papers in review, and Smallwood unpublished data).

Although I recall I said there are two scientifically legitimate ways to assess habitat, there really is a third way in the absence of direct and careful indirect approaches. The third way is to admit ignorance on the distribution and abundance of burrowing owls on the project area and to err on the side of caution in light of that ignorance. I already commented on this approach on page 12 of my original letter, so I will not repeat it here other than to point out that an investigator could estimate the numerical capacity of burrowing owls within the project area boundary based on densities reported elsewhere.

Unfortunately, the FEIR is based on none of these legitimate approaches for assessing habitat, but is instead based on an arbitrary determination that 90% of the project area is unsuitable for burrowing owls. No scientific source was cited in support of this determination, nor was there any personal experience cited. The only justification for it was the assumption that burrowing owls would not find habitat value on soils that had been disked some time ago. As I pointed out earlier, this justification is refuted by abundant evidence to the contrary. The habitat assessment in the FEIR remains unsuitable, and could result in devastating adverse impacts to burrowing owls.

O4-71 The burrowing owl survey details I said were missing from the DEIR have been added to the FEIR, but they pertain to only 9.8% of the area that needed to be surveyed for burrowing owls.

O4-72 The response claims that the burrowing owl survey reporting details I said were missing from the DEIR had actually been reported in the DEIR. The vast majority of the details I said were missing were indeed missing. These details remain missing from the FEIR. The reporting has not achieved the minimum standards recommended in CDFW (2012).

O4-73 The response refers me to O4-49, which addresses cumulative impacts and appears irrelevant to the comment at issue. The response also refers me to O4-54, which argues for their approach of relying on "*good-faith projections based on available evidence...*," or in other words the freedom to speculate at will. The response also refers me to O4-74.

O4-74 The response mischaracterizes my testimony by claiming that I found my recommended approach for predicting numerical capacity to be unreliable. I did not make this finding; otherwise, why would I have recommended it? The response then argues that the habitat assessment was based on the County's "good-faith projections" based on surveys appearing in other consultant reports, and that this is how CEQA works. Before I reply, I want to again state for the record that the direct approach for assessing habitat via adequately performed on-site surveys is the best approach. In the absence of this direct approach, and in the absence of the careful indirect approach I descripted in reply to O4-70, my suggestion for predicting numerical capacity would be far more reliable than the approach used by the County to assess impacts and formulate mitigation. Without adequate on-site surveys, what better information to rely upon than density estimates reported in peer-reviewed scientific journals or numerical estimates in similar solar project settings appearing in consultant reports? Given the spirit and intent of CEQA to rely upon the best available information, what would qualify as better information than the information I recommended? It certainly would not be the arbitrary, convenient determination that 90% of the project area is unsuitable because it had once been touched by a tractor-pulled disk.

O4-75 The response refers me to O4-74, thereby ignoring my comment about the numbers of burrowing owls and other special-status species likely to be lost to the project and to cumulative impacts.

O4-76 This response again mischaracterizes my comment by falsely claiming I found my recommended approach to be unreliable. This claim is absurd on its face – why would I recommend an approach I found unreliable? I recommended an averaging of density estimates of breeding burrowing owls counted at other solar projects and projecting the average to the Palo Verde Mesa project area, but I also suggested an approach to avoid the effects of one source of variability among the counts. My suggested approach was simple and made use of good information from other solar projects, and I even did the work for the County, but the County would rather stick to its unfounded determination that 90% of the project area is unsuitable for burrowing owls.

O4-77 This response repeats the mischaracterizations of my testimony in responses 04-74 through O4-76. The responder selected certain words from throughout a paragraph and reassembled them to distort my comment. The response is very misleading, and therefore fails to address my comment.

O4-90 This response claims that my comment was incorrect, but the response mischaracterized my comment, again. My comment read, "*...the DEIR (page 3.4-48) goes on to conclude that the project will not interfere with wildlife movement in the region because ample open space occurs in the project area. However, this conclusion is contradicted by Figure 3-1 on DEIR page 3-13, which depicts proposed or approved solar energy projects nearly completely covering Palo Verde Mesa and McCoy Wash, leaving no open space for wildlife to move across or stop-over within relatively low terrain intervening Big Maria Mountains, Little Maria Mountains and McCoy Mountains.*" The response falsely characterized my comment as referring to a "vast" area within a 40-mile radius of the project site, and then proceeds to refute my comment by arguing that wildlife will be free to move across this vast area. I was obviously discussing the area of cumulative project buildout within a defined area, and not the area out to 40 miles. Given the misleading response, my comment remains unaddressed.

O4-91 The logical flow of the argument in this response is flawed by starting with the premise that wildlife cannot move across a landscape lacking a known wildlife migration corridor. Absurd to the point of almost funny, the premise is false because wildlife often move outside of known wildlife migration corridors and they also move for reasons other than migration. The corridor concept is often misconstrued by environmental consultants as largely natural constructs rather than largely manufactured constructs intended to mitigate the effects of habitat fragmentation (Smallwood 2015), but the response takes this misunderstanding to another level. The response almost reads as if a project's impacts on wildlife movement cannot be significant unless the project interferes with a known wildlife migration corridor. If this was the case, then CEQA would state it this way. But CEQA does not state it this way. According to CEQA, the standard is whether the project will "*Interfere substantially with the movement of any native resident or migratory fish or wildlife species or with established native resident or migratory corridors...*" The response misinterpreted the standard by ignoring the broader first clause related to movement and working only from the second clause focused on established migratory corridors. The response took another liberty in replacing 'established' with 'known' migratory corridors, thereby removing the recognition of such a corridor from the species' perspective to the environmental consultant's perspective. Given the twisting of the meaning of the CEQA standard and the distortion of what is known about wildlife movement patterns and the corridor concept, my original comment about fences disrupting wildlife movement remains unaddressed.

The FEIR also lumps two of my comments into the one response. My second comment about cumulative impacts was not addressed, either.

O4-92 The response claims that only a small portion the project footprint overlaps with the habitat linkage/wildlife movement corridor depicted in Figure III.7-27 of the DRECP. I had suggested overlaying this mapped habitat linkage with the project footprint because it looks to me as though the linkage overlaps the project footprint entirely, and that it also overlaps the other projects depicted in the EIR's cumulative impacts map. Instead of overlaying the map coverage, the response makes a dubious claim of only slight overlap. My comment was inadequately addressed, and the DRECP remains unutilized in the FEIR. Furthermore, it appears that the DRECP's recognition of a major habitat linkage/wildlife movement corridor refutes the County's earlier claim that no known wildlife corridors overlap the project area (O4-91).

O4-93 The response argues that total cumulative acreage alone does not determine the significance of cumulative impacts. Of course this is true, and I never commented otherwise. However, this false cause argument does not invalidate my comment that the destruction of 127,339 acres of special-status species habitat will be cumulatively significant and devastating for some species. The response's false cause, taken to the extreme, would have us believe that obliterating the land surface of the Earth could not be determined cumulatively significant because cumulative acreage alone does not determine significance. The cumulative acreage of solar projects is very large, and will result in the takings of many individuals of multiple special-status species and will interfere with the movement of wildlife across the region.

The next false premise used in the response is that the cumulative impacts will not be considerable because they will happen on already disturbed lands. Taken to the extreme, as above, the obliteration of Earth's entire land surface could not be determined cumulatively significant because the entire land surface has been disturbed in one way or another. Even on the remotest corners of the globe, pollutants have fallen, average annual temperatures altered, precipitation patterns changed, and so on. That a land area has been disturbed does not preclude the determination that a cumulative impact is considerable. Such arguments seem inconsistent with the spirit and intent of CEQA.

Next, the response argues that the project's contribution to cumulative impacts is so small as to not materially affect the nature and extent of the impact. So small! In my experience, 3,400 acres of land conversion has never been considered small. Additionally, the project takes the last patch of open space in the area not already planned or under development for solar power generation. It adds collision risk to many birds and automobile collision risk to many mammals and reptiles. The size and lasting effects of the project cannot possibly be considered so small as to contribute no more than trivial impacts as the response argues.

Regarding my comment that the project's cumulative impact on burrowing owl might result in extirpation of the species from McCoy Wash and possible extirpation from Palo Verde Mesa, the response argues that none of the project's structures will be installed in McCoy Wash. This response ignores my inclusion of Palo Verde Mesa in my prediction. It also ignores the context of my comment, which was about cumulative impacts. If solar projects are built out to the extent indicated in the EIR, then burrowing owls will have no place to live within McCoy Wash or Palo Verde Mesa. My comment stands.

O4-94 The response claims that my comment falsely asserted the DEIR redefined open space as expanses of natural vegetation. I checked again on Page 3.4-51 and confirmed that the DEIR did exactly as I asserted. However, the response goes on to argue that it is irrelevant what one terms the lands covered by natural vegetation. This argument fails to address my point that the overly narrow definition artificially narrows the impacts assessment. The County has decided that all 'agricultural' land, including 73.2% of the entire area that might have been disked once, holds no value for wildlife. This decision resulted in a false and devastatingly narrow range of existing conditions that also resulted in a grossly inadequate survey effort for special-status species and an insufficient foundation for formulating mitigation measures. As stated in my original comment, the entire project area is open space. The County should not engage in redefining the terminology of environmental scientists to justify inadequate survey efforts, trivialize impacts, and skimp on mitigation.

O4-95 The response argues that by building one set of poles along the Gen-tie line instead of two, cumulative impacts are reduced. However, this argument does not eliminate the truth of my comment that the project contributes cumulatively to the impacts that will be caused by the gen-tie line. Given the mere argumentative trick used in the response and the lack of any substantial revision to the EIR or project, my comment remains unaddressed.

O4-96 The response rejects the conclusion in my comment, but the argument for doing so relied upon the same flawed premise for dismissing cumulative impacts as appears in the DEIR. The response simply restated the flawed premise that residual impacts remaining after mitigation would be too small to contribute toward significant cumulative impacts. This premise is inconsistent with CEQA, which characterizes cumulative effects as "*two or more individual effects which, when considered together, are considerable or which compound or increase other environmental impacts*" (Guidelines § 15355). Furthermore, the mitigation measures that would allegedly leave such tiny residual impacts are largely insubstantial and would not avoid or offset large-scale habitat loss and destruction of many individuals of multiple special-status species. The so-called residual impacts would essentially equal the unmitigated impacts.

The response says that each incremental impact was considered and found not to be cumulatively considerable after mitigation, but I did not see any analysis of cumulative impacts. I did not see a list of candidate cumulative impacts, nor any methodology for assessing impacts. However, apparently unrecognized by the County, there was a list of candidate cumulative impacts, albeit incomplete, in the list of special-status species potentially affected by the project. The list of special-status species is evidence that cumulative impacts have already occurred, and that any project affecting any of these species will be contributing significant cumulative impacts. Each species on this list was rigorously vetted by wildlife scientists and approved by decision-making bodies at the state and federal levels. The project's contribution to habitat elimination in combination with the habitat elimination of other solar projects qualifies as cumulative impacts for each special-status species potentially occurring in the area.

Whereas the FEIR mentioned incremental impacts, no other types of cumulative impacts were discussed. In addition to the CEQA definition of cumulative impacts, the National Research Council (1986) also recognized the following types of cumulative effects:

- Time-crowded perturbations, in which perturbations are so frequent that the effects of one have not dissipated prior to the next perturbation;
- Space-crowded perturbations, in which the effects overlap spatially;
- Synergisms, in which reactions between different types of perturbations cause qualitatively and quantitatively different ecological responses; and,
- Incremental and decremental effects, in which the functional integrity of the species or resource at issue is eroded.

Certainly, the effects of multiple adjacent large-scale solar projects will overlap spatially, so there will be space-crowded perturbations. There will be synergistic effects such as caused by habitat loss across nearly the entirety of McCoy Wash, leaving no alternative movement routes through or across the Wash. There will be time-crowded perturbations in the traffic deaths of animals scavenging the previous day's roadkill. None of these or any other type of cumulative effects are analyzed in the FEIR.

The National Research Council (1986) also recognized the following types of cumulative effects:

- Addition of materials to the environment, causing hazardous accumulations;
- Repeated removal of materials or organisms causing depletions to dangerous levels;
- Environmental changes over large areas and long times, thus weakening resource integrity and reliability, or loss of genetic variability;
- Combined or synergistic effects;
- Habitat fragmentation; and,
- Repeated intervention in natural processes, such as wildfire, causing accumulation of fuel and disruption of biological adaptations to the frequency of fires, or such as peak stream flows, causing sediment accumulation and vegetation infill of wide streambeds.

Relevant to the above list of effects will be the accumulation of dust from maintenance traffic and the need to clean the solar panels. Habitat fragmentation also will be a major issue, given the adjacency of multiple large-scale solar projects. None of these effects are analyzed in the FEIR.

The cumulative effects analysis needs to be performed over again so that the public and decision-makers have the information needed to understand potential project impacts. The metric used in the EIR is uninterpretable because there is no metric, and gives the false impression of cumulative impacts being trivial in magnitude. An acceptable approach would be to convert habitat areas into numerical capacities, as I demonstrated in my comment letter, but for cumulative effects analysis another step is needed. Not only do areas of habitat loss need to be converted into numerical losses, but additional numerical losses also need to be predicted from the effects of habitat fragmentation (Smallwood 2015). Where wildlife will be impeded partially or entirely by loss and degradation of connectivity between habitat patches, that loss needs to be added to that which is simple habitat loss. Nothing like this has been attempted in the EIR.

O4-97 The response plays word games to modify definitions related to mitigation in an effort to improve the appearance of the EIR's mitigation plan. CEQA lists the following types of mitigation (Guidelines § 15370):

- Avoiding the impact altogether by not taking a certain action;
- Minimizing impacts by limiting the degree or magnitude of the action;
- Rectifying the impact by repairing, rehabilitating or restoring the impacted environment;
- Reducing or eliminating the impact over time by preservation and maintenance operations during the life of the project; and,
- Compensating for the impact by replacing or providing substitute resources or environments.

Although the intent of preconstruction surveys is often implied in the alternative naming of these surveys as "take-avoidance surveys," at best what they accomplish is rectifying impacts to a limited degree by rescuing detected individuals from the project site just before machinery moves in to crush them. I qualify the accomplishments of these rectifying actions as being "at best" and "limited" because they can actually lead to additional project impacts that EIR's rarely discuss – which are (1) the displacement of rescued individuals from their nest sites, refugia, and home ranges and (2) the dumping of them into areas that are either unable to support the species being translocated or that are already occupied by conspecifics that will subsequently be forced to compete for limited resources. This dumping action can be either direct or delayed by halting construction around a nest, but the end result is the same whether the animal at issue is translocated passively or actively.

Whereas preconstruction surveys likely help people feel better about the project, rescued animals or plants lose the environment they relied upon and are dumped into environments that were not of their choosing, and where their prospects for survival are greatly diminished. The dumping of these plants and animals into other areas further risks the survival of individuals in the receiving populations, thereby spreading the extent of the project's impacts. In some cases, preconstruction surveys likely generate more adverse impacts than they rectify, even though nobody wants to just stand by while a tractor crushes a desert tortoise or a pair of burrowing owls. However, many of these animals are going to perish anyway (Griffiths et al. 1989, Schulz 1997) or others that did not even live in the project area will be injured or killed or starved as a result of competition with desperate, translocated victims of the project. In one study of relocated burrowing owls, for example, many of the translocated owls collided with windows and automobiles soon after release (Schulz 1997). The process of translocation appears to be traumatic for the translocated individuals, as it probably also is for the conspecifics having to deal with the translocated animals released into their territories.

As part of its response, Riverside County also repeats my comment that the mitigation plan relies on preconstruction take-avoidance surveys and construction monitoring, whereas substantial mitigation is missing. The County refers me to responses O4-98 to O4-103, none of which address my concern that preconstruction surveys cannot replace detection surveys. Preconstruction surveys performed years after detection surveys will be much less effective than had they followed detection surveys performed the same year because the preconstruction surveys will be blind to the distribution and abundance of special-status species surveyed too long ago. In the case of burrowing owls, the 2013 detection surveys were grossly inadequate for the reasons I listed in Table 1 and associated text, but they also fail to represent the distribution of burrowing owls in 2017 or 2018. I learned from monitoring burrowing owls among 46 sampling plots in a 16,500-ha study area that the distribution in monitoring year one can predict the distribution the next year, but the distribution in year one is increasingly less predictive of future distributions with each subsequent year (Figure 3). Within five years, the hot spots of activity observed in 2011 became cold spots, whereas the hot spots in 2016 were in entirely new locations, just as it is likely to be the case at Palo Verde Mesa in the five years between the detection surveys of 2013 and the approaching spring of 2018. When it comes to burrowing owls, waiting longer than a year to begin construction since the last detection surveys will introduce changed circumstances because burrowing owls naturally shift activity areas in response to (1) resource depletion; (2) the need to escape parasite or predator loads; (3) aggregation of emigrant dispersers, forming new activity areas while the natal population senesces; and (4) a combination of these factors.

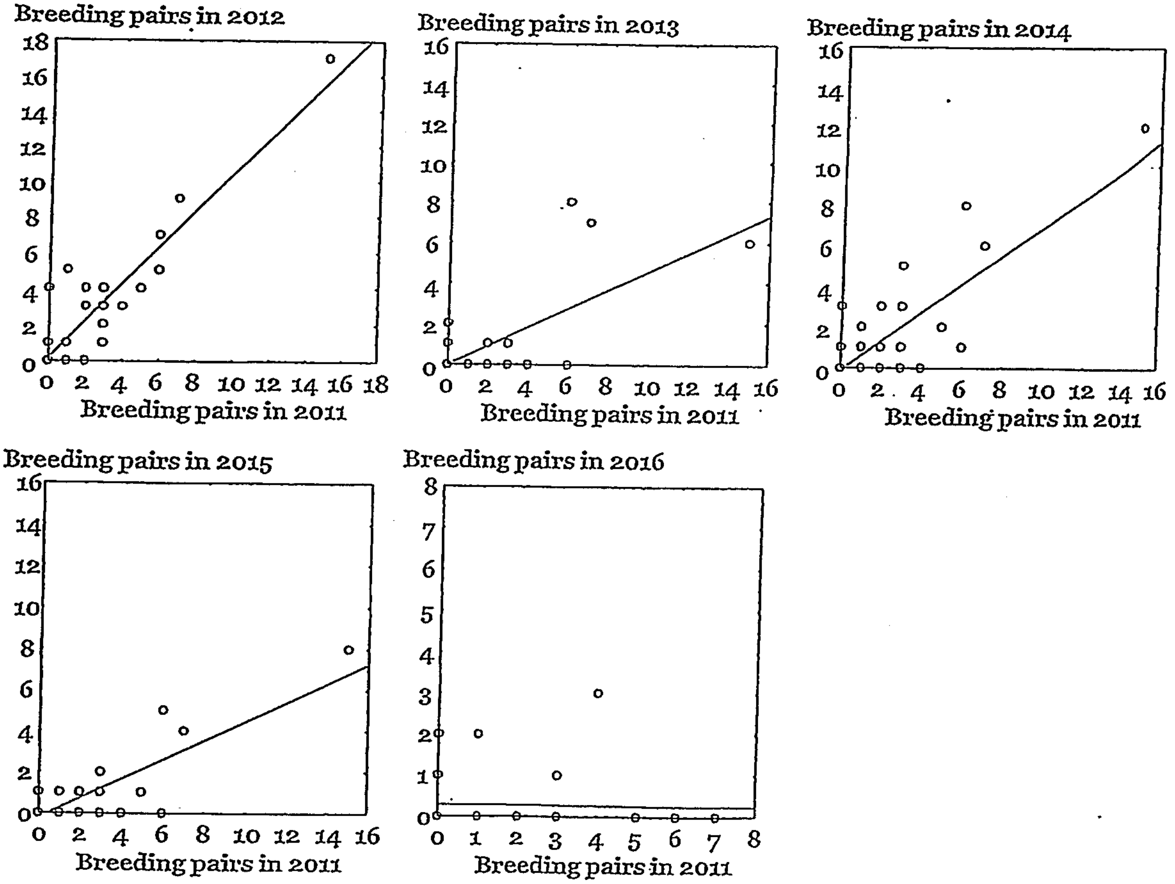


Figure 3. Breeding pairs of burrowing owls among sampling plots in the APWRA from 2012 through 2016 as functions of breeding pairs in 2011. Smallwood, unpublished data.

It is very important to refrain from temporally decoupling detection surveys and preconstruction surveys. The latter type of survey cannot replace the former, nor can it function properly when too much time elapses since having performed the type of surveys designed to measure distribution and abundance of a species, i.e., detection surveys. It has always been the intent of preconstruction take-avoidance surveys to follow closely behind detection surveys so that the information gained in the detection surveys can inform the preconstruction surveys.

O4-98 The response indicates that the candidate adaptive management measures would consist of (1) relocating PV panels and (2) rebuilding the solar project in 30 years. Two options, one with a 30-year timeline, is not very encouraging for adaptive management. This response justifies my original comment.

As for fatality thresholds triggering management action, the last sentence of the response is unsatisfactory. The statement makes no sense in the context of adaptive management, which is a structured step-by-step process that is supposed to be laid out in advance and in full transparency. Thresholds and responses are supposed to be decided up-front; otherwise the process is something other than adaptive management. I attached my comments on last year's Avian Solar Science Plan to show where adaptive management fits within a larger framework of research-based knowledge, measured impacts, mitigation, and monitoring. As is, the FEIR proposes an ad-hoc approach for deciding fatality thresholds as the project goes, and responding to any breaches of thresholds either with PV panels to yet-to-be-disclosed sites or rebuilding the project in some unspecified manner after 30 years. This is not adaptive management.

O4-99 The response justifies the project's adaptive management fatality triggers by claiming they are consistent with a U.S. Fish and Wildlife Service guidelines and will be put into a Bird and Bat Conservation Strategy (BBCS) that will serve as a 'living document.' However, I did not see any citation of any U.S. Fish and Wildlife Service guidelines. Is there a guidelines document that specifically includes the fatality thresholds in the EIR? It appears to me by the way the DEIR and response O4-99 were worded, that the County made up the fatality thresholds on its own. My comment stands, which is that excessive fatalities would have to be realized before anything is done about them.

O4-100 The response refers me to O4-99, which does not address my comment that the proposed 1 golden eagle per year will likely prove unsatisfactory as a fatality threshold triggering adaptive management. As having been a member of the Alameda Scientific Review Committee assigned to oversee fatality thresholds tied to adaptive management of a wind resource area, I am confident that never-ending arguments will emerge over the meaning of fatality finds when applied to a rate metric of 1 death per year. A rate metric requires multiple years of monitoring to arrive at an average, and the average will have a confidence range. If a rate metric is to be used, then a level of error around the point estimate needs to be decided upon in advance, and the number of years of monitoring leading to the point estimate needs to be decided. See my attached comments on the Avian-Solar Science Plan for a list of lessons learned from wildlife impacts caused by wind energy projects. As written currently, the FEIR provides ample opportunity for the project owner to argue around the fatality metric and to never have to mitigate the impact.

O4-101 The response refers me to four other responses related to my comments on the inadequacy of mitigation for burrowing owl and Mojave fringe-toed lizard. However, the response fails to address my point that the detection surveys were grossly inadequate, which contributed to inadequate mitigation. A mitigation ratio applied to an insufficient acreage of occupied habitat will yield insufficient mitigation.

Although the response referred me to four other responses regarding the burrowing owl mitigation of 146 acres, which I commented was too small, it did not refer me to response 03-106. In response 03-106, the County admits to relying on the same 146 acres already dedicated for mitigating the impacts to burrowing owl caused by the Blythe Mesa Solar Project. I do not believe that it is acceptable to use the same mitigation acreage twice for the same species, but using the methods I suggested in my comment letter (addressed on pages 12-14 of my comment letter and responses O4-74 and O4-75 in the FEIR), it is easy to compare predicted impacts of the projects to the protection benefits in the 146 acres of dual-use mitigation land. The combined acreage of Blythe Mesa Solar and Palo Verde Mesa is 7,060 acres. This acreage converted to km^2 and applied to the model in Figure 3 of my 20 November 2016 comment letter would be predicted to support 37 adult burrowing owls, whereas 146 acres would be predicted to support 11 adult burrowing owls, resulting in a post-construction net loss of 26 adult burrowing owls. Using the conservative mean density of burrowing owls counted at several large-area industrial solar project sites (see my comment letter), the area of the two projects would be predicted to support 131 adult burrowing owls, but the 146-acre mitigation site would be predicted to support 3 adult burrowing owls, resulting in a post-construction net loss of 128 adult burrowing owls. In other words the two approaches for estimating habitat capacity yields a range of 37 to 131 adult burrowing owls across the two project areas combined, but only 3 to 11 adult burrowing owls on the 146-acre mitigation site. Averaging between the two predictions would suggest a net loss of 77 adult burrowing owls, or a 92% loss of the breeding population, resulting from these two projects after mitigation. Whether relying on one approach or the other, or both averaged together, it is clear that the proposed mitigation is grossly inadequate.

As for collision impacts, the response fails to address my comment because it repeats its rationale for the arbitrary fatality thresholds and whatever else. It does not explain why none of the collision fatalities will be mitigated using compensatory measures.

The response to my comment about lack of compensatory mitigation for habitat loss was the most straightforward response to all of my comments. It reads, "*Regarding the thousands of acres of habitat loss, the Project would primarily remove agricultural and disturbed land covers. Given the limited value these land covers provide to wildlife and plants, the Project impacts are less than significant and its contribution to cumulative impacts is less than considerable. Accordingly, mitigation is not warranted.*" The County makes it perfectly clear that it deems the project area as worthless to wildlife and therefore unworthy of mitigation. Incredibly, the County comes to this conclusion after having decided arbitrarily to not search for wildlife or plants on >90% of the project area, making it easier to declare the project area is worthless to plants and wildlife. I cannot recall seeing another CEQA review that was so blatantly and deliberately failed to meet the minimum standards of CEQA.

Many species of wildlife were documented on the project site, and many more likely would have been detected had serious efforts been made to survey more than 10% of the area. Many of the species detected on site were special-status species. The project area is not worthless to wildlife. The thousands of acres of habitat destruction that will be caused by this project need to be mitigated.

The response to my comments about there being no mitigation for cumulative impacts were addressed in the same manner as the responses to my comments on habitat loss. The response repeats the false claim that individually mitigated projects cannot contribute considerably to cumulative impacts, and that residual impacts following mitigation would be trivial. As written, the FEIR fails to meet minimum standards of cumulative impacts assessment and mitigation. If the County's standard was consistent with the CEQA standard, there would be no need for cumulative effects analysis in CEQA.

O4-102 I agree with the response that CEQA does not require inclusion of the MRMP in the EIR, but including a draft MRMP would be consistent with the spirit and intent of CEQA to invite public participation with plans to minimize project impacts. In this case, when it comes to mitigating collision impacts caused by renewable energy projects, some of us in the public have more expertise than personnel at resource agencies. Experts should have the opportunity to participate with the mitigation and monitoring at industrial solar projects.

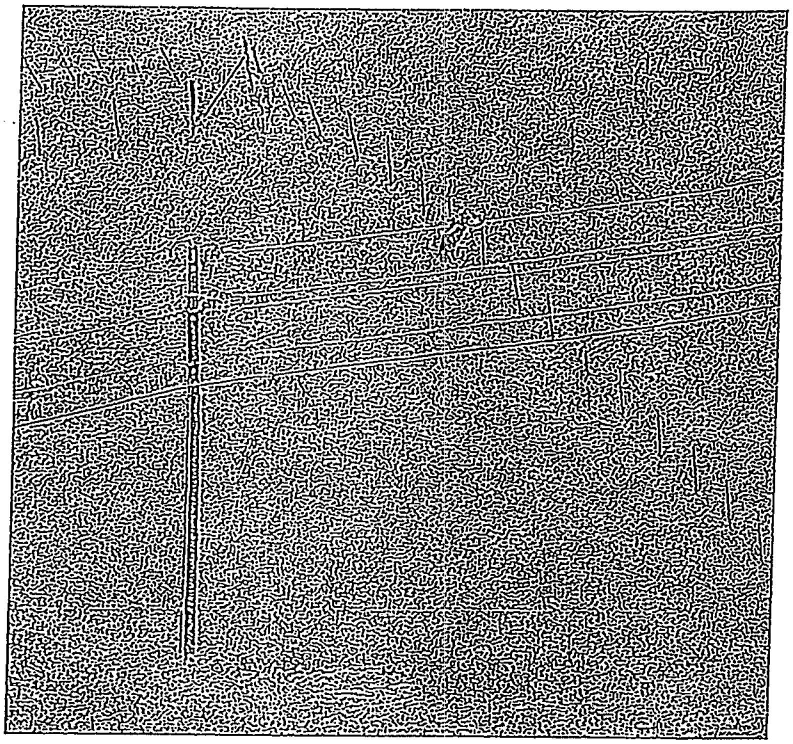
O4-103 The response lists excuses for not establishing a fund to deal with injured wildlife. I have been involved with wildlife injuries at renewable energy facilities for nearly 20 years, so I can say with confidence that wildlife rehabilitation facilities will be less able to rehabilitate project-injured animals without the up-front funding to support staff.

O4-104 The response characterizes my observation as an anecdote, therefore not worthy of consideration. As I noted in my comment, "I often see bald eagles far from water sources..." I saw two bald eagles far from water sources just last week. What's disturbing to me about the County's reaction to my comment is that the County expresses a high level of certainty over its speculative conclusions after Power Engineers barely spent any time at the project site.

O4-105 Rather than challenge my assertion that the County is unqualified to speculate over which age class of golden eagles is more vulnerable to collisions at wind projects (which is fine), the County instead assures me that mitigation measures will reduce golden eagle impacts to less than significant levels. However, not one of the measures appearing in the referenced mitigation methods will offset the impacts of 3,400 acres of habitat loss, and not one is clearly linked to preventing golden eagle collisions with the gen-tie or with automobiles when the eagles scavenge yesterday's roadkill. The referenced measures are vague, and none bear on habitat loss.

O4-106 Contrary to the response's false dichotomy, golden eagles can be both shy of intensive human land uses and still collide with transmission lines. I have personally found transmission line collision victims, even within intensive anthropogenic landscapes. In Figure 4, I show a photo I took of a golden eagle narrowly avoiding a collision with distribution lines in an industrial renewable energy project. This near-miss happens often, and sometimes I find the lethal results.

Figure 4. A golden eagle narrowly avoids colliding with electric distribution lines in Contra Costa County. Coincidentally, this eagle happened to be one tracked by Dr. Douglas Bell, myself and others – the CTT telemetry unit is visible on the eagle's back. Photo by Shawn Smallwood.



O4-107 The response faults me for not proving a negative. Of course I cannot provide evidence that the DEIR was inaccurate in claiming golden eagles don't die in the area due to their scavenging of roadkill; there are no records because there's been no monitoring. However, biologists have often encountered just this scenario all over the country. I was with an employee of the U.S. Fish and Wildlife Service just two years ago when we found a golden eagle fatality on the road, where it had landed to feed on roadkill antelope. Road traffic in the project area will increase as a result of the project, so road-caused mortality is likely to increase.

O4-108 According to the response, the County relied on more than the presence of an orchard to conclude that the project area unlikely provided foraging habitat for golden eagles. Now the County says that the project as a whole does not provide much cover for wildlife. However, this story does not comport with the reporting in the burrowing owl survey report, which describes the fallowed agricultural fields – composing 90% of the project area – as being increasingly used by ground squirrels and other small mammals. Any of us who have spent some time studying golden eagles can assure the County that ground squirrels serve as principal prey of golden eagles in California.

O4-109 The response doubles down on the DEIR's misleading referencing to sources that have nothing to do with bat displacement from either wind or solar projects. The impression given is that the County has no idea what it is talking about and is speculating in favor of the notion that the project will cause trivial impacts. My comment remains unaddressed.

Thank you for your consideration,

Shawn Smallwood

Shawn Smallwood, Ph.D.

REFERENCES CITED

DeSante, D. F., E. D.Ruhlen, and R. Scalf. 2007a. The distribution and relative abundance of burrowing owls in California during 1991-1993: Evidence for a declining population and thoughts on its conservation. In Barclay, J. H., K. W. Hunting; J. L. Lincer, J. Linthicum, and T. A. Roberts, Eds.; Proceedings of the California Burrowing Owl Symposium, November 2003. Bird Populations Monographs No. 1. The Institute for Bird Populations and Albion Environmental, Inc.: Point Reyes Station, CA, USA.

DeSante, D. F., E. D., Ruhlen, and D. K. Rosenberg. 2007b. Density and abundance of burrowing owls in the agricultural matrix of the Imperial Valley, California. Pages 104-108 in J. H. Barclay, K. W. Hunting, J. L. Lincer, J. Linthicum, and T. A. Roberts, editors. Proceedings of the California Burrowing Owl Symposium, November 2003. Bird Populations Monographs No. 1. The Institute for Bird Populations and Albion Environmental, Inc., Point Reyes Station, California, USA.

Griffith, B., J.M. Scott, J.W. Carpenter, and C. Reed. 1989. Translocation as a Species Conservation Tool: Status and Strategy. *Science* 245:477-480

Hall, L. S., P. R. Krausman, and M. L. Morrison. 1997. "The Habitat Concept and a Plea for Standard Terminology." Wildlife Society Bulletin 25:173-82.

Morrison, M. L., and L. S. Hall. 2002. "Standard Terminology: Toward a Common Language to Advance Ecological Understanding and Application." In *Predicting Species Occurrences: Issues of Scale and Accuracy*, edited by J. M. Scott, P. J. Heglund, M. Morrison, M. Raphael, J. Haufler, and B. Wall, 43-52. Island Press, Covelo, CA.

Morrison, M. L., B. G. Marcot, and R. W. Mannan. 1998. Wildlife- habitat relationships: concepts and applications, 2nd ed. University of Wisconsin Press Madison, WI.

National Research Council. 1986. Ecological knowledge and environmental problem-solving: concepts and case studies. National Academy Press, Washington, D.C.

O'Brien, M. 2000. Making better environmental decisions: an alternative to risk management. The MIT Press, Cambridge, Massachusetts.

Schulz, T. A. 1997. Observations, resightings, and encounters of rehabilitated, orphaned, and relocated burrowing owls. Journal of Raptor Research Reports 9:128-131.

Shrader-Frechette, K. S., and E. D. McCoy. 1992. Statistics, costs and rationality in ecological inference. Tree 7: 96-99.

Smallwood, K.S. 2002. Habitat models based on numerical comparisons. Pages 83-95 *in* Predicting species occurrences: Issues of scale and accuracy, J. M. Scott, P. J. Heglund, M. Morrison, M. Raphael, J. Haufler, and B. Wall, editors. Island Press, Covello, California.

Smallwood, K. S. 2015. Habitat fragmentation and corridors. Pages 84-101 in M. L. Morrison and H. A. Mathewson, Eds., Wildlife habitat conservation: concepts, challenges, and solutions. John Hopkins University Press, Baltimore, Maryland, USA.

Smallwood, K. S., L. Neher, and D. A. Bell. 2009. Map-based repowering and reorganization of a wind resource area to minimize burrowing owl and other bird fatalities. Energies 2009(2):915-943. http://www.mdpi.com/1996-1073/2/4/915

Smallwood, K. S., L. Neher, J. Mount, and R. C. E. Culver. 2013. Nesting Burrowing Owl Abundance in the Altamont Pass Wind Resource Area, California. Wildlife Society Bulletin: 37:787-795.

ATTACHMENT: REVIEW OF AVIAN-SOLAR SCIENCE PLAN

K. Shawn Smallwood, PhD

14 September 2016

The Center for Biological Diversity requested that I review an Avian-Solar Science Plan ("Plan") prepared to prioritize research needed to estimate, predict and mitigate impacts to birds caused by industrial solar development. Briefly, my qualifications for providing an expert review includes the following. I earned a Ph.D. degree in Ecology from the University of California at Davis in 1990, where I subsequently worked for four years as a post-graduate researcher in the Department of Agronomy and Range Sciences. My research has been on animal density and distribution, habitat selection, habitat restoration, interactions between wildlife and human infrastructure and activities, conservation of rare and endangered species, and on the ecology of invading species. I have authored more than 80 peer-reviewed papers, reports and book chapters on my research topics. I served as Chair of the Conservation Affairs Committee for The Wildlife Society – Western Section. I am a member of The Wildlife Society and the Raptor Research Foundation, and I've lectured part-time at California State University, Sacramento. I was also Associate Editor of wildlife biology's premier scientific journal, The Journal of Wildlife Management, as well as of Biological Conservation, and I was on the Editorial Board of Environmental Management.

I have performed avian surveys in California for three decades, mostly for studying anthropogenic impacts on birds such as golden eagle, Swainson's hawk and burrowing owl. My 17 years of research on renewable energy impacts on birds and bats has involved both diurnal and nocturnal behavior observations to understand causal factors of collisions, GPS/GSM telemetry of golden eagles, fatality rate estimation including the associated detection trials, and the effectiveness of mitigation measures. I also served for five years on the Alameda County Scientific Review Committee that was charged with overseeing the fatality monitoring and mitigation measures in the Altamont Pass Wind Resource Area, and I prepared many comment letters on proposed renewable energy projects, including many solar projects. I also collaborate with colleagues worldwide on the underlying science and policy issues related to renewable energy impacts on wildlife.

SUMMARY

Below is a quick summary of my comments on the Avian-Solar Science Plan:

- Goal and objectives appear reasonable;
- Over-emphasizes collision impacts, even though habitat loss is acknowledged early on as the obvious principal impact;
- Under-emphasizes transmission line impacts, and erroneously characterizes transmission lines as ancillary structures and their impacts as indirect (solar projects require transmission);

- Conceptual framework is difficult to follow, so I proposed an alternative framework;
- Provides no methodology for assessing impacts of habitat loss;
- Approach for assessing collision hazards are vague to nonexistent;
- Causal factors of solar project impacts are speculative, so priority research should test hypotheses related to these factors;
- For testing causal factors related to collisions, I suggest implementing experimental design principles;
- Summary of past and ongoing impact monitoring should be much more informative;
- Lessons learned from wind energy and wildlife are vague and too optimistic, so I provide some examples of lessons learned;
- The Plan targets population-level effects for impact assessment, so I explain the challenges of determining population impacts and I suggest that they are unnecessary;
- Approaches are vaguely described for pre-construction post-construction assessments, so I suggest some elements of the approaches that have worked for predicting impacts at wind projects, for estimating impacts post-construction, and for testing hypothesized causal factors;
- If adaptive management is to be the way forward, as proposed, then candidate mitigation measures need to be identified up front, in the Plan; and,
- Some of the terminology is vague.

COMMENTS

Goal and Objectives

The Plan's goal and objectives are sensible. I applaud the development of a plan with this goal and these objectives in mind. It is very important to organize the data needs and the scientific approaches for learning about impacts and how to minimize the impacts going forward with industrial solar development.

To achieve the stated goal and the objectives, the Plan needs greater focus and an improved logical flow. The Plan also needs a more honest appraisal of past and ongoing impact monitoring, peer review and transparency. It needs a more honest appraisal of adaptive management and the availability and effectiveness of impact-reduction measures. It needs greater emphasis on impacts associated with habitat loss and the reduced numerical and demographic capacities of landscapes on which industrial solar projects are built. It probably should also refrain from offering value judgements related to industrial solar energy generation and from comparing avian impacts caused by other human activities or climate change (e.g., page 11), lest the avian-solar science plan be perceived as somewhat biased in favor of industrial solar development.

Although one of the components of the Plan is to benefit from lessons learned by the development of wind energy (section 3.5), the Plan barely addresses issues around baseline studies and fatality monitoring at wind energy projects. It barely addresses fundamental scientific shortfalls associated with research and monitoring of impacts at wind projects. Instead, the Plan offers vague themes emerging from an industry-sponsored collaborative, such as *"Establishment of objectives that lead to good outcomes."* What good outcomes?

Below are some shortfalls that impeded scientific progress at wind energy projects, and which can benefit the avian-solar science plan by addressing them:

1. Industry resistance to scientific access, transparency and data-sharing;
2. Self-monitoring of impacts too often portrayed as equivalent to scientific monitoring;
3. The signing of too many confidentiality agreements, in my opinion, between consultants and wind companies, resulting in too much wind industry control of data and reports on impacts to public-trust resources, i.e., birds and bats;
4. Too few baseline and monitoring studies being submitted for publication in peer-reviewed scientific journals;
5. Efforts to trivialize wind energy impacts by comparing fatality rates at wind turbines to those caused by house cats, cars and windows of buildings, and by claiming that most of the fatalities were caused by predation or old age;
6. Reliance on cursory, haphazard surveys in baseline studies, resulting in poor or often nonexistent macro- and micro-siting of wind energy facilities to minimize avian impacts;
7. Insufficient use of statistical error or statistical power associated with data collected in baseline studies;
8. Relying too long on speculated causal factors, such as turbines mounted on lattice towers posing greater collision hazard than turbines mounted on tubular towers (this and other speculations turned out to be unsupported by data);
9. Justifying methodology in the face of mounting evidence of poor performance by arguing that consistency and standardization trump advancing methods;
10. Relying too long on erroneous methodological assumptions, such as fatality rates correlating positively with use rates measured during baseline studies (they do not: de Lucas et al. 2008, Ferrar et al. 2012, Smallwood 2016b);
11. Too much use of fatality search intervals that were too long for having a reasonable chance of detecting carcasses of bats and small birds;
12. Use of too many unfounded, arbitrary methods in fatality monitoring such as the maximum search radius and inter-transect spacing;

13. Too much use of unrealistic, poor field methodology when performing the critically important task of estimating carcass detection rates needed for adjusting fatality rate estimates for the proportion of fatalities not found;

14. Insufficient reporting of observer error rates associated with monitoring, such as species misidentifications during baseline studies and during fatality monitoring;

15. Frequent reporting of use rates and fatality rates (key metrics) at taxonomic levels greater than the species-level, and too often lumping species into larger groups when predicting fatality rates from use rates;

16. Too few opportunities to test the validity of hypothesized causal factors and the effectiveness of proposed mitigation measures using scientific experimentation;

17. Too often claiming that adaptive management will reduce fatality rates, but without revealing that impact-reduction measures known to be effective are nearly non-existent, an exception being operational curtailment used to reduce bat fatalities; and,

18. Insufficient intervention from regulatory agencies, thereby contributing to slow scientific progress toward estimating impacts, predicting impacts, macro- and micro-siting to minimize impacts, and implementing effective mitigation.

That transparency ought to be an issue in the solar Plan is evident on the Plan's title page. Nowhere is an author identified. A first principle of scientific reporting is to attribute authorship. The Plan should clearly identify the author(s).

Most of these 18 shortfalls – and I am sure I can identify more of them given more time to think about it – are too often defended as "industry standards," as if industry standards are equivalent to scientific standards; they are not. Science has proven to be a highly effective process for quickly understanding and solving problems, but it has to be practiced to the standards that are well-established by scientists. Industry standards are no substitute for scientific standards unless the industry standards meet the scientific standards. The avian-solar science plan should embrace scientific standards by first acknowledging the shortfalls in wind and wildlife research and mitigation, and then by incorporating the standards into the Plan. However, the current draft Plan made no mention of any of the 18 lessons learned that I listed above.

Conceptual Framework

The second section of the Plan presents a conceptual framework referred to as "avian life cycle conservation." The avian life cycle conservation appears potentially useful, but it is unclear how it translates into Plan elements later on. In other words, the avian life cycle conservation framework did not relate very well to the impacts and mitigation actions that I would expect to address at industrial solar projects, based on my experience. How do breeding, wintering and stop-over habitats relate to later Plan elements? I failed to see much connection.

As an example of the disconnect just pointed out, the Plan states that "*without an understanding of the migratory patterns of bird species, it may be difficult to conserve avian populations through local (i.e., site specific) management actions alone.*" But what does this mean? The Plan provided no explanation of how an industrial solar project's impacts on a species can be mitigated by knowing the species' migration route. Perhaps the meaning is that knowing the migration route might lead to a macro-siting decision, such as not going forward with a proposed project that happens to be located along the species' migration route. If so, the Plan could be improved by clarifying its meaning.

The flowchart in Figure 2-2 of the Plan serves as a useful starting point for laying out a Plan framework, but I found it difficult to follow and some of it seems of low relevance or out of place or mischaracterized. For example, habitat loss and fragmentation are characterized as indirect effects, but these are direct effects of solar energy development. Therefore I developed a framework based on my experience with wind and solar energy impacts (Figure 1). My alternative framework derives from the ecological indicators approach, which was also summarized for wind energy impacts in Smallwood and Thelander (2004). The indicators framework divides the information into categories representing a species' susceptibility to impacts, a species' vulnerability to impacts, predicted, measured or estimated impacts (red font in Figure 1), and responses in the form of mitigation measures. The effectiveness of mitigation measures can also feed back into informing about species' susceptibility and vulnerability, because after all, scientific understanding of these factors is poor at the outset of industrial solar development.

Each species has a susceptibility associated with each proposed project site. For example, a species' susceptibility to solar energy impacts would be zero if the species' geographic range does not overlap the proposed project site, or if the habitat at the project site is entirely unsuitable for the species. Directed surveys can also be used to determine presence or absence of a species on a proposed project site, but the danger of relying on such surveys is the well-known dynamic spatial distribution of animal species (Taylor and Taylor 1979). Every generation or so, activity centers of animal species typically shift locations, if they are able (not constrained). This shifting results from depletion of food resources while resources accumulate at other sites, the need to escape predator or parasite loads, the natural accumulation of dispersing young elsewhere while their natal populations senesce, or a combination of these factors. Another factor affecting susceptibility to solar project impacts is the rarity or sensitivity of the species, so if only a few are left, losing only a few can be a big problem. Or if the reproductive rate is low, then losing breeding birds can be a problem. Finally, each species moves across the landscape or behaves in particular ways affecting susceptibility. High-flying migrants might have no interaction with the solar project, whereas local territorial birds might attack reflected images of themselves on parabolic mirrors of solar thermal projects.

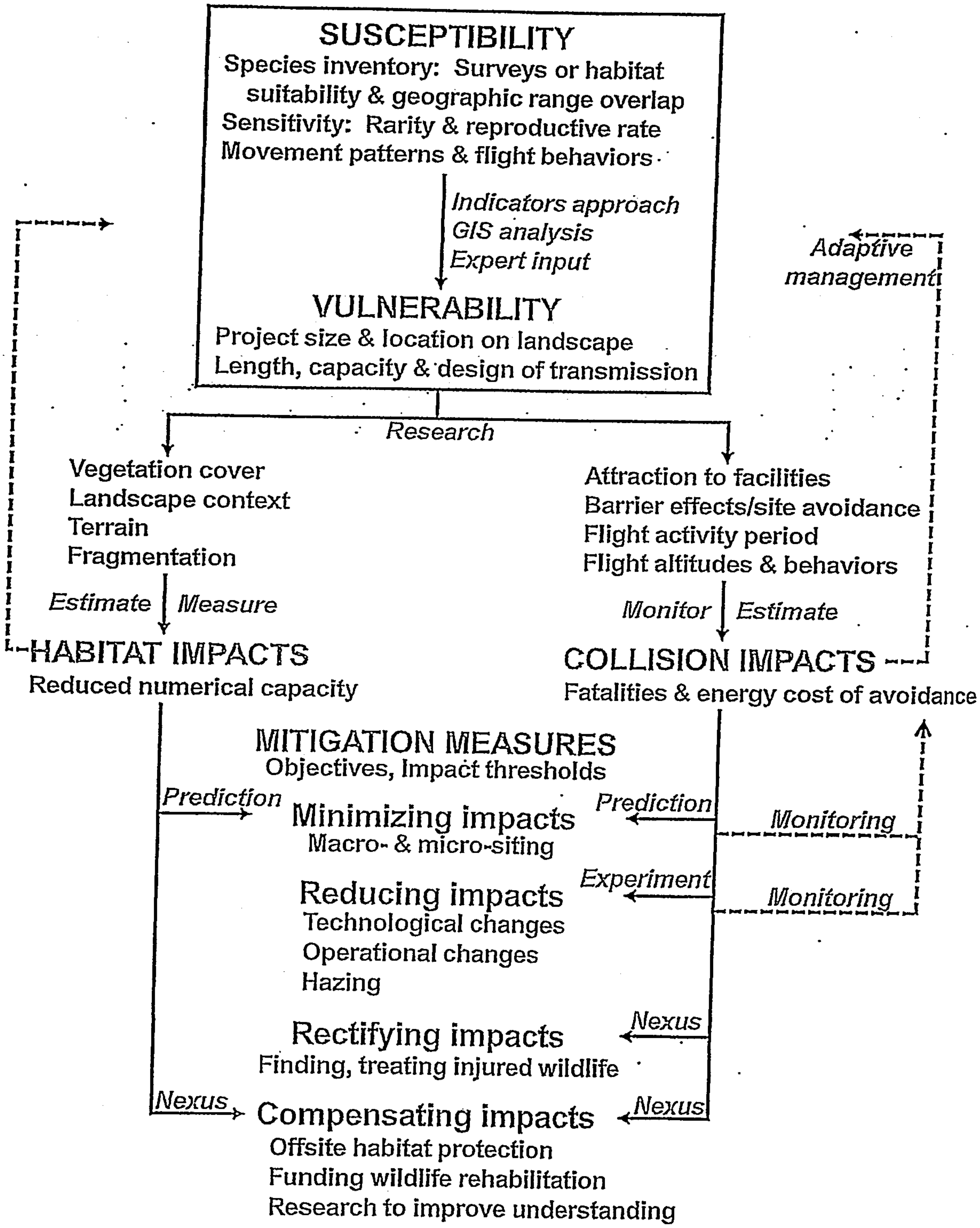


Figure 1. *Framework for directing and prioritizing information needs related to avian impacts caused by industrial solar energy development, where blue italic fonts denote processes used to assess or achieve outcomes, and dashed flow-lines denote feedback contributing to adaptive management.*

Vulnerability refers to the likelihood of impacts once the project is built. No animals are vulnerable to habitat loss or collision impacts when no project is built, but once the project is built each species will be more or less vulnerable to impacts depending the project's size and location on the landscape and on the length and design of its transmission system.

Research is the means to understand how susceptibility and vulnerability translate into impacts caused by habitat loss or collision impacts. Habitat impacts can be measured or estimated in terms of the reduction in numerical or demographic capacity of the affected area due to loss of vegetation cover or usable soil substrate. These losses in capacity can be exacerbated by habitat fragmentation resulting from the habitat losses and loss of connectivity to other habitat patches as other solar projects are developed nearby. Numerical or demographic capacity can also be diminished through habitat degradation caused by the project's light pollution or auto traffic, as examples. The numerical or demographic capacity for a species can also diminish disproportionately due to loss of access to certain terrain features formerly used as concentrated travel routes or by key ecological resources.

Collision impacts refer to avian collisions with PV panels, parabolic mirrors, support structures for panels or mirrors, the zone of solar flux in power tower projects, and with transmission lines tapping the solar project into the grid. These impacts can be influenced by avian attraction to solar facilities, the times of day or night when birds typically fly through the airspace over the project, and flight altitudes and behaviors. Birds can suffer energetic impacts, as well, caused by barrier effects of solar projects. Some bird species might be intimidated by solar projects and will expend extra effort to fly around the projects. Some birds will also expend energy attempting to avoid collisions at the last moment, as often occurs with birds flying toward transmission lines in low light.

Estimating collision impacts typically requires extensive, long-term monitoring, as well as adjustment factors for the proportion of victims not detected during monitoring. (Also noteworthy, monitoring provides the additional benefit of more often and more quickly locating injured wildlife, which then stand an improved chance of being treated and released to the wild.) On the other hand, estimating diminished numerical or demographic capacity requires a quantitative relationship between population size and habitat area. Such a relationship can be derived from home range analysis or comparisons of population density estimates to spatial areas used to derive the estimates (Smallwood 1995, 1998; Smallwood et al. 2007, 2013b). Estimating the loss of numerical capacity caused by a solar project happens once, so monitoring is not an issue.

By quantifying the impacts of solar projects, the basis can be developed for mitigating the impacts. For habitat impacts, only two types of mitigation measure are available, including macro- and micro-siting and compensatory measures. Macro-siting refers to the decision of whether to go forward with a proposed project at a particular place, whereas micro-siting refers to the layout of the solar project considering the impacts that will be caused by covering particular types or patches of vegetation or terrain. A goal would be to develop the means to predict project impacts caused by habitat loss or degradation, in support of macro- or micro-siting to minimize impacts. Compensatory measures involve habitat protections elsewhere. A goal would be to develop the means to quantify the nexus between project impacts and benefits achieved by the mitigation. Adaptive management opportunities will be limited when addressing habitat impacts.

For collision impacts, sufficient monitoring is necessary to understand causal factors and to predict impacts so that appropriate mitigation can be formulated. Understanding collision impacts can contribute to predicting impacts, and those causal factors related to vegetation, terrain and wind can contribute to macro- and micro-siting decisions. On the other hand, impact-reduction measures require experimental design elements to control the variation (Sinclair and DeGeorge 2016). In my experience, lacking experimental design results in endless, unsatisfactory debate over the effectiveness of particular measures. Collision impacts can also be mitigated through compensatory measures, including offsite habitat protection, funding of wildlife rehabilitation facilities, and research leading to improved understanding and minimization and impact-reduction measures. Ideally, compensatory mitigation would be based on a nexus between project impacts and benefits achieved by the mitigation.

Regardless of whether funding wildlife rehabilitation as a compensatory measure, the Plan should include the recommendation that rehabilitation facilities be identified in advance so that injured animals can be transported to the appropriate facilities. An annual fund provided to rehab facilities would also help ensure the continued availability to deal with wildlife emergencies. After performing a survey of rehabilitation facilities (E. Leyvas and K. S. Smallwood, unpublished data), a reasonable fee per killed or injured raptor would be $3,000 per bird. A reasonable fee for non-raptors might be half this cost, but Leyvas and I lack the information yet to make an empirically sound estimate.

Vague Terminology

Question 1b in Table 1-3 reads, "*Landscape-level thresholds to cumulative impacts (i.e., how much human development can a landscape sustain?)*" I don't think anybody can answer this question because it is too vague and lacks scientific foundation. Does the question go to a particular species? Or to something else?

The Plan's concern about measuring impacts to guilds seems out of place, such as Question 6a in Table 1-3 and management category 6 in Table 4-1. The basic unit of research and management should be at the species level, as this level will be challenging enough. It is premature to consider impacts at the guild level, which also lacks regulatory bearing.

Emphasis of Impacts Framework

Although the Plan begins by appropriately identifying habitat loss as "the most obvious impact" (last paragraph of section 1.1), most of the rest of the Plan focuses on collision impacts. And when it comes to collisions, very little attention is given to collisions with transmission lines. For example, on page 15 the Plan states, *"The nature and magnitude of impacts on bird populations and communities are generally related to three primary project-specific factors: location, size, and technology (PV vs. CSP)."* Notice that this statement neglected distance of the project from load demand, which bears on the length of transmission lines needed to bring the energy to load demand. The Plan's Table 2-1 lists contact with transmission lines as a fatality cause at solar projects, but only in the form of electrocutions. Transmission lines can cause high fatality rates of birds colliding with the lines, especially where the lines cross wetlands or other areas intensely traveled by birds (Hartman et al. 1992). In the Altamont Pass Wind Resource Area, my colleagues and I have found many birds killed by transmission line collisions, often including up to 20 birds at a time along a single span of transmission line. I have also found a golden eagle killed by a collision with transmission lines, as well as barn owls, great-horned owls, a great blue heron and many other birds. Last year I witnessed a duck fly into distribution lines (it died), and I have found many birds killed by collisions with distribution lines. A fatality search crew I managed during 1985 found hundreds of birds killed by collisions with distribution lines during 5 searches of 6,000 distribution poles and their intervening spans of circuit.

Hypothesized Causal Factors of Collisions

Page 16 of the Plan lists hypothesized causal factors, including the lake effect, glare and unexpected fluctuations of light from panels and mirrors, polarized light caused by PV panels, and lush vegetation near panels where water used to clean the panels has run off. These hypothesized causes are helpful. My only concern is that none of these hypotheses should be believed until they are tested through behavior observations of birds flying over or nearby solar projects or through experimental deployment of treatments that vary the levels of "lake effect," glare, fluctuations of light, or vegetation around panels (Sinclair and DeGeorge 2016). The experience with wind and wildlife has demonstrated that untested hypotheses, and even refuted hypotheses, too often persist for too long, and are inappropriately included as candidate mitigation options in proposed adaptive management plans.

Summary of Existing Avian-Solar Information

I am concerned that Table 3-1 in the Plan is somewhat misleading. The Table lists projects that are under construction, in operation, or decommissioned and whether each project has a monitoring plan and whether fatality data are or were collected. I am concerned that not all of the avian monitoring plans are available to the public, and that not all of the fatality data are available to the public. I tried acquiring fatality data from solar projects over the last few years, and have achieved very little success.

Another concern is what is not summarized in Table 3-1. This table does not include critical information about the usefulness of the fatality monitoring data collected at solar projects. The table should also report the MW that are monitored, the duration of monitoring, the search interval and whether detection trials are being used to adjust fatality rates for the proportion of fatalities not found. Furthermore, the table should report whether the data and reporting is or will be available to the public.

Of particular interest to me was the summary of the monitoring at the Crescent Dunes power tower project, which states that avian fatality data are being collected systematically. This reporting is misleading, however, as the search interval at any given portion of the solar project is 9 months. The circular project was divided into 8 wedges resembling pie slices, and the monthly fatality searches visited each wedge sequentially, returning to the same wedge once every 9 months. A search interval of 9 months is scientifically indefensible because the vast majority of small bird carcasses would have been removed by scavengers or degraded by exposure before the next search, and most of the large carcasses also would not have persisted to the next search. If this is the level of monitoring occurring at other solar projects in Table 3-1, then the table does not credibly summarize avian monitoring activities at solar projects.

Identifying Information and Data Needs

The information needs listed on page 31 of the Plan appeared somewhat disordered and incomplete, and too focused on collision impacts. As pointed out earlier in the Plan, habitat loss is the obvious principal impact. Therefore, a primary data need are the numerical and demographic consequences to species caused by habitat loss and habitat fragmentation. Average home range sizes can be used for this purpose, or average densities measured at proposed solar projects (Table 1) or more sophisticated interpretations of collections of density estimates (Smallwood 1995, 1998, 2001a,b; Smallwood et al. 2007, 2013b).

A simple, crude estimate can be made of the loss of numerical capacity by simply comparing the extent of habitat loss to a typical density estimate. For example, relying on the estimated 18 GW of industrial solar already developed in the USA (page 1 of the Plan) and the 8-10 acres per MW (mean 9 acres/MW) of industrial solar (page 4 of the Plan), the USA has already developed about 162,000 acres (65,559 ha or 565 km^2). Assuming about 30 km^2 per pair of golden eagles, and assuming that all of the developed area was previously golden eagle habitat (for the sake of this example), then this buildout would have displaced, or effectively destroyed, 19 pairs of eagles. The number of eagles displaced would be 38 breeding eagles or about 80 golden eagles including juveniles, subadults and floaters (based on distributions of age classes observed in earlier studies). This analysis was crude, but it provides a ballpark basis for estimating impacts and for arriving at a nexus between impacts and benefits gained from mitigation.

Of course, it is always better to work from actual data. For burrowing owls, protocol-level surveys (CDFW 2012) have been performed at many solar projects proposed for construction in California, the results of some of which appear in Table 1. It just so happens that the best solar project sites correspond with conditions that are favored by burrowing owls – relatively low on generally south-facing slopes. Additionally, 71,000 acres (287.3 km2) of predicted terrestrial impacts from the Desert Renewable Energy Conservation Plan will be in the Imperial Valley (CEC et al. 2014: Table IV.7-46), where the largest aggregation of burrowing owls occurs in California. This level of habitat loss in the Imperial Valley alone will result in the loss of 2,434 pairs of burrowing owls, or 43% of the Imperial Valley population.

A more sophisticated interpretation of density estimates would be to plot numerical estimates against the areas used to make the estimates to identify the spatial scale at which numerical asymptotes are reached (Smallwood 1999, 2001a). These asymptotes identify the spatial areas at which demographic units can be discerned, and they inform of the degree of clustering typical of the species. These density interpretations can then be applied to the spatial areas of habitat loss and fragmentation caused by solar development.

Table 1. Nesting densities of burrowing owls at proposed project sites within the DRECP.

Source	Site	Ha	Pairs	Nest density, pairs/km²
Cornett 2012	Imperial Valley Solar Company 2	64	4	6.25
Ecology and Environment 2012	Hudson Ranch Power II Geothermal Project	99	13	13.13
Ecology and Environment 2012	McDonald Road portion of Hudson Ranch	78	13	16.67
HDR 2011	Mt. Signal	1,711	72	4.21
BLM 2012	Ocotillo Sol	46	5	8.58
Imperial County 2012	Solar Gen II	813	56	5.61
Heritage Environmental Consultants, LLC. 2012a	Campo Verde	1,338	65	4.86
Average				8.47

Population Impacts

The Plan appropriately asks whether solar developments affect populations (question 6b in Table 1-3), and elsewhere (e.g., page 22) the Plan identifies population-level impact estimates as a desired outcome of monitoring or research associated with solar developments. However, I must point out three challenges associated with the Plan's wish for quantifying population-level impacts. One, whereas we would all like to know population-level effects, our wildlife protection laws do not require quantification of population-level effects (although the proposed eagle take rule would establish wind energy harvest quotas, and these would require quantification of population-level effects). Two, populations are difficult to define, and the population concept is relatively vague. Three, one of the early industry-sponsored collaborative meetings focused on population-level effects (RESOLVE 2003), but the attendees concluded that our scientific methods used at wind projects were nowhere near ready for measuring population-level impacts.

Until 1991 the clearest population definition I had found was 'some collection of organisms of the same species occupying a particular space and sharing a suite of attributes representing a unique organizational structure' (Odum 1959, Dasmann 1981). Note that this definition includes a space, which means there's at least a fuzzy boundary about the individuals forming the population. This definition also includes an organizational structure, referring to social organization. A population is more than some collection of individuals that can breed or that do breed; it includes a social history that formed the social bonds of the constituent members of the population at a spatial scale that makes sense for stabilizing a mass density over the long term (Smallwood 2001a). For eagles it is clear that the boundaries are indeed fuzzy, as GPS/GSM telemetered eagles often make flights far from their centers of activity, perhaps as a social reconnaissance or a keeping of contact between the eagle's focal area of activity and other eagle aggregations.

Populations are complex entities expressing a unit of organization over which species interface with the environment. Among allometric variables, animal density scales most strongly with female brain mass among species of mammalian Carnivora, more so than to body mass or basal metabolic rate (Smallwood 2001a).[1] This scaling of density with female brain mass among species also ties female decision-making related to foraging and social factors at spatial scales that optimize rearing of young. This same allometry of density almost certainly exists for birds.

Wildlife ecologists have long struggled with defining aggregations of individuals observed in nature. Various terms for aggregations have been proposed to clarify our scientific discussion related to the distribution and abundance of organisms, such as metapopulation, megapopulation, deme, holon, local population versus population, and many other terms. Due to confusion over the terminology it is not uncommon for wildlife biologists to use the term 'population' in place of any of these other terms. In my research I tried to let the data inform me of natural aggregations and of the spatial areas in which similar-sized aggregations occur (Smallwood 1999, 2001a). I assumed that the aggregations that were repeatedly observed within ranges of spatial areas were meaningful. The next step would be to name the aggregations at the various spatial scales, including candidate names such as deme, population, metapopulation and so on. By allowing nature to inform us of the aggregations and their spatial domains that matter, we can develop one of the bases needed for assessing biological impacts. However, also needed in an impacts assessment are how lost individuals affect organization structure and genetics.

As I mentioned earlier, one of the early wind industry-sponsored meetings addressed the biological significance of wind energy impacts. (Biological significance is often also

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[1] I have not yet investigated the allometry of space use among avian species, but I would assume that the allometry is similar to that of Mammalian carnivores. What I have confirmed is that for individual avian species, the relationship between density and the space used to estimate density mirrors the relationship for Mammalian carnivores (Smallwood 1995, 1998, 2001b, Smallwood et al. 2013b).

expressed as population-level impacts, so both terms will appear in the discussion that follows.) The title of the meeting was "HOW IS BIOLOGICAL SIGNIFICANCE DETERMINED WHEN ASSESSING POSSIBLE IMPACTS OF ONSHORE WIND POWER FACILITIES?" The meeting was held in Washington, DC on 16-17 November 2003, and was attended by many professionals involved with predicting and estimating wind energy impacts as well as mitigating the impacts. A highly regarded risk analyst presented at the meeting, as did a widely respected federal government biologist, followed by much deliberation by meeting participants. Little has changed since the meeting that would have changed the meeting outcome. Also, the meeting outcome reflected unanimous agreement among participants, who were asked to raise hands to show agreement with each of the outcome statements.

One of the invited speakers pointed out, *"Specific factors influencing whether an impact has biological significance include sex and age of the animals, time of year when the impact occurs, whether the population is at or below carrying capacity, rarity of the species, and genetic uniqueness of the individuals."* In other words, determining biological significance is highly nuanced. If the population affected by the loss of an individual is above carrying capacity, then perhaps the loss will have a relatively small impact on the population, but if it is well below carrying capacity or even small enough to jeopardize extirpation, then the loss of this individual might be devastating.

The other invited speaker at the Washington DC meeting warned, *"biological significance is ultimately in the eye of the beholder, depending on management objectives and other factors, and added that detecting population effects for migratory birds is very difficult."* Participants at the meeting concluded that *"Biological significance definitions are usually conceptual and involve subjective analysis."* They also decided *"A biologically significant effect is an effect that could result in an influence on population viability."* But, *"From an ecological perspective, mortality should be reduced as much as possible to keep it at a very low level for wind."* These conclusions acknowledge the difficulty in assessing population-level impacts, pointing out that conclusions about biological significance will ultimately be qualitative and subjective even when quantitative methods are applied. These conclusions also suggest a more practical way forward for the Plan by not only acknowledging the subjective nature of impacts assessments for biological significance (thereby getting that question out of the way), but also by emphasizing the need to focus on minimizing and reducing impacts to the degree possible.

Other conclusions of the meeting included *"The definition* [of biological significance] *should include a statement that defining biological significance for a population may require examination of the region and habitat for a specific species,"* and *"is most useful at a site specific and regional scale."* We added *"Regulators need to adopt a broad definition in general and narrow the definition for species types"*, and *"Avoid a strict definition of biological significance so that views of all stakeholders can be reflected."* In the face of uncertainty over what aggregations qualify as populations and over what the losses of individuals mean to populations, the Plan could more safely set the foundation for assessing impacts by adopting a unit of organization that can be applied universally. To this end, we participants at the Washington DC meeting agreed that regulators need to *"Frame the definition around individual birds killed, because there is no data on the risk being significant at the population level except for a narrow group of populations."*

We participants of the Washington DC meeting suggested regulators *"Use defined criteria for biological significance to evaluate potential sites as to the likelihood of resulting in major impacts as compared to other sites (i.e., avoid areas where important populations of birds migrate, are used as pathways, or are close to threatened species and suitable habitat)."* We suggested that regulators ask the following questions about conclusions of biological significance, *"Significant to what? Within what geographic area? Over what timeframe?"* In other words, the impacts assessment is far better addressed in a rigorous manner prior to the development of a solar project rather than afterwards. The questions that we suggested be asked should be asked at each proposed solar project site, as some projects might be proposed within the geographic bounds of a breeding population and others might be proposed within areas overflown by migrants of the species.

At the Washington DC meeting we suggested *"The definition* [of biological significance] *needs to include a statement about accepting uncertainty,"* and *"Precise population estimates are not required to assess whether an impact is significant."* These recommendations were consistent with the conclusions presented earlier about significance conclusions being subjective. Uncertainty in conclusions of significance can and should also be factored into thresholds of take that are more consistent with the Precautionary Principle in risk assessment.

The Washington DC meeting attendees agreed that there are data gaps inhibiting determinations of biological significance, including *"Lack of information on factors of biological significance, including cumulative Impacts..."* We concluded that wind projects (and this recommendation should apply to solar projects, as well) should be designed to help collect useful post-construction data, and that long-term data are needed, including on population dynamics. Had our recommendations been heeded since 2003, data collected since that meeting could have hastened mitigation solutions. Much of the data collected post-construction were collected using various methods, some of which were inappropriate and ineffective at informing biological significance. Also, much of the data collected since 2003 have been kept confidential and have therefore been of no use to most scientists interested in understanding fatalities at wind projects and how to minimize or reduce the impacts.

We ended the Washington DC meeting by asking *"Can we even measure biological significance?" How necessary is it to quantify impacts? Biological significance is synonymous with population viability, but this involves having to determine too many input values (e.g., timeframe, measurements, population, habitat, population stability). Biological significance can only be measured after development is in place, which then makes the issue moot."* The key conclusions of the meeting were the following:

- *"Views of biological significance are subjective.*
- *Biological significant impacts are difficult to determine.*
- *Defining and developing data for biological significance is challenging.*
- *Questions exist about what a definition will offer for various interest groups.*
- *Biological significance criteria can be used when choosing a site, but much of the data are not available.*
- *Concerns exist about addressing cumulative impacts, but this has not been thoroughly discussed."*

I recommend that the Plan be revised with these meeting outcomes in mind. The Plan can be more realistic in its goals and objectives as they relate to estimating impacts, and it can be more relevant to wildlife protection laws.

Pre-construction Studies and Project Siting

Although baseline avian activity is identified as a high-priority information need in Table 5-1, the Plan barely addresses this need. The Plan's focus on migration routes pertain mostly to the lake effect as a possible causal factor associated with collisions, but it bears little on habitat loss and fragmentation and the collisions of resident birds with solar facilities. Rather than focusing on migration routes, baseline studies should prioritize identifying which species occur at the site and what they are doing there. The emphasis of these studies should be on identifying habitat impacts, or the degree to which the numerical and demographic capacity of the site will be diminished by construction of the project. Secondarily, the species inventory should be examined to identify species with known records of collisions with PV panels, mirrors, zones of solar flux and transmission lines.

Standard point counts would be useful for baseline studies at solar projects, but more consideration needs to be given to the maximum survey radius, survey station coverage across the project area, and total survey effort, all of which will influence the number of species detected and the robustness of counts. Also, now that thermal imaging cameras are widely available, nocturnal surveys would help identify species and activity patterns not seen during daylight hours.

Some species require specialized surveys, including burrowing owls (CDFG 2012). Point counts are unsuitable for burrowing owls, and the CDFG (2012) survey guidelines could be improved by adding nocturnal surveys. Burrowing owls are highly active at night.

Whether pre-construction studies involve onsite surveys or not, the information should inform macro-siting and micro-siting, as well as the basis for compensatory mitigation. As indicated in the flowchart in Figure 1, ecological indicators can help identify those parts of the landscape that are most sensitive to land conversions (e.g., Smallwood et al. 1998). This type of indicators approach can be enhanced greatly by performing onsite surveys, such as were used to guide the repowering of the Altamont Pass Wind Resource Area (Smallwood and Neher 2016, Smallwood et al. 2016). Onsite surveys can benefit from relative abundance counts so long as the spatial grain of the surveys and survey effort comport with the data needs at the project site (Smallwood 2016c). Too many wind projects were preceded by baseline studies that were so cursory in effort that they likely failed to document the presence of most of the bird species using the sites. If surveys are to be performed for siting and for founding compensatory mitigation, then sufficient commitment is needed to collect the appropriate data. Here I must also point out that behavior data can be more useful that simple counts.

Post-construction Behavior Surveys

Understanding causal factors of collision fatalities requires substantial observational efforts, as well as drawing inferences between locations of fatality finds and project facilities. In my experience, there is nothing more informative that actually witnessing collisions and near-misses. The circumstances of collisions and near-misses, once systematically reported, reveal patterns, understanding and solutions. Having spent hundreds of hours performing behavior surveys amongst wind turbines (e.g., Smallwood et al. 2009), I have accumulated hundreds of records of near-misses and a few collisions, including near-misses with transmission lines. Having spent hundreds of hours performing nocturnal surveys using a thermal camera, I have witnessed hundreds of near-misses of bats and birds, including more collisions than I have seen during daylight hours.

It might turn out that the lake effect is more nocturnal than it is diurnal. While I perform thermal imaging surveys at night, I often see herons, cranes and waterfowl flying at night. It also might turn out that mirrors cause more collisions at night than during the day. Watching birds at night will provide opportunities to observe how often birds nearly collide with solar facilities at night, and how they avoid collisions.

Fatality Monitoring Methodology for Solar Projects

The Plan says little about monitoring methodology, other than to cite a plan prepared by Huso et al. (2016). I have yet to review Huso et al. (2016), but I will suggest that much of the challenge in fatality monitoring at wind projects has been with field methodology (Smallwood 2007, Smallwood et al. 2013a). The fatality search interval is of principle concern – more so than which estimator is used. Another major concern is the type of detection trials that are implemented, and whether monitoring is realistically simulating detection probabilities by placing fresh carcasses of the appropriate species at appropriate locations and time intervals. Are carcasses of project fatalities and detection trials being left in place? Appendix 1 [not included with this attachment] summarizes integrated detection trials, which came into use in the Altamont Pass Wind Resource Area four years ago and proved much more useful than the conventional detection trials that were performed separately for estimating searcher detection error and carcass persistence (Smallwood 2016, Brown et al. 2016). The approach summarized in Appendix 1 facilitates sound field methodology by requiring greater care in choosing which species to place in detection trials, and in ensuring the condition of the carcasses is suitable and the trial placements are realistic in their simulation of the fatality detection probabilities faced by searchers. Furthermore, it provides superior data for use in evidence of absence software that is cited on page 25 of the Plan.

REFERENCES CITED

BLM (Bureau of Land Management). 2012. El Centro Office Draft Environmental Impact Statement / Draft CDCA Plan Amendment Ocotillo Sol Project. U.S. Department of the Interior, BLM/CA/ES-2012-009+1793, DES 20-12, DOI-BLM-CA-D000-2012-0005-EIS. El Centro, California.

Brown, K., K. S. Smallwood, J. Szewczak, and B. Karas. 2016. Final 2012-2015 Report Avian and Bat Monitoring Project Vasco Winds, LLC. Prepared for NextEra Energy Resources, Livermore, California.

California Energy Commission and California Department of Fish and Game (CEC & CDFG). 2007. California Guidelines for Reducing Impacts to Birds and Bats from Wind Energy Development. Draft Staff Report CEC-700-2007-008-SD, Sacramento, California.

CDFG (California Department of Fish and Game). 2012. Staff Report on Burrowing Owl Mitigation. Sacramento, California.

Cornett, J. W. 2012. Biological Resources Assessment, Imperial Valley Solar Company 2, LLC, located within Section 34, Range 14 East, Township 10 South, Parcels 003-240-009 and 003-240-029, Imperial County, California. Unpublished report to MSA Consulting, Inc., Rancho Mirage, California.

Dasmann, R.F. 1981. Wildlife biology. 2nd ed. John Wiley and Sons, New York.

De Lucas, M., G. F. E. Janss, D. B. Whitfield, and M. Ferrer. 2008. Collision fatality of raptors in wind farms does not depend on raptor abundance. J. Applied Ecology 45:1695-1703.

Ecology and Environment, Inc. 2012. Hudson Ranch Power II CUP #G10-0002/Simbol II CUP #12-0005 Draft Environmental Impact Report, Volume I. County of Imperial, Department of Planning and Development Services, El Centro, CA.

Ferrer, M., M. de Lucas, G. F. E. Janss, E. Casado, A. R. Munoz, M. J. Bechard, and C. P. Calabuig. 2012. Weak relationship between risk assessment studies and recorded mortality in wind farms. J. Applied Ecology 49:38–46.

Hartman, P. A., S. Byrne, and M. F. Dedon. 1992. Bird mortality in relation to the Mare Island 115-kV transmission line: Final report, 1988-1991. PG&E Report Number 443-91.3.

HDR (Engineering, Inc.) 2011. Draft Environmental Impact Report Mount Signal and Calexico Solar Farm Projects, Imperial County, California. County of Imperial, El Centro, California.

Imperial County. 2012. Draft Environmental Impact Report: Solar Gen 2 Solar Array Power Project. Imperial County Planning & Development Services Department, El Centro, California.

Odum, E.P. 1959. Fundamentals of ecology. W.B. Saunders Co., Philadelphia.

RESOLVE. 2003. How is biological significance determined when assessing possible impacts of onshore wind power facilities? Meeting of the National Wind Coordinating Committee, Washington, D.C.

Sinclair, K. and E. DeGeorge. 2016. Framework for Testing the Effectiveness of Bat and Eagle Impact-Reduction Strategies at Wind Energy Projects. S. Smallwood, M. Schirmacher, and M. Morrison, eds., Technical Report NREL/TP-5000-65624, National Renewable Energy Laboratory, Golden, Colorado.

Smallwood, K.S. 1995. Scaling Swainson's hawk population density for assessing habitat-use across an agricultural landscape. J. Raptor Research 29:172-178.

Smallwood, K.S. 1998. On the evidence needed for listing northern goshawks (*Accipter gentilis*) under the Endangered Species Act: a reply to Kennedy. J. Raptor Research 32:323-329.

Smallwood, K.S. 1999. Scale domains of abundance among species of Mammalian Carnivora. Environmental Conservation 26:102-111.

Smallwood, K. S. 2001a. Linking habitat restoration to meaningful units of animal demography. Restoration Ecology 9:253-261.

Smallwood, K.S. 2001b. The allometry of density within the space used by populations of Mammalian Carnivores. Canadian Journal of Zoology 79:1634-1640.

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

Smallwood, K. S., C. G. Thelander, M. L. Morrison, and L. M. Rugge. 2007. Burrowing owl mortality in the Altamont Pass Wind Resource Area. Journal of Wildlife Management 71:1513-1524.

Smallwood, K.S. 2013. Comparing bird and bat fatality-rate estimates among North American wind-energy projects. Wildlife Society Bulletin 37: 19-33.

Smallwood, K. S. 2016a. Bird and Bat Impacts and Behaviors at Old Wind Turbines at Forebay, Altamont Pass Wind Resource Area. Report CEC-500-2016-XXX, California Energy Commission Public Interest Energy Research program, Sacramento, California. In press.

Smallwood, K. S. 2016b. Comments on proposed rule for incidental eagle take. Comments submitted to US Fish and Wildlife Service.

Smallwood, K.S. 2016c. Monitoring birds. M. Perrow, Ed., Wildlife and Wind Farms: conflicts and solutions. Pelagic Publishing. In press

Smallwood, K. S., and L. Neher. 2016. Siting Wind Turbines to Minimize Raptor Collisions at Sand Hill Repowering Project, Altamont Pass Wind Resource Area. Report to Ogin, Inc., Waltham, Massachusetts.

Smallwood, K. S. and C. Thelander. 2004. Developing methods to reduce bird mortality in the Altamont Pass Wind Resource Area. Final Report to the California Energy Commission, Public Interest Energy Research – Environmental Area, Contract No. 500-01-019. Sacramento, California.

Smallwood, K. S., L. Rugge, and M. L. Morrison. 2009. Influence of Behavior on Bird Mortality in Wind Energy Developments: The Altamont Pass Wind Resource Area, California. Journal of Wildlife Management 73:1082-1098.

Smallwood, K.S., B. Wilcox, R. Leidy, and K. Yarris. 1998. Indicators assessment for Habitat Conservation Plan of Yolo County, California, USA. Environmental Management 22: 947-958.

Smallwood, K. S., D. A. Bell, B. Karas, and S. A. Snyder. 2013a. Response to Huso and Erickson Comments on Novel Scavenger Removal Trials. Journal of Wildlife Management 77: 216-225.

Smallwood, K. S., L. Neher, J. Mount, and R. C. E. Culver. 2013b. Nesting Burrowing Owl Abundance in the Altamont Pass Wind Resource Area, California. Wildlife Society Bulletin: 37:9 pp.

Smallwood, K.S., L. Neher, and D.A. Bell. 2016. Siting to Minimize Raptor Collisions: an example from the Repowering Altamont Pass Wind Resource Area. M. Perrow, Ed., Wildlife and Wind Farms: conflicts and solutions. Pelagic Publishing. In press

Taylor, R.A.J., and L.R. Taylor. 1979. A Behavioral Model for the Evolution of Spatial Dynamics. Pages 1-28 <u>in</u> R.M. Anderson, B.D. Turner, and L.R. Taylor (editors). *Population Dynamics*. Blackwell Scientific Publications, Oxford.

ATTACHMENT

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Original Article

Nesting Burrowing Owl Density and Abundance in the Altamont Pass Wind Resource Area, California

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ABSTRACT About 600 burrowing owls (*Athene cunicularia*) are killed annually at wind turbines in the Altamont Pass Wind Resource Area (APWRA), California, USA. Understanding the biological significance of this toll requires population data, including an estimate of the number of breeding pairs in the APWRA. An empirical model of breeding density predicted 35–75 pairs within the 16,760 ha of the APWRA, but that prediction proved low in spring 2011, when we detected 78 breeding pairs of burrowing owls in 46 randomly selected plots totaling 2,563 ha. We estimated that 3.201 breeding pairs/km^2 occupied the 46 plots we surveyed in April–May. Extrapolating this density to the area of the APWRA led to an estimate of 537 pairs (90% CI = 320–753 pairs) across the APWRA, or 10× the model prediction. Counts of chicks emerged from burrows averaged 1.2 chicks/nest on the 46 plots, but these counts were minimum numbers. We estimated the APWRA supported at least 1,836 burrowing owls in 2011 (90% CI = 1,082–2,590), indicating the local population could conceivably be the sole source of fatalities attributed to wind turbines in the APWRA. Measures to conserve burrowing owls in the APWRA could include ground squirrel conservation and repowering to safer wind turbine models and careful siting to avoid areas densely populated with burrowing owls. More generally, management decisions based on comparisons of breeding-pair density should also consider whether the density estimates were made in habitat fragments, sites selected for known high density, or in randomly or systematically selected plots sampling large areas, because the type of study area strongly influences the density estimate.

KEY WORDS *Athene cunicularia*, burrowing owl, density, population size, productivity, wind turbine.

Wind turbines in the 16,760-ha, 580-MW, Altamont Pass Wind Resource Area (APWRA), California, USA, have been implicated in the deaths of hundreds of burrowing owls (*Athene cunicularia*) per year. Assuming that burrowing owl carcasses found at wind turbines were caused by collisions with wind turbines, estimated fatality rates have been 99–380/year (Smallwood et al. 2007) and 241–1,475/year (Smallwood and Karas 2009) during 1998–2002, 736–1,438/year during 2005–2007 (Smallwood and Karas 2009), and 474–960/year during 2005–2009 (Smallwood 2013). The estimates have varied greatly because of a difference in estimation methods applied to data collected during 1998–2002, and because of inter-annual variation in fatalities. The estimates could change as methods improve to estimate the undetected proportion of fatalities (Smallwood et al. 2010, 2013), but the true number of fatalities was probably approximately 600/year between 1998 and 2011 (Smallwood 2013). Causes of fatalities have been hypothesized to be collisions with wind turbines and predation that might be facilitated by aerial predators' use of wind turbines and their towers as hides. Because burrowing owls have been declining in California (DeSante et al. 2007, Shuford and Gardali 2008), the biological significance of the APWRA's annual toll on the species is of concern. To assess the biological significance of the APWRA's annual toll on burrowing owls, an estimate of population size is essential.

In lieu of surveys to estimate population size in the AWPRA, Smallwood et al. (2007) used published estimates from across the species' range to predict the APWRA's burrowing owl population. Projected to the APWRA's area, Smallwood et al.'s (2007) model of breeding-pair density regressed on study area size predicted only 35–75 nesting pairs in the APWRA. Using published estimates of the annual number of chicks produced per pair, Smallwood et al. (2007) concluded that the number of burrowing owls killed by wind turbines might exceed the number of owls residing in the APWRA, but they also suspected that their predictive model underestimated burrowing owl abundance because most studies of burrowing owls were performed where density was probably known *a priori* to be high. This

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hypothesis was later supported by a count of 21–25 breeding pairs on only 3.3% of the APWRA's land area in 2006–2007 (Smallwood et al. 2009), suggesting that many more burrowing owls breed in the APWRA than predicted (Smallwood et al. 2007).

The predictive model of Smallwood et al. (2007) was based on the available estimates of breeding-pair density, but new density estimates have been published since then. Whereas most of the density estimates in Smallwood et al. (2007) were based on census of known high-density clusters of owls, 4 recent estimates were based on randomly selected plots intended to represent much larger geographic areas than were previously studied. The latter approach likely generated breeding-pair density estimates that will lead to a substantial revision of Smallwood et al.'s (2007) predictive model. Random plot selection was also the approach we decided to use to estimate breeding-pair density in the APWRA. Our objectives were to 1) estimate the size and distribution of the breeding population of burrowing owls in the APWRA, and 2) compare the APWRA's burrowing owl breeding-pair density to densities reported at other study sites.

STUDY AREA

During our study, the APWRA (Fig. 1) included >4,000 wind turbines of various sizes, designs, tower supports, and ownerships on about 16,760 ha of hilly terrain in eastern Alameda and Contra Costa Counties, California. The APWRA's permitted installed capacity was 580 MW. Most of the wind turbines were arranged in rows along ridge crests and ridgelines, and since 2008 most were shut down November through January to mitigate the impacts of collisions with raptors, including burrowing owls. The study area ranged from 78 m to 470 m above sea level, intersected by ephemeral streams and including stock ponds, seasonal ponds, and marshes. Most ridges were oriented northwest–southeast, increasing in size westward. Non-native annual grassland dominated the vegetation cover, including soft chess (*Bromus hordeaceus*), rip-gut brome (*B. diandrus*), foxtail barley (*Hordeum murinum*), Italian rye grass (*Lolium multiflorum*), and wild oats (*Avena fatua*). Common forbs included black mustard (*Brassica nigra*), fiddle-neck (*Amsinckia menziesii*), chick lupine (*Lupinus microcarpus*), and bush lupine (*L. albifrons*). The entire area included many wind turbine access roads and ranch roads. Landowners grazed livestock and leased land to wind companies.

METHODS

Sampling Plots

Our sampling goal was to count all burrowing owl breeding pairs within randomly selected plots that represented environmental conditions where wind turbines also occurred in the APWRA (i.e., where collision risk was greatest). Rather than overlaying a grid of candidate plots onto a map

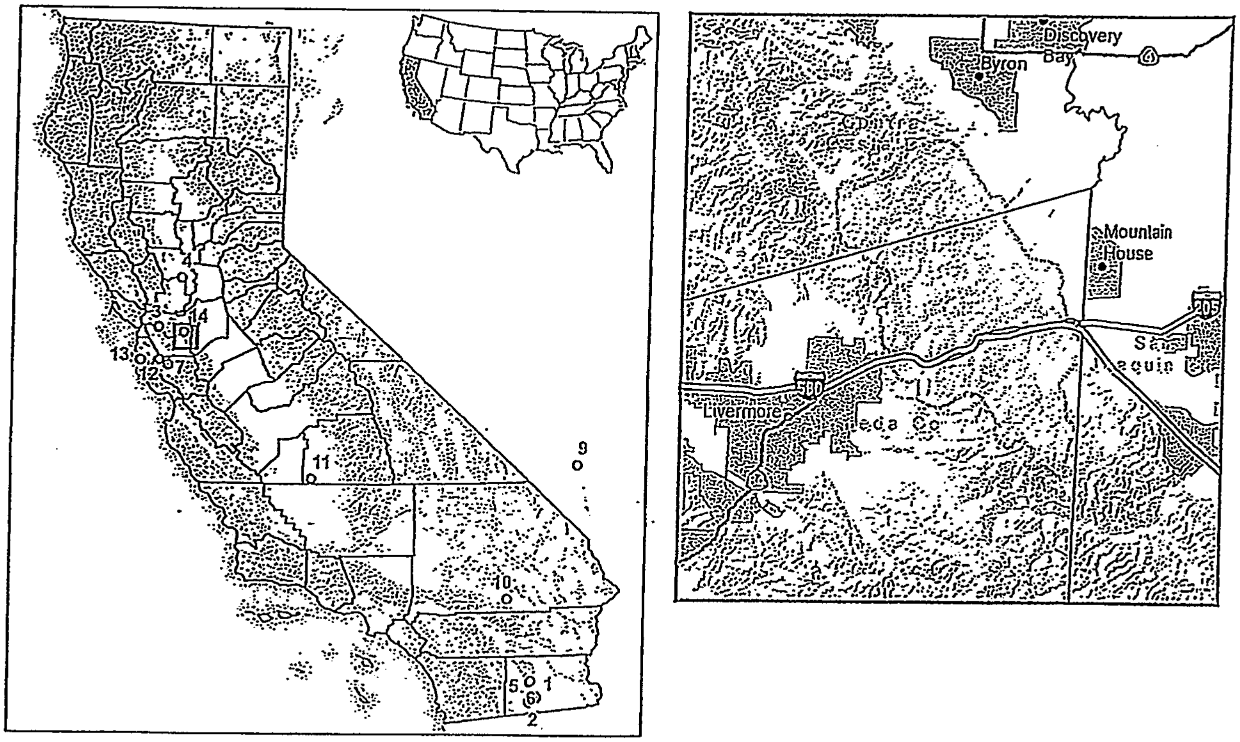

Figure 1. The Altamont Pass Wind Resource Area (APWRA; blue box in left panel, expanded in right panel) forms a low-mountain pass between the Livermore Valley and the Great Central Valley, California, USA. Burrowing owl nesting density has been estimated at other sites in the region, where location numbers referenced 1 and 2 = Coulombe (1971), 3 = Thomsen (1971), 4 = Johnson (1997), 5 = DeSante et al. (2007), 6 = Rosenberg and Haley (2004), 7 = Barclay (2007), 9 and 10 = Crowe and Longshore (2010), 11 = Koshear et al. (2007), 12 and 13 = Trulio and Chromczack (2007), and 14 = Smallwood et al. (2009). Random plot selections were made at sites 5, 9, 10, and in the APWRA (this study).

of the APWRA, we relied on natural features of the landscape to serve as boundaries of all candidate plots. As a first step toward this goal, we used Geographic Information Systems (GIS) to delineate the APWRA's watershed and sub-watershed boundaries from a 10-foot-cell size (3-m-cell size) digital-elevation model, which was derived from a 2-foot-cell size (0.6-m-cell size) digital-elevation model produced from LIDAR data and ARC TIN software by Mapcon Mapping, Inc. (Salt Lake City, UT) during 2007–2008, and from 2-foot contour-interval vector GIS data from Contra Costa County and produced by the same contractor. We divided sub-watershed polygons by GIS line features representing prominent valley bottoms (i.e., streambeds). The resulting polygons represented slopes bounded by ridge crests and ridge lines and that drained rainfall into a reach of stream between stream intersection nodes. We intersected slope polygons with wind turbine locations to arrive at the 952 slope polygons ($\bar{x} = 11.3$ ha, range = 1–90 ha, sum = 10,803 ha), which we then divided into 19 areas of similar geography including wind turbines of common ownership and model (Fig. 2). We referred to these 19 groups of slope polygons as "turbine field polygons," which we used for stratified random sampling to locate our sampling plots. The 19 turbine-field polygons varied in size, so we established 3 plots within 10 turbine field polygons >500 ha, 2 plots in 7 moderate-sized turbine field polygons, and 1 plot in the 2 smallest turbine field polygons.

Sampling plots needed to be larger than individual slope polygons, so we built plots from an initial random selection of slope polygons in each turbine field polygon. We used a stratified random number generator in Statistica 10.0 (StatSoft, Inc., Tulsa, OK) to select slope polygons that

Figure 2. Nineteen wind-turbine field groups, represented by colors other than gray, composed 10,803 ha (64.5%) of the 16,760-ha Altamont Pass Wind Resource Area (CA, USA) bounded by the blue line. Faint lines within turbine field groups represented boundaries of 952 slope polygons.

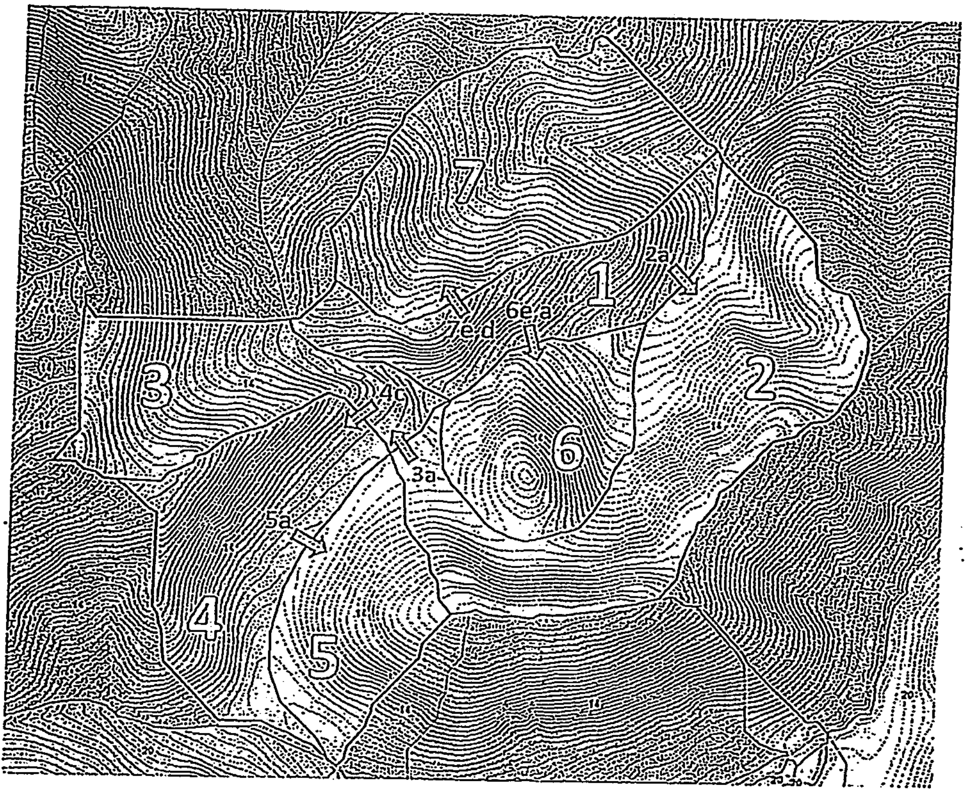

Figure 3. An example of a sampling-plot construction from a randomly selected slope polygon (1) in turbine field 14 of the 16,760-ha Altamont Pass Wind Resource Area (CA, USA), following our decision rules for appending additional slope polygons until the plot was ≥40 ha in size. In addition to selecting slope polygons within the boundary of the turbine-field polygon, decision rules were to (a) cross the nearest prominent valley bottom line, and attempt to do so as priority in steps (c) and (d); (b) attempt to add adjacent downstream slope polygon, which was not possible in this case; (c) append the next adjacent polygon on the downslope edge and along the same prominent valley bottom line; (d) append the next adjacent polygon on the upslope edge and along the same prominent valley bottom line; and (e) return to the randomly selected polygon to restart the decision rules when the first application of the rules runs into a boundary or loses the common prominent valley bottom line.

would serve as starting points for building plots. From each of these initially selected slope polygons, we built a sampling plot by using a rule system to append adjacent slope polygons until the total area of each plot was 40–100 ha, and that favored the selection of slopes facing each other across a shared major streambed to equalize the incidence of aspect (i.e., slope orientation) within each plot (Fig. 3). The first rule was that all appended slope polygons must have been located within the turbine field boundary, and the second rule was that no slope polygons could be included in >1 sampling plot. If adding an adjacent slope polygon would extend the survey plot across a prominent valley bottom line, then that polygon would be selected next, else the adjacent slope polygon nearest a prominent valley bottom line would be added next. Finally, we would add the next slope polygon that presented an opportunity to cross a prominent valley bottom line with the following slope polygon addition, else we added a slope polygon that was both adjacent to the starting polygon and closest to the nearest prominent valley bottom line.

Breeding-Pair Surveys

From 11 April to 29 June 2011, we searched for burrowing owl nest sites within 46 sampling plots totaling 2,560 ha. We randomized the sequence of plots we searched, though we adjusted the sequence to accommodate property access to 8 plots held by one property owner who could not grant access until May. During July 2011, we extended our surveys to another 1,176 ha around plots where we had recorded the highest densities of breeding pairs. The extended areas connected plots and formed larger geographic areas from which hypotheses about burrowing owl distribution and abundance could be tested, but they were not used to estimate population size in the APWRA. During July we returned to most of the nest burrows we mapped in April through June, and we counted chicks that had emerged from the burrows.

To detect burrowing owl breeding pairs, we generally adopted the methods of Restani et al. (2001). We surveyed for burrowing owls using 7× and 10× to 15× binoculars from inside a parked vehicle at multiple roadside vantage points within plots. We used vehicles as blinds because burrowing owls are less wary of stationary vehicles than they are of ambulating people, but we walked over areas not readily visible from roadside vantage points and during early spring when tall grass obscured owls. Using a combination of vantage surveys and walkover surveys, we surveyed for burrowing owls in each plot until we felt that we had detected all breeding pairs. Evidence of nesting included the long-term sun exposure. Evidence also included alarm-calling, short flights around the burrow, and presence of an adult female. Evidence at the burrow included decorations, such as decapitated lizards and mice, broken cattle dung, sticks, and other items. Evidence also included abundant shed feathers, whitewash, and pellets. Later in the season, chicks emerging from the burrow confirmed many of the burrows as nesting sites. Through June, we searched 12.9 person-hr/plot, including 14 person-hr/plot at the 15 plots with chicks. Adding visits in July, we estimate that we searched 18–19 person-hr/plot during the nesting season.

We mapped nest burrows using a Trimble Geo-XT (Trimble Navigation, Ltd., Sunnyvale, CA) global positioning system accurate to within 1 m. Global positioning system data also included date and time, vegetation cover, attributes of the burrow, and diagnostics used to classify the burrow as a nesting burrow, satellite burrow, or refuge burrow. Global positioning system data were converted to GIS shapefiles for analysis.

We counted breeding pairs at mapped nest burrows in each plot, and we regarded these counts as minimum numbers of pairs, even though we felt we had detected all pairs. We used the mean and standard error (SE) of counted pairs to estimate the size of the breeding population in the APWRA. Although our sampling basis was composed of the 952 slope polygons intersecting wind turbine locations on the encompassing 10,803 ha, we extrapolated the estimate of abundance to the 16,760 ha of the APWRA because vegetation cover and topography did not vary throughout the area. We multiplied our estimate of breeding-pair density by the area of the APWRA to arrive at an abundance estimate.

Emerging Chicks Per Nest

To facilitate our estimate of total population size, we conducted multiple re-visits to burrows occupied by pairs that had exhibited sign of breeding behavior during our breeding-pair surveys. We used 7× and 10× to 15× binoculars from inside a vehicle to count chicks. We generally followed the protocol advocated by Gorman et al. (2003), which included five 30-min observation sessions of nest sites, although our visits were sometimes shorter and sometimes longer than 30 min. Counts of emerged chicks correlated ($r = 0.82$) with known numbers of chicks in 21 nest boxes at 3 study sites in California (Gorman et al. 2003), but we acknowledge that our counts were likely low at some nests because not all chicks are always visible above ground. To assess whether our survey effort influenced our counts of emerging chicks, we performed a correlation test between number of survey visits and chicks/nest.

Comparing Breeding-Pair Densities Among Studies

To revise the predictive model of Smallwood et al. (2007), we compared our breeding-pair density estimates with other density estimates. We obtained burrowing owl density estimates and study area sizes that were used to estimate the density estimates from Coulombe (1971), Thomsen (1971), Martin (1973), Butts (1976), Gleason and Johnson (1985), Haug and Oliphant (1990), Rodriguez-Estrella and Ortega-Rubio (1993), Trulio (1993), Leptich (1994), Desmond and Savidge (1996), Johnson (1997), Botelho and Arrowood (1998), Wiley (1998), Restani et al. (2001), Shyry et al. (2001), Rosenberg and Haley (2004), Teaschner (2005), Barclay (2007), Koshear et al. (2007), Crowe and Longshore (2010), Trulio and Chromczack (2007), DeSante et al. (2007), and Smallwood et al. (2009). From DeSante et al. (2007), we used only the sampling plots selected randomly, and not those selected for known high density. Following the methods of Smallwood (1995) and Smallwood and Schonewald (1996), we regressed $\log_{10}$ density on $\log_{10}$ study-area size using linear regression analysis. We relied on the root mean square error (RMSE) in the regression model to estimate the uncertainty range applied to the estimate of breeding-pair density to be applied to the spatial area of the APWRA. The estimate of burrowing owl breeding-pair density for the APWRA was multiplied by the area of the APWRA to arrive at an abundance prediction.

RESULTS

Breeding-Pair Density

We searched 46 plots (2,563 ha) an average of 3.3 times each, but we searched the 15 plots with chicks 5.3 times each. We found 78 pairs of breeding burrowing owls from 11 April to 29 June. In July, we found another 7 nest sites, but these additional nest sites might have been shifts in locations of nesting pairs recorded earlier. In the extended survey areas (1,176 ha) in July, we found another 118 breeding pairs. We found another 37 breeding pairs incidentally between plots and extended areas while driving between plots. Minimally, we found 233 breeding pairs of burrowing owls in the APWRA.

Based on 78 pairs of burrowing owls, the breeding density averaged 3.201 pairs/km^2 (SE = 0.786) among 46 plots totaling 2,563 ha. Expanding the mean density to the estimated 167 km^2 composing the APWRA, we estimated 537 pairs of burrowing owls in the APWRA (90% CI = 320–753) during spring 2011. Of 120 nest burrows recorded from 11 April to 20 June, including those found in extended areas, we found 50 (41.7%) vacant when we revisited them in July.

Of the 46 plots searched, we detected 0 burrowing owl pairs in 24 (52%) plots, low densities (1.7–4.2 pairs/km^2) in 10 plots, moderate densities (5.0–7.5 pairs/km^2) in 7 plots, high densities (10.0–13.6 pairs/km^2) in 3 plots, and very high densities (>15 pairs/km^2) in another 2 plots (Fig. 4). Breeding-pair density was also high in extended search areas around high-density sampling plots and around plots in the north-central aspect of the APWRA (Fig. 4). Breeding-pair densities tended to be higher toward the northwest–southeast axis of the APWRA, and lower along the outer edges of the APWRA. In our study, breeding-pair density did not change with increasing plot size ($r^2 = 0.00$, RMSE = 0.45, $F = 0.19$, df = 1.45, $P > 0.05$).

Comparing Breeding-Pair Density

Relying on the sampled areas in published reports as the basis for predicting breeding-pair density

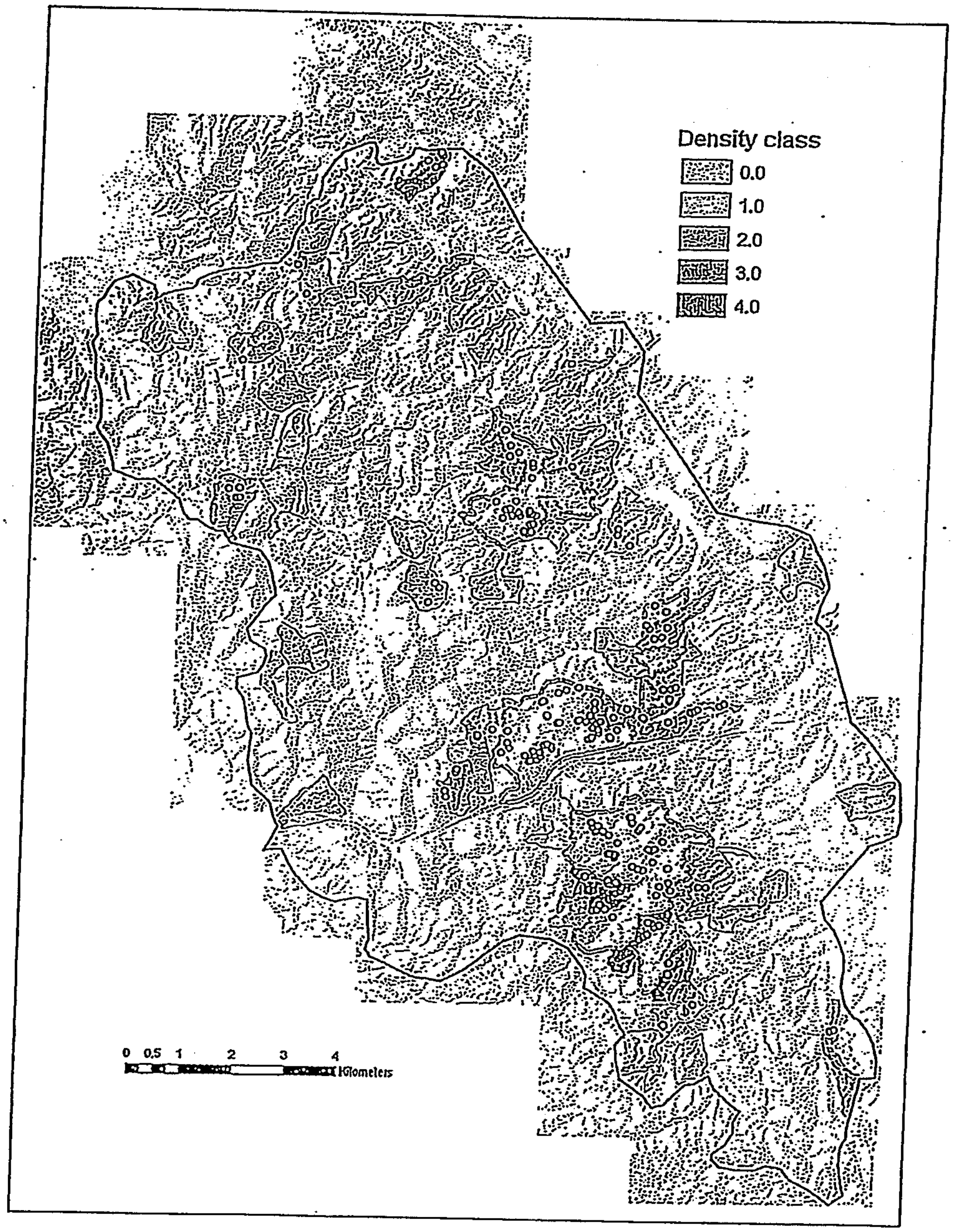

Figure 4. Distribution of nesting burrowing owls in 2011 in the Altamont Pass Wind Resource Area, Alameda and Contra Costa Counties, California, USA. Nesting density in sampling plots was colored yellow for 0 nests/km^2, beige for 1.7–4.2 nests/km^2, orange for 5.0–7.5 nests/km^2, light red for 10.0–13.6 nests/km^2, and dark red for >15 nests/km^2. Dark green-filled circles identified burrowing owl nest locations, including within extended search areas between sampling plots (purple borders).

revised the Smallwood et al. (2007) model (Fig. 5). Five of the new density estimates since Smallwood et al. (2007) were extrapolated to larger sampled areas, including from 46 random plots covering 15.3% of the APWRA, California (this study), 6 random plots covering 3.3% of the Imperial Valley, California, and 9 non-random plots covering 4.9% of the Imperial Valley (DeSante et al. 2007); and from random strip transects covering 11% of Lake Mead National Recreation Area, California, and 5.1% of Marine Corp Air Ground Combat Center near Twentynine Palms, California (Crowe and Longshore 2010). Holding out our new sampling result from the APWRA, the revised model was $\log_{10}(Y) = 0.8921 - 0.6762 \times \log_{10}(X)$, where Y was breeding pairs/km^2 and X was the size of the study area, km^2 ($r^2 = 0.77$, RMSE $= 0.24$, $P < 0.0001$). The revised model of burrowing owl density predicted 0.245 breeding pairs/km^2, which, when extrapolated to 167 km^2, predicted 41 breeding pairs in the APWRA, which was only 7.6% of our point estimate based on field surveys.

Emerging Chicks Per Nest

Among the 286 burrows we recorded during the nesting season, we observed 0 chicks at 48.6% of them, and 7 chicks at one (0.35% of all burrows) and 8 at another (Fig. 6). We counted 386 chicks, including 96 chicks among 78 pairs on sampling plots, 239 chicks among 118 pairs in extended

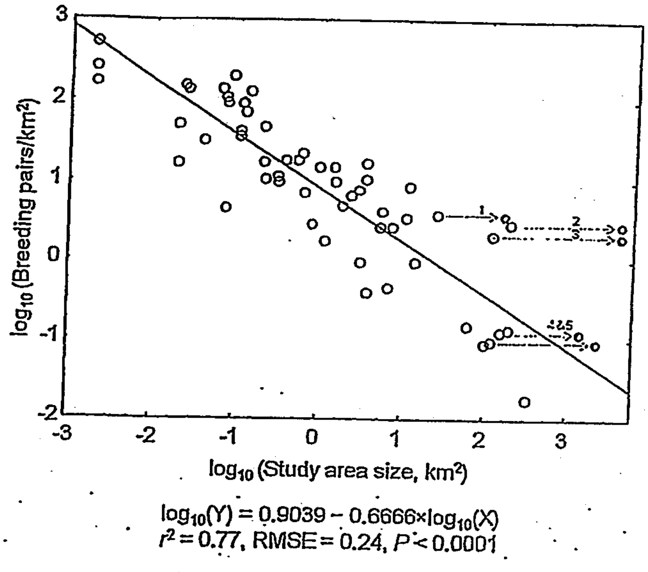

Figure 5. Nesting density related as an inverse power function to study-area size (open circles). Five density estimates (numbered arrows) were extrapolated to larger, sampled areas, including from (1) 46 random plots covering 15.3% of the Altamont Pass Wind Resource Area, California, USA (this study), (2) 6 random plots covering 3.3% of the Imperial Valley, California, and (3) 9 non-random plots covering 4.9% of the Imperial Valley (DeSante et al. 2007) and from random strip transects covering (4) 11% of Lake Mead National Recreation Area, California, and (5) 5.1% of Marine Corp Air Ground Combat Center near Twentynine Palms, California, USA (Crowe and Longshore 2010).

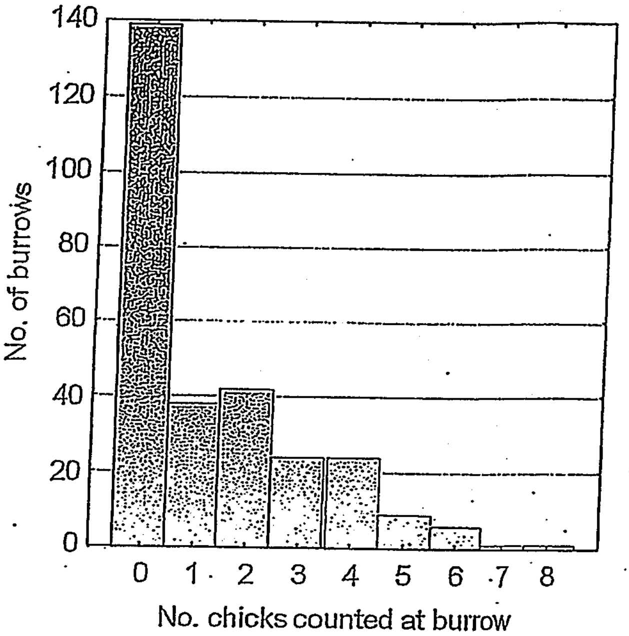

Figure 6. At nearly half the burrowing owl nest burrows, we failed to detect emerging chicks among 46 random plots during 2011 in the Altamont Pass Wind Resource Area, California, USA, and we found 7 chicks at 1 burrow and 8 chicks at another.

areas, and 51 chicks among 37 pairs that we detected incidentally between plots and extended areas. Thus, we counted 1.20 chicks/pair on plots, 2.03 chicks/pair on extended areas, and 1.38 chicks/pair incidentally. Our correlation between number of visits to plots and counted chicks/plot was not significant ($r = 0.24$, $P > 0.05$). Relying solely on the sampling plots to extend the average number of chicks/plot across the APWRA, we estimated 762 chicks (90% CI = 442–1,084), but this estimate was likely low because we probably missed chicks that remained in burrows and because we observed chicks as late as 7 October (chicks seen after July were not counted). Minimally, we estimated the APWRA supported 1,836 burrowing owls in 2011 (90% CI = 1,082–2,590).

DISCUSSION

The APWRA supported a large breeding population of burrowing owls in 2011, perhaps one of the most significant populations in California. With an estimated 537 breeding pairs (90% CI = 320–753) and a low estimate of 762 chicks (90% CI = 442–1,084) in spring 2011, the APWRA hosted enough burrowing owls to explain the large fatality estimates associated with the wind turbines (Smallwood et al. 2007, Smallwood and Karas 2009). Assuming the APWRA wind turbines kill 858–1,112 burrowing owls/year, as estimated by Smallwood and Karas (2009) over 2 time periods (1998–2003 and 2005–2007), this toll would comprise 42–55% of the 2011 population. However, fatality monitoring during the 1998–2003 period occurred at wind turbines now known to kill disproportionately larger numbers of burrowing owls, and the monitoring during 2005–2007 included a spike in fatalities in 2006 that has not been observed again. A more realistic estimate of the wind turbine-caused fatalities would be about 600 burrowing owls/year, and this toll would comprise <30% of the 2011 population.

Our count of 1.2 emerged chicks/pair appeared lower than the average count of 3.08 chicks/pair among 10 studies across North America (Smallwood et al. 2007), but this difference might not be significant because of the potential error in chick counts (Gorman et al. 2003). We observed 2.03 chicks/pair in the extended areas, perhaps because we visited all these nest sites only in July, after other nests in the extended areas had already failed in May and June. Our low chick counts might also have reflected a difficult year for burrowing owls in the APWRA due to lower prey availability or higher predation rates.

Most burrowing owl nests occurred in similar, predictable conditions, usually low on slopes and in low-stature grassland. They tended to be most abundant in the APWRA's interior, concentrated along major canyon passes oriented east–west. We found a few breeding pairs in drain culverts and rock caves where ground squirrels (Sciuridae) were absent, but most nested within ground squirrel colonies. Breeding pairs did not occur in all ground squirrel colonies, but the large expanse of low-stature grassland supporting thousands of ground squirrel burrow systems also provided a large habitat capacity for burrowing owl nesting in the APWRA.

We had to adjust survey methodology as vegetation and owl behaviors affected visibility. In mid-April, we could readily detect burrowing owls and their burrows from vantage points, so we spent proportionately more time from automobiles. By late April, grass had grown tall enough to hide burrowing owls and their burrows, and burrowing owls increasingly hid in their burrows, forcing us to walk transects. Furthermore, no chicks had yet emerged, so detections were possible only by observing either one or both members of a pair or by seeing sign around burrows.

Burrowing owl detection improved by June, because 1) most of the grass had either been consumed by livestock or had been flattened because of wind, and 2) chicks emerged, providing more owls per nest that could be detected. With the improved detection, we returned to relying primarily on vantage points and automobiles, though we always walked over areas hidden from vantage points. By July, our detections proved increasingly complicated by chicks learning to fly between burrows, and by probable brood sharing. To meet this challenge, we spent more time in automobiles watching the owls and determining their relationships to nest burrows and other types of burrow use before we exited the vehicles to map burrow locations.

With so many burrowing owls nesting in the APWRA, and with burrowing owls declining statewide, the Altamont Pass should be considered for directed conservation efforts. Repowering old-generation wind turbines, especially by locating new wind turbines farther away from landscape settings where burrowing owls often nest, would reduce the number of wind turbine-caused fatalities (Smallwood et al. 2007, 2009; Smallwood and Karas 2009). Also, conserving ground squirrels would likely increase the breeding population of burrowing owls in the APWRA, because the distribution of breeding pairs was limited by the presence of ground squirrel burrows. Conservation easements restricting ground squirrel control could be offered to ranchers as a compensatory mitigation measure required of new wind turbine projects.

Comparing density estimates.—As a predictor variable, the size of study area used to estimate breeding-pair density explained most of the variation in breeding-pair density (Fig. 5), but the resulting regression model greatly under-predicted breeding-pair density in the APWRA (it predicted 6.5% of the number estimated from field sampling). This model can predict breeding-pair density within small areas known *a priori* to support high densities of breeding pairs, but not over large areas sampled within random or systematic plots. Therefore, a simple extrapolation of mean breeding-pair density estimates reported in the published literature would be inappropriate. From the published estimates of breeding-pair density we used to construct the model, mean breeding-pair density extrapolated to the 167.6 km^2 of the APWRA would predict 7,827 pairs, or 12.3 times the number we estimated based on field surveys. Sampling over large areas, as was done in this study, appears to be a new trend (e.g., DeSante et al. 2007, Crowe and Longshore 2010, this study), and one that can generate more accurate estimates of burrowing owl population size in a larger landscape context. Based on the residuals in our regression model, our estimate of breeding-pair density was the second highest recorded for burrowing owls, lower only than an estimate from the Imperial Valley, California (Rosenberg and Haley 2004, DeSante et al. 2007).

MANAGEMENT IMPLICATIONS

Comparing density estimates to make management decisions should first classify the types of estimates being compared as derived from habitat fragments, known high-density clusters, or randomly or systematically selected plots. The size of the area used to estimate breeding-pair density can explain most of the variation in breeding-pair density when study sites were selected based on *a priori* knowledge of high density or at fragments of burrowing owl habitat. However, random or systematic sampling over large areas generates estimates of breeding-pair density that do not decline with increasing study area size.

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LITERATURE CITED

Barclay, J. H. 2007. Burrowing owl management at Mineta San Jose International Airport. Pages 146–154 *in* J. H. Barclay, K. W. Hunting, J. L. Lincer, J. Linthicum, and T. A. Roberts, editors. Proceedings of the California burrowing owl symposium, November 2003. Bird Populations Monographs No. 1. The Institute for Bird Populations and Albion Environmental, Point Reyes Station, California, USA.

Botelho, E. S., and P. C. Arrowood. 1998. The effect of burrow site use on the reproductive success of a partially migratory population of western burrowing owls (*Speotyto cunicularia hypugaea*). Journal of Raptor Research 32:233–240.

Butts, K. O. 1976. Burrowing owls wintering in the Oklahoma Panhandle. Auk 93:510–516.

Coulombe, H. N. 1971. Behavior and population ecology of the burrowing owl, *Speotyto cunicularia*, in the Imperial Valley of California. Condor 73:162–176.

Crowe, D. E., and K. M. Longshore. 2010. Estimates of density, detection probability, and factors influencing detection of burrowing owls in the Mojave Desert. Journal of Raptor Research 44:1–11.

DeSante, D. F., E. D. Ruhlen, and R. Scalf. 2007. The distribution and relative abundance of burrowing owls in California during 1991–1993: evidence for a declining population and thoughts on its conservation. Pages 1–41 *in* J. H. Barclay, K. W. Hunting, J. L. Lincer, J. Linthicum, and T. A. Roberts, editors. Proceedings of the California burrowing owl symposium, November 2003. Bird Populations Monographs No. 1. The Institute for Bird Populations and Albion Environmental, Point Reyes Station, California, USA.

Desmond, M. J., and J. A. Savidge. 1996. Factors influencing burrowing owl (*Speotyto cunicularia*) nest densities and numbers in western Nebraska. American Midland Naturalist 136:143–148.

Gleason, R. S., and D. R. Johnson. 1985. Factors influencing nesting success of burrowing owls in southeastern Idaho. Great Basin Naturalist 45: 81–84.

Gorman, L. R., D. K. Rosenberg, N. A. Ronan, K. L. Haley, J. A. Gervais, and V. Franke. 2003. Estimation of reproductive rates of burrowing owls. Journal of Wildlife Management 67:493–500.

Haug, E. A., and L. W. Oliphant. 1990. Movements, activity patterns, and habitat use of burrowing owls in Saskatchewan. Journal of Wildlife Management 54:27–35.

Johnson, B. S. 1997. Demography and population dynamics of the burrowing owl. Journal of Raptor Research 9:28–33.

Koshear, J., C. Shafer, E. F. van Mantgem, and R. V. Olsen. 2007. Burrowing owl management at Colonel Allensworth State Historic Park. Pages 155–164 *in* J. H. Barclay, K. W. Hunting, J. L. Lincer, J. Linthicum, and T. A. Roberts, editors. Proceedings of the California burrowing owl symposium, November 2003. Bird Populations Monographs No. 1. The Institute for Bird Populations and Albion Environmental, Point Reyes Station, California, USA.

Leptich, D. J. 1994. Agricultural development and its influence on raptors in southern Idaho. Northwest Science 68:167–171.

Martin, D. J. 1973. Selected aspects of burrowing owl ecology and behavior. Condor 75:446–456.

Restani, M., L. R. Rau, and D. L. Flath. 2001. Nesting ecology of burrowing owls occupying black-tailed prairie dog towns in southeastern Montana. Journal of Raptor Research 35:296–303.

Rodriguez-Estrella, R., and A. Ortega-Rubio. 1993. Nest site characteristics and reproductive success of burrowing owls (Strigiformes: Strigidae) in Durango, Mexico. Revista de Biologia Tropical 41:143–148.

Rosenberg, D. K., and K. L. Haley. 2004. The ecology of burrowing owls in the agroecosystem of the Imperial Valley, California. Studies in Avian Biology 27:120–135.

Shuford, W. D., and T. Gardali, editors. 2008. California bird species of special concern: a ranked assessment of species, subspecies, and distinct populations of birds of immediate conservation concern in California. Studies of Western Birds 1. Western Field Ornithologists, Camarillo, California, USA.

Shyry, D. T., T. I. Wellicome, J. K. Schmutz, G. L. Erickson, D. L. Scobie, R. F. Russell, and R. G. Martin. 2001. Burrowing owl population-trend surveys in Southern Alberta: 1991–2000. Journal of Raptor Research 35:310–315.

Smallwood, K. S. 1995. Scaling Swainson's hawk population density for assessing habitat-use across an agricultural landscape. Journal of Raptor Research 29:172–178.

Smallwood, K. S. 2013. Comparing bird and bat fatality-rate estimates among North American wind-energy projects. Wildlife Society Bulletin 37:19–33.

Smallwood, K. S., D. A. Bell, B. Karas, and S. A. Snyder. 2013. Response to Huso and Erickson's comments on novel scavenger removal trials. Journal of Wildlife Management 77:216–225.

Smallwood, K. S., D. A. Bell, S. A. Snyder, and J. E. DiDonato. 2010. Novel scavenger removal trials increase estimates of wind turbine-caused avian fatality rates. Journal of Wildlife Management 74:1089–1097 + Online Supplemental Material.

Smallwood, K. S., and B. Karas. 2009. Avian and bat fatality rates at old-generation and repowered wind turbines in California. Journal of Wildlife Management 73:1062–1071.

Smallwood, K. S., L. Neher, and D. A. Bell. 2009. Map-based repowering and reorganization of a wind resource area to minimize burrowing owl and other bird fatalities. Energies 2009:915–943. < http://www.mdpi.com/1996-1073/2/4/915 >. Accessed 20 Aug 2013.

Smallwood, K. S., and C. Schonewald. 1996. Scaling population density and spatial pattern for terrestrial, mammalian carnivores. Oecologia 105: 329–335.

Smallwood, K. S., C. G. Thelander, M. L. Morrison, and L. M. Rugge. 2007. Burrowing owl mortality in the Altamont Pass Wind Resource Area. Journal of Wildlife Management 71:1513–1524.

Teaschner, A. 2005. Burrowing owl nest site use and productivity on prairie dog colonies in the southern high plains of Texas. Thesis, Texas Tech University, Lubbock, USA.

Thomsen, L. 1971. Behavior and ecology of burrowing owls on the Oakland Municipal Airport. Condor 73:177–192.

Trulio, L. A. 1993. Comparisons of selected aspects of burrowing owl ecology at two sites in Santa Clara County, California. Journal of Raptor Research 27:63–64.

Trulio, L. A., and D. A. Chromczack. 2007. Burrowing owl nesting success at urban and parkland sites in northern California. Pages 115–122 *in* J. H. Barclay, K. W. Hunting, J. L. Lincer, J. Linthicum, and T. A. Roberts, editors. Proceedings of the California burrowing owl symposium, November 2003. Bird Populations Monographs No. 1. The Institute for Bird Populations and Albion Environmental, Point Reyes Station, California, USA.

Wiley, J. W. 1998. Breeding-season food habits of burrowing owls (*Athene cunicularia*) in southwestern Dominican Republic. Journal of Raptor Research 32:241–245.

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Habitat Fragmentation and Corridors

K. SHAWN SMALLWOOD

Since its conceptualization, habitat fragmentation has factored prominently in academic discussions on threats to biodiversity and species' conservation. Habitat fragmentation has also been incorporated into environmental law; e.g., one of the standards of the California Environmental Quality Act is to assess a project's potential interference with the movement of fish and wildlife. A principal countermeasure to habitat fragmentation—maintaining and improving habitat connectivity—has also factored into many academic and legal discussions. Natural or constructed corridors have often been promoted to mitigate the effects of habitat fragmentation by maintaining or improving animal movement and gene flow between habitat patches. Given its prominence in conservation biology and environmental law, one would expect that the term *habitat fragmentation* is well understood and based on a strong theoretical foundation.

However, given the widespread confusion over the meaning of the term *habitat* (Hall et al. 1997; Guthery and Strickland, this volume), which is a conceptual foundation of habitat fragmentation, it might be worth examining how well habitat fragmentation and corridors are understood. At its inception, some degree of vagueness around the concept was reasonable. Wilcox and Murphy (1985) argued, "That current theory is inadequate for resolving many of the details should not detract from what is obvious and accepted by most ecologists: habitat fragmentation is the most serious threat to biological diversity and is the primary cause of the present extinction crisis." Nearly three decades after Wilcox and Murphy (1985), and after humans have converted vast additional areas for human use, it is time to ask whether the theory has advanced sufficiently to resolve the details that matter. More importantly, it is time to examine whether the concepts of habitat fragmentation and corridors have mattered where these interrelated concepts needed to be implemented.

My objectives for this chapter are first to compare definitions of habitat fragmentation and corridors and synthesize these interrelated concepts. Next, I review environmental documents to reveal how practitioners perceive these concepts and to what degrees they apply them in impact analyses and conservation planning. Finally, I suggest how these concepts could be more consistently understood and effectively applied.

Habitat Fragmentation Defined

Wilcox and Murphy (1985) described habitat fragmentation as habitat loss and insularization. They described the risk of fragmentation as (1) destruction, reduction, or subdivision of demographic units; (2) loss of potential sources of immigrants; and (3) impedance of immigration caused by conversion of habitat between habitat patches. Saunders et al. (1991) described it as clearing of natural vegetation resulting in isolated, remnant patches of vegetation. Yahner (1996) recommended that habitat fragmentation be considered a process of diminishing size and increasing isolation of habitat fragments, where the result is habitat loss. Villard et al. (1999) described habitat fragmentation as a process in which a focal habitat type is partially or completely removed, resulting in an alternate configuration and reduced population persistence on the landscape. Bender et al. (1998) defined habitat fragmentation as an event that creates a greater number of habitat patches that are smaller in size than the original contiguous habitat. Francis (this volume) similarly described it as the spatial arrangement and shape of remaining habitat patches remaining after habitat loss. Bender et al. went on to define it as an event that produces an even greater population decline than would occur due to simple habitat loss.

Karr (1994) described fragmentation as a disruption of the linkages among patches that exchange ecologically important resources. Wilcox et al. (2002) described it as the loss of contiguity of accessible landscape from the perspective of the organism or some other ecologically important element, where landscape context, the spatial extent of the organisms' ecological interactions, and its demographic organization are critical factors.

Saunders et al. (1991) argued that habitat fragmentation not only causes biogeographic changes, i.e., increasing isolation of habitat patches, but also physical changes within the patches due to alterations in microclimate. Similarly, Wiens (1997) argued that the land conversions causing habitat fragmentation often sharpen habitat patch boundaries, alter connectivity, and shift the cost-benefit contours on the landscape. In other words, land conversions often inject into the remaining habitat fragments invasive species (Alberts et al. 1993), light pollution (Rich and Longcore 2006), noise pollution, atmospheric pollution, water pollution (Longcore et al. 1993), soil erosion, and added mortality factors such as automobile collisions, line collisions, electrocutions, poaching, and animal damage control. Furthermore, the net length of boundary increases between habitat fragments and the nonhabitat matrix, thereby increasing the number and variety of ways in which the land conversions can degrade the remaining habitat fragments. For example, large carnivores, due to their greater ranging behavior, will more often experience conflict with humans when the habitat fragments they occupy include greater edge-to-interior ratios (Woodroffe and Ginsberg 1998). Loss of the large predators can result in mesopredator release, thereby putting more predation pressure on other wildlife species residing in the fragments (Zembal 1993). Habitat fragmentation can expose patch-interior species to competitors, predators, and parasites that were more prevalent along the patch edges (Laurence and Yensen 1991; McCollin 1993; Porneluzi et al. 1993), even leading to pest outbreaks that can further degrade or remove the vegetation in habitat fragments (Roland 1993).

Ultimately, what separates habitat fragmentation from simple habitat loss is the disproportionate reduction in numerical capacity of the remaining habitat of the same net area (also see Francis, this volume). In other words, habitat reduced to a contiguous area of one hundred ha might support one hundred individuals of a certain species, but one hundred ha of habitat fragmented by impassable barriers (e.g., irrigation ditches, a freeway, residential or commercial development) and contaminated by physical and biological pollutants might support sixty individuals, or even zero individuals. Hypothetically, the protection or creation of corridors can maintain or restore some of the numerical capacity that would otherwise be lost to fragmentation. Applied to the previous example, the creation of one or more movement corridors might restore the capacity of the one hundred ha from sixty individuals to ninety individuals. Fragmentation is thought to disproportionately reduce numerical capacity, whereas corridors are thought to counteract this effect.

Why the disproportionate reduction in fragmented habitat? Genetic isolation is often cited, and so are other processes from island biogeography theory. Another factor is the suite of altered physical and biological conditions caused by greater edge-to-interior ratios and by the activities in the emergent nonhabitat matrix (Saunders et al. 1991; Bolger et al. 1997; Wiens 1997). But a third factor is social organization, which is a much stronger force often given credit in discussions of habitat. In an extreme example, our one hundred ha of fragmented habitat might consist of one hundred one-ha patches of isolated habitat, each patch capable of supporting one individual of a particular species on the basis of resources but incapable of supporting any individuals beyond a single generation due to insufficient space for breeding pairs, offspring, or larger demographic units. The spatial extent of habitat must comport with the social organization and associated behaviors of the species.

Long-term ecological relationships have established trajectories of how individuals of each species interface with their habitats. For example, some species have developed territorial behaviors to stabilize numerical responses to resource variability and therefore cannot tolerate crowding that might initially be forced by habitat fragmentation. Many species routinely shift locations of high activity, i.e., high density, either generationally or multigenerationally. Taylor and Taylor (1979) hypothesized that this spatial shifting of abundance enables species to rest local food resources or to escape predator or parasite loads. They also suggested that dispersed young might naturally congregate in new locations while the natal colonies senesce. Increasing barriers to movement and reducing the effective area of habitat patches, whether through land conversions or patch contamination, will act upon the species' behavior trajectories, which often involve much larger habitat areas than commonly studied by biologists (Smallwood 1999).

Consider that the mass density of populations of species of mammalian Carnivora averages nine kg/km^2 when scaled to the spatial area encompassing the population, referred to as the threshold area (Smallwood 2001a). This scaling of the spatial extent of a population to achieve a common mass density suggests an ecological allometry that is more sensitive to the spatial extent at which the species' population operates than it is to density or body mass. Indeed, threshold area was proportional to average female brain mass among species of Carnivora, and it was more responsive to female brain mass than to body mass (Smallwood 2001a). Species of Carnivora aggregate in similar numbers, but these numbers spread across larger spatial extents in proportion to increasing average brain mass. I hypothesize that the habitat areas intervening populations also scale with female brain mass. I also hypothesize that the habitat areas at which the populations of other animal species operate also scale with brain mass. Female brain mass as the axis of similitude would implicate parental care and other life-history attributes, along with basal metabolic rate and sensory perception. Species are probably much more sensitive to habitat fragmentation than can be measured by extrapolations of average density to the remaining areas of habitat fragments.

Discussion of habitat fragmentation often centers on individual species, but also often it encompasses multiple species. This dual use of the term causes confusion because the original definition of the term *habitat* applies to single species, but *habitat* is increasingly used to characterize vegetation complexes associated with multiple species, i.e., *habitat types*. When the discussion refers to wildlife habitat or habitat types, the basis of habitat fragmentation is typically a map of vegetation cover types that are convenient for mapping but probably ill-suited for analysis of habitat fragmentation. Measuring species richness in response to habitat fragmentation implies the same concept of habitat type as the basis of the analysis. In reality, a map of a species' habitat, i.e., that part of the environment where the species lives, would rarely match a mapped habitat type intended to apply to multiple species. That is, the nonhabitat matrix derived from such a map probably rarely applies to a given species. This source of confusion in discussions of habitat fragmentation will be discussed in the following discussion.

Corridors Defined

Beier and Loe (1992) defined the wildlife movement corridor as a linear habitat whose primary function is to connect habitat patches. Beier and Noss (1998) defined corridor as linear habitat connecting habitat patches of the same type and separated by a dissimilar matrix. Beier and Noss specifically excluded linear reaches of vegetation that were dissimilar to the habitat patches at issue; e.g., they excluded riparian corridors that might stretch between two patches of chaparral. In their meta-analysis of corridor effectiveness, Gilbert-Norton et al. (2010) defined corridor "as a narrow, linear (or near-linear) piece of habitat that connects two larger patches of habitat that are surrounded by a nonhabitat matrix." They also distinguished between natural and constructed corridors. Simberloff and Cox (1987) characterized what they termed *conservation corridors* as constructed corridors intended to connect habitat reserves to facilitate immigration and genetic exchange. Forman and Godron (1981) defined another two types of corridor as (1) strip corridor: a protected strip of landscape that is wide enough to include interior conditions typical of the interior conditions of habitat patches being connected by the strip, and (2) line corridor: a protected strip of landscape that is too narrow to include interior conditions typical of the interior conditions of habitat patches being connected by the strip but instead includes edge conditions that are typical of the edge conditions of the connected habitat patches. There are many other definitions of *corridor* in the literature, including movement corridor, habitat corridor, dispersal corridor, and landscape linkages, so it is clear that the corridor concept varies among investigators according to their application.

Beier and Noss (1998) concluded that corridors enhance population viability in connected habitat patches, but the papers they reviewed generally did not support their conclusion due to pseudoreplication and confounding caused by inadequate experimental design. In response to criticism of the evidence supporting the claim that habitat corridors improve population viability (Simberloff et al. 1992), Beier and Noss asserted that an experimental design was unnecessary for concluding that corridors are effective.

Synthesis of Habitat Fragmentation and Corridors

The concepts of habitat fragmentation and corridors are interrelated. These terms are part of the same discussion about a particular type of anthropogenic impact on wildlife species and whether and to what degree this type of impact can be moderated by the protection or creation of habitat or movement corridors. Habitat fragmentation can certainly happen naturally (Morrison et al. 1998), but naturally occurring habitat fragmentation has been rarely discussed and is of relatively low interest. Much greater focus has been on anthropogenic habitat fragmentation and whether and how to mitigate its adverse effects via corridor protection or construction. Discussions about corridors have been more confusing on the distinction of whether the context is about corridors as naturally occurring landscape or habitat features or as a conservation asset that needs to be protected or constructed to counter the effects of habitat fragmentation.

Corridor implies concentrated movement of one or more species, or disproportionate use of a linear portion of a landscape. Naturally occurring corridors are typically characterized as linear features of the landscape, such as stream basins and the vegetation that grows along streams. Linear features of the landscape have been thought to be used by animals as convenient guides to migration, dispersal, home range patrol, and other types of long-distance movement. However, biologists often assume that this relationship exists, rather than actually measuring it. In fact, animals move across all types of landscape features and vegetation patterns, not just linear features. The capacity for animals and plants to move across the "matrix" landscape might be just as important as the capacity for movement along corridors.

Conservation corridors also imply concentrated movement of one or more species but only in the context of navigating between habitat patches in a changing environment. Whether protected or constructed, conservation corridors are intended to enable continued or improved movement of wildlife between habitat fragments. However, the effectiveness of conservation corridors is rarely tested, and many so-called corridors may themselves be nothing more than remnant fragments of habitat. Furthermore, many conservation corridors have been established to suit the convenience of the planners. I have rarely seen evidence that animal movement patterns were measured prior to the establishment of conservation corridors. The common assumption appears to have been that the animals surviving habitat fragmentation will use the corridors, perhaps because they have no other choice. Nevertheless, the purpose of conservation corridors has been to offset the adverse impacts of habitat fragmentation. The only overlap in the scientific dialogue between naturally occurring corridors and conservation corridors is in the protection of the former as the latter as habitat fragmentation proceeds.

Habitat Fragmentation and Corridors in Practice

The concepts of habitat fragmentation and corridors have been favorite topics of debate in the scientific literature, having inspired thousands of papers and books with one or both of these concepts being addressed. Scientists debate definitions related to these concepts, and they debate how to measure habitat fragmentation and how to identify corridors from within a landscape matrix. While these scientific debates have continued for nearly three decades, other human actions have transformed large tracts of wildlife habitat for human purposes. It is at the business end where the concepts of habitat fragmentation and corridors have been in most urgent need of general understanding and implementation.

It is in environmental reviews, management plans, environmental laws, and, ultimately, actions where the concepts of habitat fragmentation and corridors matter most (Salwasser 1990). The practitioners who need to understand these concepts include environmental consultants, biologists working for regulatory and natural resource agencies (i.e., permitting agencies), members of the public who participate with project planning, environmental lawyers, and decision-makers including city councils, county supervisors, state commissions, and judges. Unless the concepts of habitat fragmentation and corridors are understood clearly by practitioners, and unless these concepts influence management and conservation decisions in meaningful ways, then, to be frank, the scientific discussion of them has been of little value.

To ascertain the effectiveness of the concepts of habitat fragmentation and corridors, I examined environmental review documents with which I participated as an expert witness over the past twenty years (appendix 7.A). I examined how the concepts of habitat fragmentation and corridors were portrayed and implemented in the face of real proposed projects that, if approved, could potentially affect wildlife through the process of habitat fragmentation and destruction or protection of corridors. Some of these projects involved very small areas, but even a project on a small habitat area can potentially sever habitat linkages, thereby contributing to habitat fragmentation. Many of these projects were constructed, some were not, and others are waiting for permits or construction starting dates, but the actual outcomes of these proposed projects were irrelevant to my examination of how the concepts of habitat fragmentation and corridors were treated in the environmental reviews.

Under the California Environmental Quality Act (CEQA), which is the law that I most often work under as an expert witness, a project would have a significant impact on the environment if it were to "interfere substantially with the movement of any native resident or migratory fish or wildlife species or with established native resident or migratory wildlife corridors, or impede the use of native wildlife nursery site." This language is the closest that CEQA comes to recognizing habitat fragmentation as an impact and corridors as the solution. Consultants and resource agency biologists often cite this CEQA standard before discussing wildlife movement corridors in the context of project impacts or mitigation. Because environmental reviews performed in California are supposed to address the project's impacts on wildlife movement, habitat fragmentation and corridors should be expected to be discussed in environmental review.

I assessed the environmental review documents of fifty-seven proposed projects involving 83,413 ha (app. 7.A). Wildlife movement corridors were mentioned in thirty-four reviews (60%) and analyzed in zero reviews. Habitat fragmentation was mentioned in twenty-six reviews (46%) and analyzed in two reviews (3.5%). Where the concepts of habitat fragmentation and corridors matter most—where they matter to environmental decision-making—about half of the environmental reviews made no mention of them.

In one of the two reviews that included an analysis of habitat fragmentation (app. 7.B, 5), the level of project impact was calculated in acres and was based on the starting habitat value and ending habitat value. Habitat values were 100% for parcels zoned ≥40 acres, 75% for parcels zoned ≥20 acres, and 25% for parcels zoned ≥10 acres. However, because >90% of the project area was composed of parcels zoned ≥40 acres, the project area was concluded to have 100% habitat value. In the other review including a quantitative analysis (app. 7.B, 36), habitat fragmentation was analyzed as the amount of habitat area that will be lost as a percentage of the cumulative impacts in the county.

An analysis that is sufficient for the California Environmental Quality Act or the National Environmental Policy Act is not the same type or level of analysis that is practiced by research scientists, so even the two crude analyses that I encountered in environmental review documents would not have been regarded as scientific by the research community. Essentially, there has been no scientific analysis performed and no scientific predictions made of the effects of habitat fragmentation or corridor loss or protection resulting from any of the environmental reviews with which I have been professionally involved. Consultants and agency biologists preparing the environmental reviews for about half of the projects have expressed familiarity with the terms *habitat fragmentation* and *corridors*, but neither knew how to perform impact assessments relevant to these concepts, or chose not to do so.

Descriptions of Habitat Fragmentation

Environmental reviews defined or described habitat fragmentation in various ways, expressing some overlap as well as considerable divergence in understanding of the concept. One review defined habitat fragmentation as habitat loss resulting in displacement of individuals from the developed area (app. 7.B, 1). This review equated habitat fragmentation with habitat loss characterized by displacement rather than by any overall population reduction. In another review, this notion of displacement was rolled into one of crowding within fragmented patches of Swainson's hawk (*Buteo swainsoni*) foraging habitat, "increasingly fragmented with more intensive uses (agricultural/residential or urban development) on smaller minimum parcel sizes" (app. 7.B, 5). In this version of habitat fragmentation, the affected species suffers the inconvenience of crowding but not necessarily any overall numerical reduction.

In contrast to the notion of simple displacement, or at worst crowding due to habitat fragmentation, another review explained that small fragments of habitat can only support small populations and are more vulnerable to extinction (app. 7.B, 6). Alluding to habitat fragmentation, an environmental review document included the statement, "The ability for wildlife to freely move about an area and not become isolated is considered connectivity and is important to allow dispersal of a species to maintain and exchange genetic characteristics, forage (food and water), and escape from predation" (app. 7.B, 8). Imposing barriers to movement qualified as habitat fragmentation in one review (app. 7.B, 41), and in two other reviews barriers were implied: "Fragmentation of open space areas by urbanization creates 'islands' of wildlife habitat that are more or less isolated from each other" (app. 7.B, 4), and "Habitat fragmentation involves the potential for dividing sensitive habitat and thereby lessening its biological value" (app. 7.B, 10). In another review, the public learned that "habitat fragmentation can result when development occurs within larger regions of natural habitat. The effects of habitat fragmentation can extend beyond the boundaries of an area proposed for development" (app. 7.B, 13). This review went on to describe how fragmentation of a creek could adversely affect downstream wetlands by altering hydration periods. Thus, some environmental reviews recognized the potential for net greater impacts caused by movement barriers and habitat fragmentation as compared to simple habitat loss.

Descriptions of Corridors

Environmental reviews defined or described corridors more often than they defined habitat fragmentation. One review explained that corridors link blocks of habitat, where the habitat in corridors resembles habitat preferred by the target species (app. 7.B, 33), and another defined wildlife corridors as "pathways or habitat linkages that connect discrete areas of natural open space otherwise separated or fragmented by topography and changes in vegetation." (app. 7.B, 31).

Corridor definitions sometimes appeared tailored to serve the strategy of the impacts assessment or mitigation plan. Kern County (app. 7.B, 2, 4.3–20) wrote, "Wildlife movement corridors, also referred to as dispersal corridors or landscape linkages, are generally defined as linear features along which animals can travel from one habitat or resource area to another . . . drainages, ridgelines, and other natural and built linear features and barriers often serve as areas that wildlife routinely use to access essential natural resources. It is assumed that wildlife species would use such features for movement if they occurred within the survey area." This definition implies that wildlife will use the land left undeveloped as a corridor, thereby mitigating the project's impacts.

In the review of a project seeking to mitigate its impacts by leaving undeveloped land as corridors, PBS&J (app. 7.B, 4, 5.4–8) wrote,

> Terms such as habitat corridors, linkages, crossings, and travel routes are used to describe physical connections that allow wildlife to move between patches of suitable habitat in undisturbed landscapes as well as environments fragmented by urban development . . . Wildlife corridors are usually bounded by urban land areas or other areas unsuitable for wildlife. The corridor generally contains suitable cover, food, and/or water to

> support species and facilitate movement while in the corridor. Wildlife corridors link areas of suitable habitat that are otherwise separated by areas of non-suitable habitat such as rugged terrain, changes in vegetation, or human disturbance. Wildlife corridors are essential to the regional ecology of a species because they provide avenues of genetic exchange and allow animals to access alternative territories as dictated by fluctuating population densities. . . . Wildlife corridors are typically relatively small, linear habitats that connect two or more habitat patches that would otherwise be fragmented or isolated from one another.

This last part of the definition appeared to rationalize leaving only a narrow strip of land as the corridor.

The review of another large residential development—for which the principal mitigation measure was the protection of land to be left undeveloped as movement corridors (app. 7.B, 9)—defined wildlife movement corridors as the "gentlest topography and more open habitat." A subsequent review document for this project identified a wildlife corridor as a drainage or riparian vegetation in a canyon, which traverses no other topographic features and will not be surrounded by development in the future. Similarly, the consultants who prepared the planning for a multispecies habitat conservation plan across a large portion of San Diego County (app. 7.B, 33) defined a wildlife corridor as "a route used by one or more species to move between two areas of habitat. A corridor can be defined by topographical features such as ridges or valleys, habitat types such as bands of riparian vegetation, areas of natural open space passing between two man-made constraints, or even game trails used by many generations of animals" (page 6-1). The review defined two types of corridor—local and regional (page 6-1): "Regional corridors connect open space in a region and allow activities such as dispersal of young, genetic transfer between subpopulations, and seasonal migration. Local corridors are routes within a habitat used regularly by an animal to commute between resources such as denning sites, water sources, and hunting or foraging areas."

According to another review (app. 7.B, 10), "Wildlife corridors are areas of habitat used by wildlife for seasonal or daily migration." In another (app. 7.B, 14), "Wildlife corridors are routes frequently utilized by wildlife that provide shelter and sufficient food supplies to support wildlife species during migration. Movement corridors generally consist of native or undeveloped matrix habitats (e.g., greenbelts, parks, or other open spaces) that span contiguous acres of unfragmented habitat. Wildlife movement corridors are an important element of resident species home ranges." Pacific Municipal Consultants (app. 7.B, 16, 4.4–20) defined wildlife movement corridors as "traditional routes used by wildlife to travel within their home range. Movement corridors typically provide wildlife with undisturbed cover and foraging habitat and are generally composed of several trails in contiguous spans of forested, riparian, riverine, and woodland habitats. The width of movement corridors varies depending on the topography. Movement corridors are an essential element of home ranges of a wide variety of wildlife including black bear (*Ursus americanus*), grey fox (*Urocyon cinereoargenteus*), mountain lion (*Felis concolor*), and other migratory wildlife."

According to EGI (app. 7.B, 34), "Wildlife movement corridors and habitat linkages are areas that connect suitable wildlife habitat areas in a region otherwise fragmented by rugged terrain, changes in vegetation, or human disturbance. Corridors are generally local pathways connecting short distances usually covering one or two main types of vegetation communities. Linkages are landscape level connections between very large core areas and generally span several thousand feet and cover multiple habitat types. Natural features such as canyon drainages, ridgelines, or areas with vegetation cover provide corridors and linkages for wildlife travel. The habitat connectivity provided by corridors and linkages is important in providing access to mates, food, and water, allowing the dispersal of individuals away from high-density areas, and facilitating the exchange of genetic traits between populations."

According to ECORP (app. 7.B. 36), corridors are where surrounding habitat concentrates wildlife movement, or which link large areas of undeveloped open space. Other reviews defined wildlife corridors as "areas that connect suitable wildlife habitat areas within a region, especially where species are known to migrate" (app. 7.B, 37), and "where resident and migratory animals freely move, generally within or between preferred habitat areas" (app. 7.B, 30).

Environmental reviews sometimes identified particular parts of the project area as wildlife movement corridors, which were then claimed to be unaffected by the project or proposed to be protected or enhanced as mitigation (app. 7.B, 2, 9, 18, 25, 41). Corridor designations were arbitrary in these reviews and may have been influenced by convenience. More often, environmental reviews claimed that the project site did not occur in an established or major wildlife movement corridor (app. 7.B, 2, 3; 5, 7, 11, 12, 14, 15, 17, 19, 20, 26, 30, 34, 36, 46). The premise implied by this strategy was that there exists a registry or inventory of wildlife movement corridors that was consulted and turned up a negative result. Even if there was an inventory of corridors, the inventory's scientific credibility would hinge on species-specificity and empirical foundation.

Summary of Habitat Fragmentation and Corridors in Practice

Environmental review documents rarely cited peer-reviewed papers on the topics of habitat fragmentation and corridors. Given their varied characterizations of habitat fragmentation and corridors, it was apparent that practitioners often only vaguely understood these concepts. The concept of habitat fragmentation was the most poorly understood of the two concepts, having been omitted from discussion in most reviews and defined erroneously in most reviews that addressed the topic. In some cases, the practitioners may have crafted definitions of these terms to minimize estimates of project impacts or to minimize the cost of mitigation. Among the environmental documents I reviewed, none of the alleged existing corridors were measured on any variables for corridor function. None of the corridors proposed for protection as project mitigation were accompanied by commitments to test for corridor functionality following project development. Instead of testing for corridor functionality, those who performed environmental reviews of the fifty-seven projects addressed herein relied on hopeful speculation that land designated as corridors would actually function as corridors.

Based on my review of environmental assessments on the impacts and mitigation planning of fifty-seven projects, I conclude that the concepts of habitat fragmentation and corridors have too often failed to matter where they most needed to be implemented. There might exist nonscientific reasons for this poor translation of scientific concepts into conservation practice, but even so, it should be more difficult to abuse concepts that are clearly and consistently understood. If these concepts are going to matter in the future, if they are going to conserve wildlife in the face of massive anthropogenic land use changes, then barriers must be overcome to effective measurement and consistent understanding.

Barriers to Measuring Habitat Fragmentation and Corridors

The Dependent Variable

The effects of habitat fragmentation and corridors must be measureable if they are going to translate into meaningful theory or sound conservation planning and decision-making. It is not enough for conservation biologists to proclaim that "we know that habitat fragmentation is happening and corridors are effective at rectifying the effects of fragmentation." The effects need to be quantified and relevant hypotheses tested at appropriate spatial and temporal scales. It is therefore necessary to identify what it is that fragmentation and corridors are thought to be affecting.

Species richness and species diversity were early favorites as dependent variables responding to habitat fragmentation, due to their frequent use in island biogeography theory and the often-claimed likeness between habitat fragments and islands. Species richness remains in use, as Gilbert-Norton et al. (2010) used it as an indirect measure in their meta-analysis of corridor effectiveness. However, the comparability of terrestrial habitat fragments to islands is questionable because the fragments occur in very different ecological contexts than do islands. Furthermore, species richness and species diversity are community measures, which, by a proper definition of habitat (Hall et al. 1997), qualify as poor measures of habitat fragmentation. In other words, because habitat is a species-specific concept, its fragmentation is best measured for the species to which the habitat is associated. Species richness and species diversity might be more suitable dependent variables in analyses of landscape or ecosystem fragmentation.

Population persistence would probably generate the greatest agreement among ecologists and wildlife biologists as the ultimate expression of the effects of fragmentation and corridors, although there might not be agreement on what qualifies as a population or what time period is sufficient for measuring persistence. Abundance could be measured as a variable that indicates the population and could be expressed as the number of individuals, breeding pairs, or colonies or populations. The health of the population can also be measured, such as by genetic variation, productivity, and survival, but these variables are difficult to measure and are probably more proximal expressions of the effects of fragmentation and corridors.

Discussed more often than applied, a common measure of corridor effectiveness has been movement of individuals within the corridor, or corridor use. This measure goes to a hypothesized principal mechanism of population decline caused by habitat fragmentation and of population rescue provided by corridors. Organisms must be able to disperse, which habitat fragmentation is hypothesized to hamper and which corridors are hypothesized to facilitate. Indeed, Gilbert-Norton et al.'s (2010) meta-analysis of corridor effectiveness tested whether movement increased with the existence of natural or created corridors. However, a dependent variable measured as the rate of movement between habitat patches does not express the capacity of the remaining habitat. The investigators using this dependent variable only assume that improved movement translates into greater habitat capacity. They selected movement as their dependent variable because movement is critical to colonization and gene flow, which they believed increases population viability and decreases extinction probability. In reality, the movement that is measured could be of the same individual traveling back and forth between habitat patches, or it could be of individuals dispersing into an ecological sink. Whereas the measurement of movement rates along a corridor can be reassuring, it cannot alone express corridor effectiveness because it is in the connected habitat patches where the corridor's effects ultimately matter.

Investigators have often discussed or used indicators of habitat fragmentation, including percentage of habitat lost, perimeter-to-area ratio, mean patch size, and fractal dimension. The usefulness of these indicators depends on the degree of correspondence between the indicators and direct measures of fragmentation.

Measuring Habitat

The concept of habitat fragmentation has been poorly understood by the community in greatest need of understanding it. This poor understanding might be exacerbated by a generally poor understanding of habitat. The habitat concept itself has lacked consistent definition (Guthery and Strickland, this volume), leading to ample confusion among practitioners as well as theorists. The simplest and perhaps most appropriate definition of habitat would be that part of the environment within a species' geographic range where the species actually lives (Morrison and Hall 2002). However, Hall et al. (1997) found vague or imprecise characterizations of habitat in 82% of the fifty papers they reviewed. A popular characterization of habitat has been the classification of vegetation into "habitat types," to which species are then associated based on relative abundance of the species in each type. Even vaguer is the characterization of habitat as all types of natural vegetation bounded by lands converted for human use (Bolger et al. 1997). This characterization of habitat glosses over what specifically it is in the environment that species are selecting; it overlooks the resources in the environment that each species needs for metabolism and reproduction, and which limits the species' abundance.

The very fact that species are selecting resources from the environment indicates that the second law of thermodynamics applies to the distribution and abundance of wildlife (Smallwood 1993, 2002). The primary regulator of the distribution and abundance of any wildlife species is the degree to which individuals can metabolize sufficiently and for long enough to successfully reproduce. The primary resource must be energy for metabolism, but then other resources are needed to locate and process this energy, including atmosphere (water), nutrients, ecosystem processes, and the suite of other biological species in the environment. Because energy needed for metabolism is the primary resource that defines a species' habitat, these energy sources should be the core of any definition of a species' habitat, and the portions of the environment that facilitate the metabolism of these resources should round out the definition. Thus, habitat is defined as the suite of environmental elements—soils, plant species, and other animal species—that includes the species' limiting resources. Habitat is a vague, indicator-level representation of the parts of the environment that are essential to the species' metabolism and reproduction, and hence to its persistence (Morrison et al. 1998).

The complexities of measuring habitat fragmentation increase when considering the more fundamental challenges of measuring habitat selection. After all, measuring habitat fragmentation presupposes that the habitat undergoing fragmentation was accurately characterized as selected habitat. A fundamental problem with this presupposition is that the species performing the measurements is not the species whose habitat selection is being measured, so we should not assume that we perceive the species' environment in the same manner as the species we are studying. We draw inferences from patterns in the data, and tend to conclude that a species has selected a portion of the environment based on the species' disproportionate number of individuals or time spent in that selected area. Our inferences, however, could be confounded by multiple factors, including our measurement of the wrong environmental variables, the wrong spatiotemporal grain or extent, and our counting of the wrong study unit, e.g., individuals versus breeding pairs versus colonies versus populations (discussed in the following section). In some cases, disproportionate use of an area might represent an ecological sink (Smallwood 2002).

A second fundamental problem with measuring habitat selection is the epistemological context of the measurements, such as whether the investigator measures meaningful pattern as deviation from a uniform or random null pattern (Smallwood 2002). The starting point matters for measuring meaningful associations with places on the Earth where environmental resources might be selected. The null condition in which the investigator assumes that species' individuals would be uniformly spaced would result in greater deviations between the null condition and the measured condition, so long as the appropriate variable(s) is being measured at the appropriate spatiotemporal grain and extent. If the species' individuals space themselves out in a regular pattern due to social rules (home ranges), then the study unit might more appropriately consist of social groups rather than individuals, and measurements might be needed at much larger spatiotemporal grain and extent. As for the null condition in which the investigator assumes that species' individuals would be randomly spaced, deviations between the null condition and the measured condition would be shorter, and these deviations might vary depending on what is meant by *random*.

A uniform pattern is easy to define. A uniform pattern results when the study units are maximally distant from each other. It is the pattern one would expect if there were no relationships between organisms and environmental resources. An interesting exception to this expectation would be when regular patterns of distribution result from social rules organized around the species' long-term experience with the resources, but even in cases of socially driven regularity, deviations from a uniform pattern should be evident at larger spatial extents. Not only is meaningful pattern relatively easy to measure as deviations from a null pattern of uniformity, but these deviations can also be interpreted from laws of thermodynamics and information theory (Shannon and Weaver 1949; Kullback 1959; Phipps 1981).

Many might regard a random pattern as also easy to define. A random pattern is the pattern of distribution resulting from lack of influence from the locations of other individuals (Fisher 1950; Taylor 1961). Because many observed or simulated patterns of distribution can be regarded as random, the starting point for measuring meaningful patterns of aggregation is less clear than it is for uniformity. Along a theoretical continuum from uniform to aggregated patterns, randomness occupies an unknown and perhaps varying proportion of this range somewhere between uniformity and completely aggregated (see Smallwood 2002, fig. 6.3). Furthermore, the definition of a random pattern highlights interactions among individuals, which serve as the most common study unit in habitat selection studies. It implies that interactions among individuals result in the meaningful patterns of aggregation that we measure, but in so doing it also obscures the influence of environmental resources. Of course, we know that individuals interact and that these interactions influence the patterns of distribution that we observe, but randomness as the null condition invokes these interactions as paramount and starts the analysis of habitat selection from a different worldview than starting it from uniformity as the null condition.

A third fundamental problem with measuring habitat selection—and this problem is related to the second when it comes to detecting meaningful patterns of habitat selection—is the spatiotemporal grain and extent of the measurements, whether the measurements are made during a snapshot of the organisms life (or over a season, a year, multiple years, multiple generations, or multiple periods of the species' multiannual cycle of abundance) and whether the measurements are made within an animal's home range, across multiple home ranges, or across an area capable of supporting an entire population or larger demographic unit. In other words, the sampling resolution and measurement sensitivity must be sufficient for testing hypotheses of habitat selection (Huston 2002) before any meaningful tests can be performed on the effects of habitat fragmentation or corridors. As an example, measuring habitat selection immediately after land conversions fragmented the habitat could be misleading due to temporary crowding of individuals in the remaining habitat fragments (Whitcomb et al. 1981; Lovejoy et al. 1986; Scott 1993).

The capacity of a species to utilize an environment (i.e., its habitat), or to populate its numbers within an environment, will be limited by the most limiting resource according to Liebig's Law of the Minimum. This limitation might vary across the species' environment or through time at given locations. This variation in where and when Liebig's Law of the Minimum will operate defines the ultimate source of instability in the environment with which a species must contend. Therefore, each species evolves strategies to contend with the instability in its most limiting resource(s), and the most effective strategies tend to be behavioral and social in nature, often resulting in time lags and feedback loops affecting numerical responses. Species of wildlife tend to buffer themselves against instability in the most limiting resources by developing stubborn social rules, such as home range tenure (home ranges often remain unchanged even in the face of superabundant food resources) and mating hierarchies. They also buffer themselves against resource instability by developing stubborn behaviors such as seasonal migrations, foraging self-limitations, and foraging by gestalt where resources are expected to be found rather than where they are known to be found (Hutto 1990). Therefore, not only is measuring habitat selection prone to confounding due to resource instability in past and even relic environments, but measuring the effects of habitat fragmentation is prone to further complications due to a new type of resource instability in the current environment.

A fourth fundamental problem with measuring habitat selection is achieving replication and interspersion of "treatments," which in the context of habitat selection studies are the categories of vegetation, landscape, or specific resources being associated with counts of the species under study. Pseudoreplication results from inadequate replication and interspersion of treatments, but these conditions are often more difficult to achieve in mensurative studies (Hurlbert 1984). Testing the effects of habitat fragmentation and corridors is likely even more prone to pseudoreplication than is testing for habitat selection.

A fifth fundamental problem is often not knowing why the animals that were counted were located where they were found by researchers. The aggregations we encounter can be one of four types (Smallwood 2002): resource, demographic, early stage, and constrained. A resource aggregation would likely be a temporary collection of individuals where a limiting resource has become available. A demographic aggregation might be a temporary collection of breeding pairs or a gathering of juveniles or subadults developing social relationships. An early-stage aggregation would be the collection of individuals corresponding with the level of colonization at the time of the habitat survey. A constrained aggregation would be the type of aggregation that is most prone to confounding in the context of fragmented habitat, because it is a forced aggregation, or even an expression of crowding. Either way, the constrained aggregation is likely temporary, so measurement of it can yield an overoptimistic interpretation of habitat selection (or corridor use).

Also related to habitat selection and how its measurement can influence interpretation of the effects of habitat fragmentation is the effect of location. Although there has been discussion of the influences of the extent of habitat loss and degree of isolation on habitat fragmentation, as well as the edge-to-interior ratio and the types of activities and species composition in the areas surrounding habitat fragments, there has been little, if any, discussion of the effect of location. The influence of location on corridor effectiveness has often been discussed, but its influence on habitat fragmentation has been neglected. Certain habitat areas are likely strategic to a species for one or more reasons, so their removal would have disproportionate consequences for the species. An example would be a landscape bottleneck, where animals must be able to negotiate for dispersal. Another example would be systematic loss of habitat in a particular landscape context, such as the upland grassland areas surrounding ponds. Many species forage or breed in ponds but spend the rest of their lives in the surrounding grasslands. Therefore the effects of habitat fragmentation might vary by location, all else being equal.

Permeable Movement Barriers

Just as difficult as it might be to characterize habitat fragments as places where a species would really choose to reside is to characterize the matrix areas surrounding the fragments as nonhabitat and as barriers to movement between fragments. For some species, the matrix might be easily shown to block movement, but for others the matrix might serve as a portion of the species' habitat and be completely or partially permeable to movement. La Polla and Barrett (1993) tested corridor width and presence on captive-reared meadow vole (*Microtus pennsylvanicus*) density in grassland fragments, but the matrix was simply mowed grass. The mowed grass may have been completely permeable to vole movement between unmowed fragments. Tests for habitat fragmentation and corridor effectiveness need to clearly characterize the ability of each species' ability to move across or reside within the matrix areas.

Conclusions

Habitat fragmentation likely qualifies as a major threat to the persistence of many plant and animal species, and corridors might help to mitigate this threat. However, conceptualizations of habitat fragmentation and corridors vary widely in the scientific literature and are either poorly understood or intentionally abused by many practitioners. The science supporting these concepts needs to improve. Appropriate dependent variables representing responses to habitat fragmentation and corridors need to be measured at appropriate spatial grain and extent and over appropriate time periods. Quantitative tests of habitat fragmentation and corridor effectiveness should be made to single species that are sufficiently understood to measure habitat variables rather than using overly simplistic maps of vegetation cover types that are applied to suites of wildlife species (e.g., Bolger et al. 1997). Much more care is needed on the measurement of habitat fragmentation and corridor effectiveness, and in the discussion of these concepts.

Definitions of habitat fragmentation and corridors need to be more explicit. Scientists need to more effectively convey their concepts of habitat fragmentation and corridors to biologists in natural resource agencies and environmental consulting firms, as well as to attorneys, political decision-makers, and the public. Where these concepts really matter is where decisions are being made about whether and how to further fragment habitat and how to mitigate the effects of this fragmentation. These decisions are being made now, and with the proliferation of habitat conservation plans these decisions increasingly involve very large areas of wildlife habitat. Vague and poorly understood concepts such as habitat fragmentation and corridors can cause more harm than good in wildlife conservation, as I have seen too many times in the aftermath of the environmental reviews I addressed in this chapter.

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APPENDIX 7.A

Summary of environmental reviews with which I have had personal experience and whether the reviews mentioned or analyzed the potential project impacts on habitat fragmentation or movement corridors. Citations appear in Appendix 7.B by reference (Ref) number in the right column. MW refers to megawatts in energy projects.

Source	Project	Acres	Mention/analysis of potential impacts related to: Habitat fragmentation	Mention/analysis of potential impacts related to: Movement corridors	Ref
BLM 2012a	104 MW Steens Mt Wind Energy Project	8,700	yes/no	no/no	1
Kern County 2012	250 MW Beacon Photovoltaic Project	2,301	no/no	yes/no	2
Yolo County 2008	Clark-Pacific Concrete Precast Factory	90	no/no	yes/no	3
PBS&J 2008	Delta Shores residential/commercial development	782	yes/no	yes/no	4
Sacramento LAFCo 2011	Expand City of Elk Grove's Sphere of Influence to accommodate future residential/commercial development	7,869	yes/yes	yes/no	5
BLM 2010	370 MW Ivanpah Solar Electric Generating System	4,073	yes/no	yes/no	6
City of Lancaster 2012	60 MW Summer Solar and Springtime Solar Projects	216	no/no	yes/no	7
HDR 2011	600 MW Mount Signal & Calexico Solar Farm Projects	4,228	no/no	yes/no	8
Impact Sciences 2002	Newhall Ranch residential development	12,000	yes/no	yes/no	9
Caltrans 2006, 2010	Route 84 Safety Improvement Project (Niles Canyon)	24	yes/no	yes/no	10
BLM 2012b	20 MW Ocotillo Sol Project	115	yes/no	yes/no	11
Foothill Associates 2006	Regional University development	1,282	yes/no	yes/no	12
EDAW 2008	Rio del Oro Specific Plan, housing development	3,828	yes/no	yes/no	13
Imperial County 2012	200 MW Solar Gen 2 Array Project	2,009	yes/no	yes/no	14
Design, Community & Environment 2010	St. John's Church expansion, requiring removal of mature forest canopy	3	no/no	yes/no	15
Pacific Municipal Consultants 2005	Yuba Highlands Specific Plan residential development	2,902	yes/no	yes/no	16
Ecology and Environment 2007a, b	67 MW Windy Flats West Wind Energy Project	616	no/no	no/no	17
	Antonio Mountain Ranch residential development	523	yes/no	yes/no	18
BLM 2012c	150 MW Desert Harvest Solar Project	1,464	yes/no	yes/no	19
Ecology and Environment 2012	49.9-MW Hudson Ranch Power II Geothermal Project and Simbol Calipatria Plant II	100	yes/no	yes/no	20
WRA Environmental Consultants 2009	J&J Ranch, 24 Adobe Lane residential development	20.5	no/no	yes/no	21
Harzoff 1992; Huffman & Associates 1992	Putah Creek Parkway Demonstration Project, adding bike path and fence along centerline of riparian corridor	10	no/no	no/no	22
	UC Davis Long-Range Development Plan		no/no	no/no	23
Yeast 1998	Sunset SkyRanch Airport expansion	108	no/no	no/no	24
Raney Planning & Management, Inc. 2004	Covell Village residential development	422	no/no	yes/no	25
Planning Consultants Research	West Bluff residential development, the last upland area bordering the Ballona Wetlands	44	no/no	yes/no	26
Thomas Reid Associates 1997	Natomas Basin HCP for residential development on wetlands and fields used for rice cultivation	21,300	yes/no	no/no	27
County of Yolo 2010	365-foot-tall Results Radio Tower with guy wires	1.7	no/no	no/no	28
Glazner Environmental Consulting 1999	Atwood Apartments	55.6	yes/no	no/no	29
City of Sierra Madre 2001; EIP Associates 1999	Maranatha High School	63	yes/no	yes/no	30
Grassetti Environmental Consulting 2004; Edelstein 2003	Creekside Highlands residential development along riparian zone of Ward Creek	7.1	yes/no	yes/no	31

Appendix 7.A continued

Source	Project	Acres	Mention/analysis of potential impacts related to: Habitat fragmentation	Mention/analysis of potential impacts related to: Movement corridors	Ref
Humboldt Bay Harbor & US Army Corps of Engineers 1995	Dredging and disposal of 5.6 million cubic yards of benthic marine sediment in Humboldt Bay Harbor	—	no/no	no/no	32
Ogden 1998	San Diego Multiple Habitat Conservation Program for incidental take permits of 85 species	121,023	yes/no	yes/no	33
EGI 2012	140 MW Campo Verde Solar Project	1,990	no/no	yes/no	34
ESA 2012	33 MW Casa Diablo IV Geothermal Development Project	52.5	no/no	yes/no	35
ECORP 2011; Kern County 2012	20 MW FRV Orion Solar Project	260	yes/yes	yes/no	36
Chambers Group 2012	30 MW Imperial Valley Solar Company 2 Project	158.5	no/no	yes/no	37
Glazner Environmental Consulting 1999	Atwood Ranch Unit III Subdivision	55.6	no/no	yes/no	38
City of Sacramento 2011; AES 2011	20 MW Sutter Landing Park Solar Photovoltaic Project	104	no/no	no/no	39
CEC 2007	660 MW Colusa Generating Station, gas-fired power plant	31	yes/no	no/no	40
City of Petaluma 2013	Residential development	59	yes/no	yes/no	41
Kern County & ESA 2012	125 MW Pioneer Green Solar Project	720	no/no	no/no	42
Swaim 2011	Oakland Zoo expansion	62	yes/no	no/no	43
Contra Costa County 2001	Expansion of Temple B'nai Tikyah, Walnut Creek, CA	0.3	no/no	no/no	44
Jones & Stokes Associates 1999	Valley elderberry longhorn beetle HCP for gravel mine in Yolo County, CA	9.9	no/no	no/no	45
City of Anderson 2003	Anderson Marketplace (Walmart) commercial development	26.6	no/no	no/no	46
Sacramento County 2002	The Promenade commercial development	8	no/no	no/no	47
North Fork Associates 2000	Silver Bend Apartments	6	no/no	no/no	48
Foothill Associates 2002	Winters Highlands residential development	102.6	no/no	no/no	49
EIP Associates 2001	UC Merced NCCP/HCCP	2133	yes/no	yes/no	50
URS 2001	UC Merced Long Range Development Plan EIR	2000	yes/no	no/no	51
Beck 2003	Table Mountain mining	50	no/no	no/no	52
Foothill Associates 2004; City of Winters 2004	Callahan Estates residential development	26.4	no/no	no/no	53
CEC 2000	Blythe Energy Project	76	yes/no	no/no	54
Kern County 2013	100 MW Rosamond Solar Project	400	yes/no	yes/no	55
Amold 2013	7.5 MW LANDPRO Solar Project	80.6	no/no	yes/no	56
ESA 2012, Sage Institute, Inc. 2011	Expansion of Metropolitan Air Park, City of San Diego	331	no/no	yes/no	57

APPENDIX 7.B.

References for environmental review documents reviewed for treatment of corridors and habitat fragmentation.

Ref	Reference
1	BLM (Bureau of Land Management). 2012. North Steens 230-kV Transmission Line Project Draft Environmental Impact Statement. DOI-BLM-OR-B060-2010-0035-EIS, Hines Oregon.
2	Kern County. 2012. Draft Environmental Impact Report, SCH# 2012011029, Beacon Photovoltaic Project by Beacon Solar LLC, Conditional Use Permit 11, Map 152, Kern County Planning and Community Development Department, Bakersfield, California.
3	Yolo County. 2008. Clark Precast, LLC's "Sugarland" Project, Initial Study/Mitigated Negative Declaration, Zone File # 2007-078, Yolo County Planning & Public Works Department, Woodland, CA.
4	PBS&J. 2008. Delta Shores Draft Environmental Impact Report. City of Sacramento. Sacramento, CA.

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Appendix 7.B continued

Ref	Reference
5	Sacramento LAFCo (Local Agency Formation Commission). 2011. Draft Environmental Impact Report: City of Elk Grove Proposed Sphere of Influence Amendment (LAFC # 09-10). State Clearinghouse No. 2010092076. City of Elk Grove, Sacramento County, California.
6	BLM (Bureau of Land Management). 2010. California Desert Conservation Area Plan Amendment / Final Environmental Impact Statement for Ivanpah Solar Electric Generating System, FEIS-10-31, Needles, CA.
7	City of Lancaster. 2012. Initial Study for Conditional Use Permits 12-08 and 12-09, Summer Solar and Springtime Solar Projects. Lancaster, CA.
8	HDR (Engineering, Inc.) 2011. Draft Environmental Impact Report Mount Signal and Calexico Solar Farm Projects, Imperial County, California. County of Imperial, El Centro, California.
9	Impact Sciences. 2002. Newhall Ranch November 2000 Administrative Draft EIR. Los Angeles County Department of Regional Planning, Los Angeles, California.
10	Caltrans (California Department of Transportation). 2006. Route 84 Safety Improvement Project Negative Declaration (CEQA). Dividing Border Between Union City and the City of Fremont in Alameda County, California, 04-ALA-84-KP 19.5/21.4, EA 174400. Caltrans, Oakland, California.
10	Caltrans (California Department of Transportation). 2010. Niles Canyon Safety Improvement Project Draft Environmental Impact Report/Environmental Assessment. Alameda County, California.
11	BLM (Bureau of Land Management). 2012a. El Centro Office Draft Environmental Impact Statement / Draft CDCA Plan Amendment Ocotillo Sol Project. US Department of the Interior, BLM/CA/ES-2012-009+1793, DES 20-12, DOI-BLM-CA-D000-2012-0005-EIS. El Centro, California.
12	Foothill Associates. 2006. Biological Resources Assessment: Regional University site and off-site improvements, Placer County, California. Prepared for KT Development. County of Placer, Auburn, California.
13	EDAW. 2008. Recirculated Draft Environmental Impact Report/Supplemental Draft Environmental Impact Statement, Rio del Oro Specific Plan Project, State Clearinghouse #2003122057, City of Rancho Cordova and US Army Corps of Engineers, Sacramento District.
14	Imperial County. 2012. Draft Environmental Impact Report: Solar Gen 2 Solar Array Power Project. Imperial County Planning & Development Services Department, El Centro, California.
15	Design, Community & Environment. 2010. St. John's Church Project Draft Environmental Impact Report, Oakland, California, State Clearing House Number 2008032031.
16	Pacific Municipal Consultants. 2005. Yuba Highlands Specific Plan Draft Environmental Impact Report, State Clearing House Number 2001032070, Yuba County, Marysville, CA.
17	Ecology and Environment, Inc. 2007a. Windy Flats Wind Energy Farm, Klickitat County, Washington: Environmental Report. Prepared for Windy Point Partners, LLC. Portland, Oregon.
17	Ecology and Environment, Inc. 2007b. Windy Flats Wind Farm, Klickitat County, Washington: State Environmental Policy Act Checklist WAC-197-11-960. Prepared for Windy Point Partners, LLC. Portland, Oregon.
18	Antonio Mountain Ranch Specific Plan Public Draft Environmental Impact Report. Placer County Planning and Redevelopment Department, Rocklin, California. State Clearing House Number 2000022053
19	BLM (US Bureau of Land Management). 2012c. Desert Harvest Solar Project Draft Environmental Impact Statement and Draft California Desert Conservation Area Plan Amendment, CACA-49491, DOI Control #: DES 12-17, Publication Index #: BLM/CA/ES-2012-006+1793, US Department of Interior, Bureau of Land Management, Palm Springs Field Office, Palm Springs, California.
20	Ecology and Environment, Inc. 2012. Hudson Ranch Power II CUP #G10-0002/Simbol II CUP #12-0005 Draft Environmental Impact Report, Volume I. County of Imperial, Department of Planning and Development Services, El Centro, CA.
21	WRA Environmental Consultants. 2009. Biological Resources Assessment, 24 Adobe Lane, Orinda, Contra Costa County, California. Prepared for City of Orinda, California.
22	Harzoff, D. 1992. Negative Declaration for the Putah Creek Parkway Demonstration Project. City of Davis, California.
22	Huffman, A. (Huffman & Associates, Inc.) 1992. Evaluation of North Fork of Putah Creek Channel, Davis, California. Report to City of Davis, California.
23	UC Davis. 2003. UC Davis Long-range Development Plan. Office of Resource Management & Planning, University of California, Davis, California
24	Yeast, D. E. 1998. Negative Declaration: Sunset SkyRanch Airport Use Permit. Control #: 97-UPP-0594. County of Sacramento, California.
25	Raney Planning & Management, Inc. 2004. Covell Village (SCH# 2004062089) Draft Program Level Environmental Impact Report. Prepared for City of Davis, California.
26	Planning Consultants Research. 1998. West Bluffs Project Subsequent Draft Environmental Impact Report. Prepared for City of Los Angeles, California.
27	Thomas Reid Associates. 1997. Natomas Basin Habitat Conservation Plan, Sacramento and Sutter Counties, California. City of Sacramento, California.

Appendix 7.B continued

Ref	Reference
28	County of Yolo. 2010. Initial Study/Mitigated Negative Declaration for Results Radio Zone File #2009-001. Woodland, California.
29	Glazner Environmental Consulting. 1999. Biological Resources Assessment for the 55.6-acre Atwood No, 3 Property, Placer County, California. Report prepared for A.,R. Associates, Auburn, California.
30	City of Sierra Madre. 2001. Negative Declaration and Initial Study for Proposed Maranatha High School Impact Reduction Plan. Sierra Madre, California.
31	Edelstein, D. 2003. Analysis of impacts of Creekside Highlands. App. A in Grassetti Environmental Consulting (2004). Initial Study and Draft Mitigated Negative Declaration, Vesting Tentative Map, Tract 7270, Creekside Highlands Project: Submitted to Alameda County Planning Department, Hayward, California.
31	Grassetti Environmental Consulting. 2004. Initial Study and Draft Mitigated Negative Declaration, Vesting Tentative Map; Tract 7270, Creekside Highlands Project. Submitted to Alameda County Planning Department, Hayward, California.
32	Humboldt Bay Harbor Recreation and Conservation District & US Army Corps of Engineers. 1995. Final Environmental Impact Statement / Environmental Impact Report: Humboldt Harbor and Bay Deepening Navigation Project. Eureka, California.
33	Ogden (Ogden Environmental and Energy Services Co., Inc.) 1998. Biological goals, standards, and guidelines for Multiple Habitat Preserve Design. Prepared for San Diego Association of Governments, San Diego, California.
34	EGI. 2012. Final Environmental Impact Report for the Campo Verde Solar Project, SCH. No. 2011111049. Prepared for Imperial County, El Centro, California.
35	ESA (Environmental Science Associates). 2012. Casa Diablo IV Geothermal Development Project, Draft Environmental Impact Statement and Draft Environmental Impact Report. Prepared for Bureau of Land Management, DOI Control #: DES 12-21, Publication Index #: BLM/CA-ES-2013-002+1793, State Clearinghouse No. 2011041008, Bishop Field Office, Bishop, California.
36	ECORP. 2011. Biological Evaluation and Impact Analysis Orion Solar Site, Kern County, CA. Report to SunEdison, San Francisco, California.
36	Kern County. 2012. Draft Environmental Impact Report, SCH# 2012031079, FRV Orion Solar Project (PP12232). Bakersfield, California.
37	Chambers Group, Inc. 2012. Draft Environmental Impact Report, Imperial Valley Solar Company 2 Project, Imperial County, California. County Of Imperial, Planning & Development Services Department, El Centro, California.
39	AES (Analytical Environmental Services). 2011. Biological Resources Assessment, City Of Sacramento 28th Street Solar Photovoltaic Farm. Report to City of Sacramento, California.
39	City of Sacramento. 2011. Community Development Department Solar Photovoltaic Park at 28th Street Landfill Project; Initial Study. Mitigated Negative Declaration, Sacramento, California.
40	CEC (California Energy Commission). 2007. Preliminary Staff Assessment: Colusa Generating Station; Application For Certification (06-AFC-9), Colusa County, CEC-700-2007-003-PSA, Sacramento, CA.
41	City of Petaluma. 2013. Davidon Homes Tentative Subdivision Map and Rezoning Project Draft Environmental Impact Report and Technical Appendices. State Clearinghouse No. 2004072137. City of Petaluma, California.
42	Kern County and ESA (Environmental Science Associates). 2012. Draft Environmental Impact Report, Pioneer Green Solar Project by Pioneer Green Solar II, LLC, SCH# 2012011025, Kern County Planning and Community Development Department, Bakersfield, California.
43	Swaim, K. (Swaim Biological, Inc.). 2011. Oakland Zoo California Project Biological Assessment. Report to East Bay Zoological Society, Oakland, California.
44	Contra Costa County. 2001. Initial Study for the Expansion of the Congregation Temple B'nai Tikyah, Walnut Creek, CA.
45	Jones & Stokes Associates. 1999. Habitat Conservation Plan for the Valley elderberry longhorn beetle for A. Teichert & Sons, Inc., Yolo County, California. Prepared for US Fish and Wildlife Service, Sacramento, California.
46	City of Anderson. 2003. Anderson Marketplace Project Draft Environmental Impact Report. City of Anderson Planning Department, Anderson, California.
47	Sacramento County. 2002. Revised Draft Environmental Impact Report: The Promenade Community Plan Amendment, Rezone, Use Permit, Variances, and Special Review of Parking. State Clearinghouse No. 1998042028. Sacramento County Department of Environmental Review and Assessment, Sacramento, California.
48	North Fork Associates. 2000. Biological Resources Assessment for the ffl6-acre Silver Bend Apartment Project, Placer County, California. Auburn, California.
49	Foothill Associates. 2002. Biological Resources Assessment: Winters Highlands Project Site. City of Winters, California.
50	EIP Associates. 2001. University Community Plan EIR. State Clearinghouse No. 2001021056. County of Merced, California.
51	URS. 2001. UC Merced Long Range Development Plan Draft Environmental Impact Report. Merced County, California.
52	Beck, A. J. 2003. Biological assessment of a portion of North Table Mountain, Central Butte County, California. Unpubl. report by Eco-Analysts, Chico, California.
53	City of Winters. 2004. Initial Study and Negative Declaration for Callahan Estates. City of Winters, California.
54	CEC (California Energy Commission). 2000. Preliminary Staff Assessment: Blythe Energy Power Plant Project, Application for Certification 99-AFC-8, Riverside County, Sacramento, CA.

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Appendix 7.B continued

Ref	Reference
55	Kern County. 2013. Addendum to the Environmental Impact Report for the Rosamond Solar Project, ROSAMOND SOLAR MODIFICATION PROJECT (PP13292), SGS Antelope Valley Development, LLC, Specific Plan Amendment No. 18, Map No. 232, Specific Plan Amendment No. 19, Map 232, Zone Change Case No. 36, Map No. 232, Modification to Conditional Use Permit No. 27, Map No. 232, Streets and Highways—Nonsummary Vacation—Map No. 232. Kern County Planning and Community Development Department, Bakersfield, California.
56	Arnold, R. (RCA Associates). 2012a. General biological resources assessment, LANDPRO 7.5 MW Solar Project, APN0466-181-059, 060, 061 & 062, San Bernardino County, California. Unpublished report to Sunlight Partners, LLC, Mesa, Arizona.
57	ESA. 2012. Metropolitan Airpark Project Draft Environmental Impact Report, SCH No. 201007054, Project No. 208889, City of San Diego Development Services Department, San Diego, California. Sage Institute, Inc. (SII). 2011 (revised by ESA in 2012). Metropolitan Airpark Project Volume 1, 2011 Biology Survey Report, Project No. 208889. Prepared for Metropolitan Airpark, LLC.

LITERATURE CITED

Alberts, A. C., A. D. Richman, D. Tran, R. Sauvajot, C. McCalvin, and D. T. Bolger. 1993. "Effects of Habitat Fragmentation on Native and Exotic Plants in Southern California Coastal Scrub." In *Interface between Ecology and Land Development in California*, edited by J. E. Keeley, 103–10. Southern California Academy of Sciences, Los Angeles.

Beier, P., and S. Loe. 1992. "A Checklist for Evaluating Impacts to Wildlife Movement Corridors." *Wildlife Society Bulletin* 20:434–40.

Beier, P., and R. F. Noss. 1998. "Do Habitat Corridors Provide Connectivity?" *Conservation Biology* 12:1241–52.

Bender, D. J., T. A. Contreras, and L. Fahrig. 1998. "Habitat Loss and Population Decline: A Meta-analysis of the Patch Size Effect." *Ecology* 79:517–33.

Bolger, D. T., A. C. Alberts, R. M. Sauvajot, P. Potenza, C. McCalvin, D. Tran, S. Mazzoni, and M. E. Soule. 1997. "Response of Rodents to Habitat Fragmentation in Coastal Southern California." *Ecological Applications* 7:552–63.

Fisher R. A. 1950. "The Significance of Deviations from Expectation in a Poisson Series." *Biometrics* 6:17–24.

Forman, R. T. T., and M. Godron. 1981. "Patches and Structural Components for a Landscape Ecology." *Bioscience* 31:733–40.

Gilbert-Norton, L., R. Wilson, J. R. Stevens, and K. H. Beard. 2010. "A Meta-analytic Review of Corridor Effectiveness." *Conservation Biology* 24:660–68.

Hall, L. S., P. R. Krausman, and M. L. Morrison. 1997. "The Habitat Concept and a Plea for Standard Terminology." *Wildlife Society Bulletin* 25:173–82.

Hurlbert, S. H. 1984. "Pseudoreplication and the Design of Ecological Field Experiments." *Ecological Monographs* 54:187–211.

Huston, M. A. 2002. *Predicting Species Occurrences: Issues of Scale and Accuracy*, edited by J. M. Scott, P. J. Heglund, M. Morrison, M. Raphael, J. Haufler, and B. Wall. Island Press, Covelo, CA.

Hutto, R. L. 1990. "Measuring the Availability of Food Resources." *Studies in Avian Biology* 13:20–28.

Karr, J. R. 1994. "Landscapes and Management for Ecological Integrity." In *Biodiversity and Landscape: A Paradox for Humanity*, edited by K. C. Kim and R. D. Weaver, 229–51. Cambridge University Press, New York.

Kullback S. 1959. *Information Theory and Statistics*. John Wiley & Sons, New York, NY.

La Polla, V. N., and G. W. Barrett. 1993. "Effects of Corridor Width and Presence on the Population Dynamics of the Meadow Vole Microtus pennsylvanicus." *Landscape Ecology* 8:25–37.

Laurence, W. F., and E. Yensen. 1991. "Predicting the Impacts of Edge Effects in Fragmented Habitats." *Biological Conservation* 55:77–92.

Longcore, J. R., H. Boyd, R. T. Brooks, G. M. Haramis, D. K. McNicol, J. R. Newman, K. A. Smith, and F. Stearns. 1993. "Acidic Depositions: Effects on Wildlife and Habitats." *Wildlife Society Technical Review* 931.

Lovejoy, T. E., R. O. Bierregaard, A. B. Rylands, J. R. Malcom, C. E. Quintela, L. H. Harper, K. S. Brown Jr., A. H. Powell, G. V. N. Powell, N. O. R. Schubart, and M. B Hays. 1986. "Edge and Other Effects of Isolation on Amazonian Forest Fragments." In *Conservation Biology: The Science of Scarcity and Diversity*, edited by M. E. Soule, 257–85. Sinauer Associates, Sunderland, MA.

McCollin, D. 1993. "Avian Distribution Patterns in a Fragmented Wooded Landscape (North Humberside, U.K.): The Role of between-Patch and within-Patch Structure." *Global Ecology and Biogeography Letters* 3:48–62.

Morrison, M. L., and L. S. Hall. 2002. "Standard Terminology: Toward a Common Language to Advance Ecological Understanding and Application." In *Predicting Species Occurrences: Issues of Scale and Accuracy*, edited by J. M. Scott, P. J. Heglund, M. Morrison, M. Raphael, J. Haufler, and B. Wall, 43–52. Island Press, Covelo, CA.

Morrison, M. L., B. G. Marcot, and R. W. Mannan. 1998. *Wildlife-Habitat Relationships: Concepts and Applications*. 2nd edition. University of Wisconsin Press Madison, WI.

Phipps, M. 1981. "Entropy and Community Pattern Analysis." *Journal of Theoretical Biology* 93:253–73.

Porneluzi, P., J. C. Bednarz, L. R. Goodrich, N. Zawada, and J. Hoover. 1993. "Reproductive Performance of Territorial Ovenbirds Occupying Forest Fragments and a Continuous Forest in Pennsylvania." *Conservation Biology* 7:618–22.

Rich, C., and T. Longcore, eds. 2006. *Ecological Consequences of Artificial Night Lighting*. Island Press, Covelo, CA.

Roland, J. 1993. "Large-Scale Forest Fragmentation Increases the Duration of Tent Caterpillar Outbreak." *Oecologia* 93:25–30.

Salwasser, H. 1990. "Conserving Biological Diversity: A Perspective on Scope and Approaches." *Forest Ecology and Management* 35:79–90.

Saunders, D. A., R. J. Hobbs, and C. Margules. 1991. "Biological Consequences of Ecosystem Fragmentation: A Review." *Conservation Biology* 5:18–32.

Scott, T. A. 1993. "Initial Effects of Housing Construction on Woodland Birds along the Wildland Urban Interface." In *Interface between Ecology and Land Development in California*, edited by J. E. Keeley, 181–87. Southern California Academy of Sciences, Los Angeles, CA.

Shannon, C. E., and W. Weaver. 1949. *The Mathematical Theory of Communication*. University of Illinois Press, Urbana, IL.

Simberloff, D., and J. Cox. 1987. "Consequences and Costs of Conservation Corridors." *Conservation Biology* 1:63–71.

Simberloff, D., J. A. Farr, J. Cox, and D. W. Mehlman. 1992. "Movement Corridors: Conservation Bargains or Poor Investments?" *Conservation Biology* 6:493–504.

Smallwood, K. S. 1993. "Understanding Ecological Pattern and Process by Association and Order." *Acta Oecologica* 14:443–62.

———. 1999. "Scale Domains of Abundance among Species of Mammalian Carnivora." *Environmental Conservation* 26:102–11.

———. 2001a. "The Allometry of Density within the Space Used by Populations of Mammalian Carnivores." *Canadian Journal of Zoology* 79:1634–40.

———. 2001b. "Linking Habitat Restoration to Meaningful Units of Animal Demography." *Restoration Ecology* 9:253–61.

———. 2002. "Habitat Models Based on Numerical Comparisons." *Predicting Species Occurrences: Issues of Scale and Accuracy*, edited by J. M. Scott, P. J. Heglund, M. Morrison, M. Raphael, J. Haufler, and B. Wall, 83–95. Island Press, Covelo, CA.

Taylor, L. R. 1961. "Aggregation, Variance and the Mean." *Nature* 189:732–35.

Taylor, R. A. J., and L. R. Taylor. 1979. "A Behavioral Model for the Evolution of Spatial Dynamics." In *Population Dynamics*, edited by R. M. Anderson, B. D. Turner, and L. R. Taylor, 1–28. Blackwell Scientific Publications, Oxford.

Villard, M.-A., M. K. Trzcinski, and G. Merriam. 1999. "Fragmentation Effects on Forest Birds: Relative Influence of Woodland Cover and Configuration on Landscape Occupancy." *Conservation Biology* 13:774–83.

Whitcomb, R. F., C. S. Robbins, J. F. Lynch, B. L. Whitcomb, M. K. Klimkiewicz, and D. Bystrak. 1981. "Effects of Forest Fragmentation on Avifauna of the Eastern Deciduous Forest." In *Forest Island Dynamics in Man-Dominated Landscapes*, edited by R. L. Burgess and D. M. Sharp, 125–205. Springer-Verlag, New York, NY.

Wiens, J. A. 1997. "Metapopulation Dynamics and Landscape Ecology." In *Metapopulation Biology: Ecology, Genetics, and Evolution*, edited by I. A. Hanski and M. E. Gilpin, 43–62. Academic Press, San Diego, CA.

Wilcox, B. A., and D. D. Murphy. 1985. "Conservation Strategy: The Effects of Fragmentation on Extinction." *American Naturalist* 125:879–87.

Wilcox, B. A., K. S. Smallwood, and J. A. Kahn. 2002. "Toward a Forest Capital Index." In *Managing for Healthy Ecosystems*, edited by D. J. Rapport, W. L. Lasley, D. E. Rolston, N. O. Nielsen, C. O. Qualset, and A. B. Damania, 285–98. Lewis Publishers, Boca Raton, FL.

Woodroffe, R., and J. R. Ginsberg. 1998. "Edge Effects and the Extinction of Populations Inside Protected Areas." *Science* 280:2126–28.

Yahner, R. H. 1996. "Habitat Fragmentation and Habitat Loss." *Wildlife Society Bulletin* 24:592.

Zembal, R. 1993. "The Need for Corridors between Coastal Wetlands and Uplands in Southern California." In *Interface between Ecology and Land Development in California*, edited by J. E. Keeley, 205–8. Southern California Academy of Sciences, Los Angeles, CA.

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Concepts, Challenges, and Solutions

EDITED BY
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CHAPTER

6

Habitat Models Based on Numerical Comparisons

K. Shawn Smallwood

Habitat Model Types and Analysis

Habitat analysis typically involves either a comparison of used versus not used environmental elements or a test for disproportionate *use* of environmental elements from a measured set of *available* environmental elements. This chapter will focus on the latter approach (Table 6.1). The measure of use can include numerical spatial patterns (distributions) based on an individual's locations or on collections of individuals (Fig. 6.1). Using estimates of density or the number of individuals to perform habitat analysis also involves the use of pattern analysis, because density and spatial distribution are interlinked (Taylor 1961; Taylor et al. 1978). If organisms were patterned uniformly or randomly across a landscape or region, then habitat analysis would not be informative. The species would be considered as simply ubiquitous. However, species do not distribute themselves this way. Numbers vary spatially, and implicit in most habitat models is the assumption that numerical peaks coincide with locations where habitat is of the highest quality (but see Van Horne 1983 for warnings against this assumption).

Using densities or numbers of individuals for habitat analysis, the measurement units for the response side of the analysis can range in demographic organization from subpopulations and populations to megapopulations (sensu Garshelis and Visser 1997) and metapopulations (sensu Hanski and Gilpin 1997). These response units are measured on geographic areas that encompass at least several home ranges to regions (Fig. 6.1). Many habitat models include the term *N* to represent number of individuals, *D* to represent density, or some other term representing number or spatial intensity in the environment. The terms *N* and *D* are explicitly represented in some habitat models (e.g., Table 6.1, Equations 6.1, 6.5, 6.11, 6.12, 6.13) and are needed for frequency counts and ratios used in the other models. Therefore, habitat analysis is scale-dependent because *N* and *D* are scale-dependent (Verner 1981; Blackburn and Gaston 1996; Smallwood and Schonewald 1996). Furthermore, these models do not specify which demographic unit is being represented by *N* or *D*, nor the contexts of demography, season, interannual variability, or condition of the landscape, all of which can affect *N* or *D* (Cyr 1997; Smallwood in press).

Habitat models using numerical comparisons have been reviewed already (e.g., Morrison et al. 1998). Van Horne (1983) pointed out that density is a misleading indicator of habitat quality, mainly because density estimates are frequently too narrow in their representation of a population's success. According to Van Horne (1983), habitat studies are often spatially and temporally inadequate for including areas occupied during all seasons, nor do they include the full

TABLE 6.1.

Habitat models based on numbers or densities.

Equation Number and Model Name	Model Structure	Explanation
Measures of effect, no P-value		
6.1. Edge index (Helle and Jarvinen 1986)	$E = \frac{D_i}{D_j} * 100\%$	D_i and D_j are densities at sites i and j
6.2. Index of selection (Paloheimo 1979)	$S = \frac{r_i}{p_i}$	r_i is the percentage of the ith element used, and p_i is the percentage available in the measured set
6.3. Index of electivity (Ivlev 1961; Jacobs 1974; Gordon 1989)	$E = \frac{r_i - p_i}{r_i + p_i}$	
6.4. Usage-availability rank difference (Johnson 1980)	$t_{ij} = rank\ r_i - rank\ p_i$	
Hypothesis tests with P-values		
6.5. Isodar Theory of habitat selection (Morris 1990)	$D_1 = \frac{a_1 - a_2}{b_1} + \frac{b_2}{b_1} * D_2$	in the ith habitat, a_i = maximum fitness, b_i = per capita reduction in fitness
6.6. χ^2 test with Bonferoni Z test (Pearson 1900; Neu et al. 1974)	$\chi^2 = \sum \frac{(O-E)^2}{E}$	O = observed, and $E = p_i * N$ = expected number of observations
6.7. Log-likelihood ratio criterion (Fisher 1924, 1950; Everitt 1977)	$\chi^2 = 2\sum O * \log_e \frac{O}{E}$	
6.8. Measure of aggregation (David and Moore 1954; Pielou 1977)	$\chi^2 = \left(\frac{1}{n\bar{x}}\right)\sum_{i=1}^{n}(x_i - \bar{x})^2$	x_i = number of occurrences in ith element; x = mean of occurrences among n elements
6.9. Ecological order (Smallwood 1993)	$M_i = \frac{O}{E}$	M_i = the number observed as a multiple of the number expected in the ith element
6.10. Negentropy (Fisher 1924; Shannon and Weaver 1949; Rothstein 1951; Phipps 1981)	$H' = \sum_{i=1}^{s} O \log_e \frac{O}{E}$	Use of information; use of the measured set of choices; deviation from uniform association indicates negative entropy
6.11. Surface regression model (Rotenberry 1986)	$D = b_0 + \sum_{i}^{h} b_i X_i + \sum_{i}^{h} b_{ii} X_i^2 + \sum_{i}^{h}\sum_{j}^{h} b_{ij} X_i X_j$	X_i = the ith habitat, h is the number of habitat variables, and bs are fitted coefficients
6.12. Correlation (Wiens and Rotenberry 1981)	$d = ah_i$	In a strip transect, h_i is the ith habitat attribute expressed as a continuous variable, e.g., percent cover
6.13. Patch shape/Type (Otis 1998)	$\ln\{E(D_{ij})\} = \beta_0 + \ln(A_{ij} + \beta_1(S_{ij}) + \sum \beta_i P_{ijk}$	A_{ij} = area of the jth patch in the ith environmental element; S = shape, which is a function of the patch perimeter and its area; P = patch perimeter

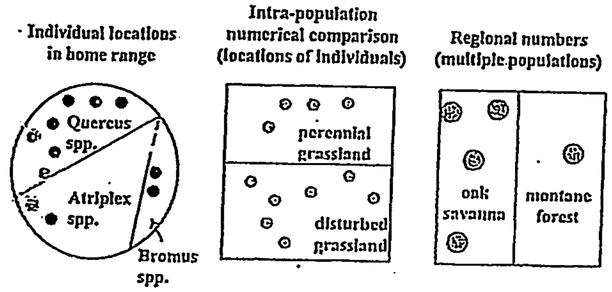

Figure 6.1. Numerical data in habitat models can include individual locations within a home range or any number of individuals with or without reference to a demographic unit. The grain of the measured set of environmental elements typically grades from micro- to macro-habitat representation as the numerical data grades from individual locations to regional in scope.

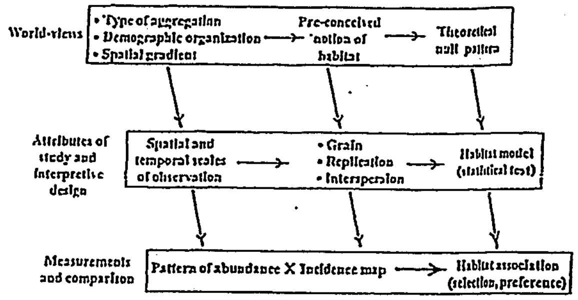

Figure 6.2. The effectiveness of numerically based habitat models and the measurements of use and availability ultimately depend on the attributes of the field studies and the world-views of the analyst, which have primacy over the actual measurements of use and availability.

range of numerical responses to environmental changes, nor do they address the social interactions leading to occupation of both source and sink areas. Vickery et al. (1992) found density to be a poor predictor of nest success among three species of passerine bird species, although they did not account for the effect their variable plot sizes had on density (Smallwood 1999). Nevertheless, unburdened by any possible effect of variable plot sizes on density, Morris (1989) found mean litter size of the white-footed mouse (*Peromyscus leucopus*) to decline with increasing density. Alldredge et al. (1998) discussed faulty assumptions underlying various habitat models, such as the assumption that the sample of animals is random (rather than belonging or not belonging to a population), that each observation is independent of the next, that each animal's habitat selection was independent of others in the sample, that the availability of environmental elements is known rather than estimated, that availability is constant over the period of the study (rather than changing seasonally or for other reasons), and that detectability of animals is constant among the environmental elements sampled. Smallwood (1993) began a discussion on the theoretical foundations of these models, focusing on the role of thermodynamics in resource selection models.

In this chapter, I will frame habitat analysis within the larger context of world-views on how and why animals distribute themselves and how and why analysts apply experimental design principles and statistical tests to habitat studies based on relative number or density. Figure 6.2 depicts a hierarchical framework for discussing habitat analysis, first considering world-views (paradigms), then attributes of study and interpretive design, and finally the measurements and comparisons, the results and interpretation of which depend on world-views, and study and interpretive design. I intend to demonstrate that the available habitat models are inadequate by themselves for characterizing habitat of animal species; these models are no substitute for long-term research experience, although their predictions can be used as testable hypotheses. Just as habitat analysts need operational terms with which to work (Morrison and Hall, Chapter 2), they also need a larger framework with which to interpret applications of the habitat models based on numerical comparisons.

The Theoretical Null Pattern

The significance of a measured numerical pattern (spatial distribution) to habitat use is decided largely by its comparison to a theoretical null pattern. Measured numerical patterns can be uniform, random, aggregated, or regular, and null patterns can be uniform, random, or both (Fig. 6.3). Deviation of a measured pattern from the null pattern is assumed to be caused by a relationship between the species and the energy or material resources composing the environmental elements (Smallwood 1993). The theoretical foundations of the null pattern are therefore critical for interpreting the association. The null pattern in thermodynamics is uniformity, which connotes the equilibrium cold state, or

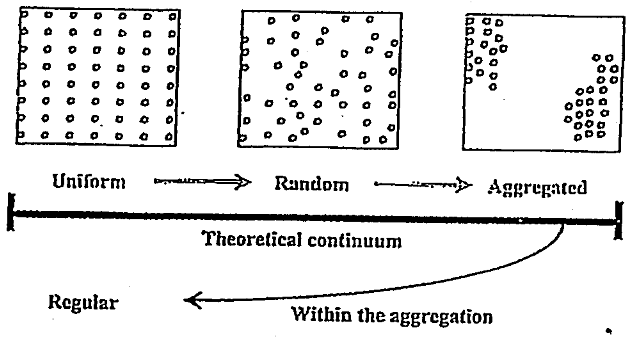

Figure 6.3. Random patterns occur somewhere along a gradient ranging from uniformity (regularity) to aggregation, thereby reducing the distance that can be measured between an observed aggregated pattern and the null pattern of randomness.

lack of energy relationships (Hutchinson 1953). The null pattern in information theory is also uniformity, which connotes no use of the available information (Shannon and Weaver 1949; Kullback 1959; Phipps 1981). Uniformity is the state of maximum entropy in both thermodynamics and information theory. The null pattern in ecology is randomness, which connotes lack of influence from the locations of other individuals (Fisher 1950; Taylor 1961). The null pattern chosen by the habitat analyst identifies the world-view of the analyst regarding how and why nature is organized, and it determines the set of alternative results.

Habitat analysts usually explicitly or implicitly assume that spatial distributions are determined by some form of energy relationship(s) between the organisms and their environment (Hall et al. 1997b; Morrison et al. 1998). Although rarely discussed explicitly, many ecologists hold the world-view that biological processes are governed ultimately by the laws of thermodynamics (Hutchinson 1953). The measures of effect used in Equations 6.6–6.10 are derived from these laws, and all the other models in Table 6.1 (except Equation. 6.5) are theoretically and mathematically related to Equations 6.6–6.10 (Smallwood 1993). Many aspects of habitat are assumed to be energy-related, and the goals of wildlife are usually assumed to be resource acquisition and total fitness (Southwood 1977; Rosenzweig 1985; Wiens 1989b,c), which are energy-related and energy-dependent, respectively. Habitat elements other than energy are recognized as important, such as water, nutrients, refugia, travel corridors, nest sites, and so on. However, even the response variable, density, has its conceptual origins in the physical sciences and is energy-dependent. High-quality habitat is said to have greater levels of energy available (Hall et al. 1997b).

However, if the laws of thermodynamics largely govern the spatial distributions of animals, then assuming randomness as the null pattern is not only inappropriate on theoretical grounds but also reduces the analyst's capacity to recognize meaningful aggregations and selection of environmental elements (Smallwood 1993). Random patterns occur somewhere along a gradient that ranges from uniformity (regularity) to aggregation (Fig. 6.3). This gradient is represented by probability values of the χ^2 distribution, the lower-tail values of which correspond with uniformity and increasingly upper-tail values grading through randomness and eventually aggregated patterns. The distance that can be measured between an observed aggregated pattern and the null pattern of randomness is less than between the aggregated pattern and the null pattern of uniformity. Therefore, assuming the null pattern is random instead of uniform washes out some of the potential significance that can be attributed to an observed aggregated pattern. The world-view of the analyst is at least implicitly expressed by the types of habitat model and statistical test used, because these models and tests are structured either on the assumption that the null pattern is uniform (e.g., χ^2 tests) or random (e.g., ANOVA tests).

Only aggregated patterns of animals can lead to inferences about habitat use. Purely uniform or random patterns of use will fail to reveal any selection of environmental elements from a measured set because no disproportional patterns will be evident (Fig. 6.4). A regular pattern of distribution might indicate that the study area included territories of individuals numbering fewer than occurred in the larger population (Fig. 6.5). In other words, the study area was smaller than the area occupied by the population, which often arranges itself into regularly spaced home ranges or territories held by its members. Similarly, random patterns have been observed when progeny (of insects) dispersed passively in air or water (Taylor et al. 1978), or when occurrence was scarce or at the edge of a larger aggregation (Fig. 6.5).

Recognizing whether the spatial pattern is uniform, random, aggregated, or regular depends on both the

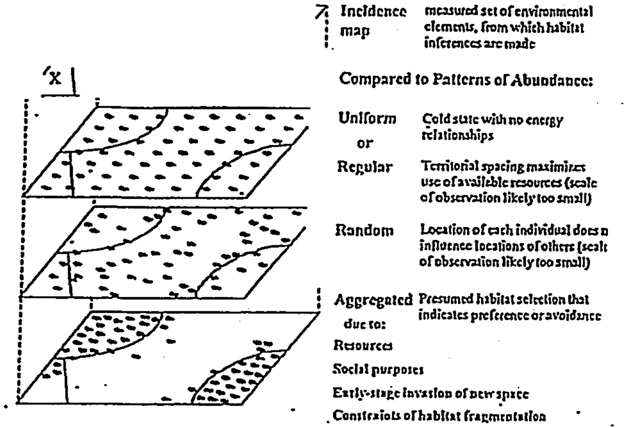

Figure 6.4. To interpret meaningful associations, patterns of distribution are compared to a map of the measured incidence of environmental elements, composing a measured set. The meaning of an association ultimately depends on the world-view of the analyst and on the theoretical foundations of the statistical test or measure of effect being used. It also depends on the type of aggregations that are being compared to the measured set of available environmental elements.

spatial scale of observation and knowledge of the species' demographic organization. Examining the spatial relations of individuals from a portion of a population can mislead an analyst into thinking the pattern of distribution is random or uniform, when it is more likely regular at the scale appropriate for measuring the population and aggregated at the regional scale. For example, prairie dog (*Cynomys* spp.) burrows occur fairly regularly within colonies (Tileston and Lechleitner 1966), which are themselves aggregations in the region (Koford 1958). Of course, information about the spatial distribution is lost when transforming the locations of individuals into numerical estimates and then to densities. Comparing *N* or *D* for habitat analysis cannot lead to conclusions about demographic organization unless the spatial areas are included in the comparison (Smallwood 1999).

Types of Aggregation

Aggregations can form for several reasons (Fig. 6.6), and each of these reasons poses a unique implication for interpreting the habitat association. These reasons bear on the choice of the theoretical null pattern and the habitat model. It will be important for wildlife biologists to determine the relative occurrence frequencies of each of the following four types of aggregation: resource, demographic, early-stage, and constrained.

Individuals may aggregate around a centralized or patchy resource, forming a *resource aggregation*. Competition for a limited resource can force some animals to live on the fringe of the resource patch, or even to spill over into ecological sinks. Such an aggregation can extend beyond the boundary of the resource patch just because off-patch individuals get sufficient access to the resource or because those exploiting the resource generate progeny that disperse

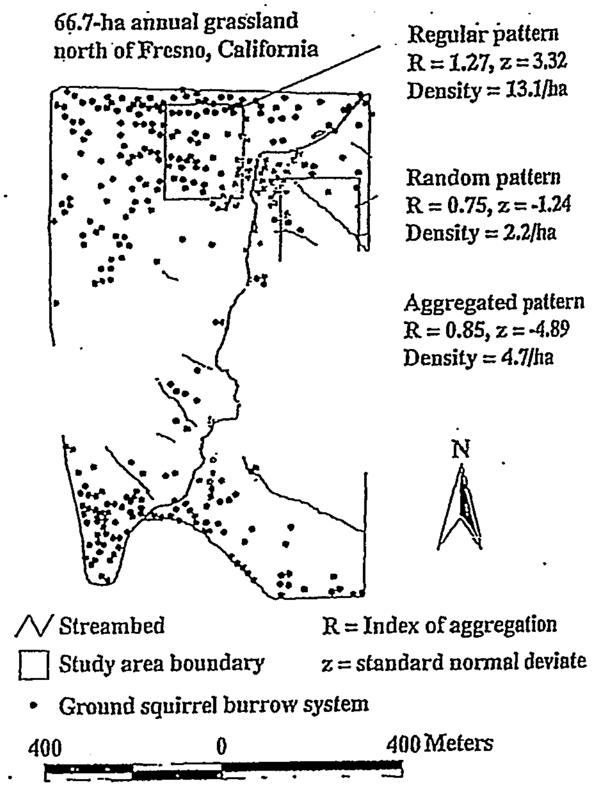

Figure 6.5. The size and position of study areas inform of different spatial patterns among burrow systems of California ground squirrels (*Spermophilus beecheyi*). Using the pacing method for mapping burrow systems of pocket gophers (Smallwood and Erickson 1995), I mapped the approximate centers of ground squirrel burrow systems on 66.7 hectares of annual grassland in the low-elevation foothills north of Fresno, California, during April and May 2000. A nearest-neighbor-distance method (Clark and Evans 1954) indicated that ground squirrel burrow systems were regularly distributed within a larger aggregation and randomly distributed at the edge of the aggregation. The larger aggregation was only recognizable within the boundary of the largest (66.6 hectares) study area. The density of burrow systems was also greater within the aggregation compared to the edge.

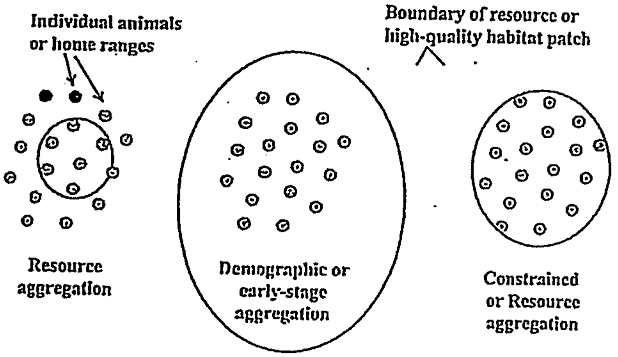

Figure 6.6. The types of aggregations observed largely determine the pattern of distribution and the association with the measured set of environmental elements. Demographic, resource patchiness, habitat fragmentation, and early-stage filling of ecological space can affect our perception of use and availability.

outward, beyond the patch. In either case, and in the case of other possible explanations, the number of individuals associated with the resource-based aggregation need not be bound within the resource patch.

Individuals also aggregate with conspecifics to improve fitness through reproduction, rearing of young, and group cooperation such as foraging and predator avoidance. They may choose places to live based on the momentum of the congregation rather than the energetic or nutrient accessibility of the habitat patch (Alatalo et al. 1985). *Demographic aggregations* may have spatial limits that are imposed by behaviors or by uncertainties of habitat quality outside the aggregation (Stamps 1991). Demographic aggregations conceivably could be confined to space that is smaller in extent than the resource patch (Taylor and Taylor 1979). Social constraints could therefore limit our observed use of an environmental element (resource) to less than that expected based on its availability. Dasgupta and Alldredge (1998) devised a behavioral dependency parameter for use with χ^2 tests, involving situations where multiple individuals are observed together. However, aggregations can occur without individuals being observed together, per se (e.g., *Puma concolor*, Smallwood 1997), but which may have formed to serve social and demographic needs nevertheless (Lloyd 1967).

An early-stage aggregation can also form while immigrants invade previously unoccupied habitat, which could have been cleared of former occupants, or which was recently discovered or became available to the species. An invasion of pocket gophers (*Thomomys* spp.) into new stands of alfalfa (Smallwood and Geng 1997) is a good example. Early-stage aggregations are combined resource and demographic aggregations but temporarily give different results when analyzing habitat selection based on use and availability methods. Such aggregations provide the best opportunity to observe preference of the available environmental elements because they are relatively free of demographic constraints (e.g., competition and territoriality). However, as numbers increase due to continued immigration, social mechanisms such as territoriality force later arrivals into less-preferred locations, thereby affecting our perception of use (Fig. 6.7). An example of early-stage aggregations can be found in "Intrapopulation Numbers," later in this chapter.

Constrained aggregations result from habitat fragmentation (sensu Wilcox and Murphy 1985) or the division of previously contiguous habitat patches (Addicott et al. 1987). They may also result from intolerable environmental conditions occurring naturally outside the aggregation, including the occurrence of competitors or predators (Hutchinson 1953; Koford 1958). Patches of low-quality habitat are occupied by constrained aggregations because the species is left with no other place to go (du Toit 1995). Low-quality habitat can appear to be high-quality habitat when loss of habitat constrains individuals to fragmented habitat patches or to peripheral habitat areas. Habitat analysis using constrained aggregations can be misleading due to packing of individuals into the habitat fragment or the inclusion of the intolerable areas in the measured set of environmental elements. Unfortunately, many special-status rare species are found only in fragmented habitat, or in habitat at the boundary of their tolerable conditions (e.g., Scott et al. 1986), so their constrained aggregations should not be used alone to interpret habitat associations. For example, giant garter snakes (*Thamnophis gigas*, federally endangered) occur in marshes and adjacent irrigation canals where marshes in the Central Valley of California have not been converted entirely to agricultural fields and houses. The study of historical data and taxonomically and functionally similar species might help investigators interpret habitat associations for special-

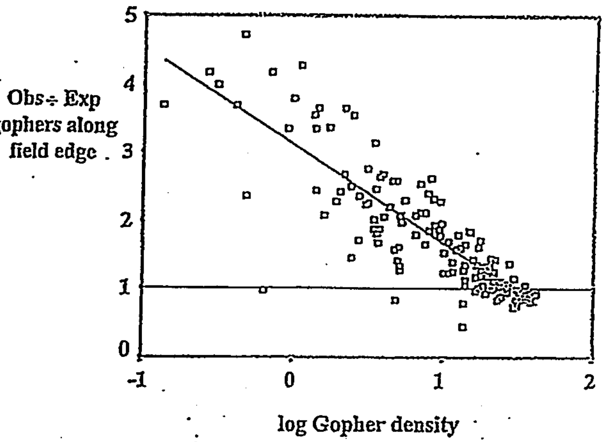

Figure 6.7. The relationship between selection for the field edge and pocket gopher (*Thomomys bottae*) density across alfalfa fields (*Medicago sativa*). The observed value divided by the expected value of 1.0 indicates the edge was used in proportion to its availability.

status rare species. For example, habitat descriptions of *T. sirtalis*, *T. couchi*, and *T. elegans* might provide insight into the historical habitat of *T. gigas*.

The prevailing view among those using the models in Table 6.1 appears to be that resource aggregations result in our measurements of disproportionate use of environmental elements. For example, Rotenberry et al. (Chapter 22) present a habitat selection model for which they assume that most wildlife observations are made where these animals want to be—where they derive some benefit from resources. However, the models in Table 6.1, and that of Rotenberry et al., can yield the same measures of disproportionate use when applied to aggregations that were forced by habitat fragmentation or the need to congregate. Applied to *T. gigas*, these models will indicate that *T. gigas* selects irrigation canals, probably because the landscape matrix is annual field crops where the snake cannot live. The models in Table 6.1 will not discriminate among aggregations influenced by various disparate factors, so they cannot be relied upon to measure habitat quality.

Demographic Organization

The demographic units represented by the numbers compared also bear on habitat analyses (Van Horne 1983). Whether or not demographic organization can be related to use of environmental elements, it affects the pattern of distribution observed. I recently discovered a range in size of study areas in which the number of individuals might be given some meaning in terms of demographic organization (Smallwood 1999). As I had also found for mammalian carnivora (Smallwood 1999), nearly half the numerical estimates of northern goshawk (*Accipiter gentilis*) increased proportionally with increases in the sizes of their corresponding study areas (scale domain A, Fig. 6.8). Given that species of carnivora and northern goshawk typically space themselves at fairly even distances due to territory maintenance, I interpreted the aforementioned pattern as an indication that these estimates were made at areas smaller than those occupied by the "population." Each species in Smallwood (1999) appeared to have what I termed a *threshold area*, at which the number of individuals ceased to increase proportionally with increasing study area size (as in Fig. 6.8). The threshold area was the low end of a spatial scale domain (B), in which the number of individuals varied considerably but did not regress on study area size with a slope significantly different from zero. I interpreted the numerical estimates within scale domain B to represent populations because no other demographic units have been defined for the clusters of twenty-five to sixty adults that are typical of this scale domain (Smallwood 1999).

Regardless of whether the estimates in scale domain B represented distinct populations, the theoretical foundation of animal ecology includes the population as a key demographic unit with functional, goal-directed significance. Odum (1959) and Dasmann (1981) defined a population as some collection of organisms of the same species occupying a particular space and sharing a suite of attributes representing a unique organizational structure. Yet our use of N (number of individuals) and D (density) in habitat and other analyses is usually given no meaning with respect to the population concept. Usually, no demographic unit is attributed to N or D in habitat models. In conducting habitat analysis using a measured set of environmental elements, what does it mean to compare three individuals in element X to fifty in element Y and to four hundred in element Z, when the three individuals are a small portion of a population, the fifty compose an entire population, and the four hundred are from six populations? The three in element X

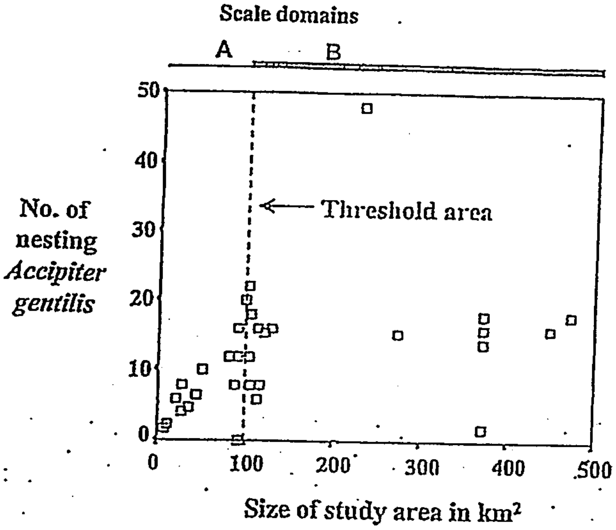

Figure 6.8. Based on the published estimates (summarized in Smallwood 1998), the number of nesting pairs of northern goshawks (*Accipiter gentilis*) increases proportionally with increasing size of study area until the study area is at least 100 square kilometers (scale domain A). Study areas larger than 100 square kilometers include aggregations that no longer increase with increasing study area size (scale domain B).

may occur in an ecological sink after having been forced there by the fifty, which themselves occur in element Y out of proportion to Y's availability but well within the space needed to support a population.

That the number of individuals in spatial scale domain B did not correlate with study area size also revealed a mathematical artifact in measuring animal density (Smallwood 1999). The number of individuals in domain B is relatively similar in magnitude to those in larger-area domains (see Smallwood 1999). Regressing density on its corresponding study area size will force a slope of –1 when density was calculated by dividing a constant number by a variable area. Similarly, regressing density on its corresponding study area size will force a slope of 0 when density was calculated using the numbers that increased proportionally with study area size, such as those in scale domain A and those forming the transitions between scale domains B and larger. Therefore, density appears to be a continuous variable only in the absence of demographic organization, and its discontinuity can confound habitat analyses based on the models in Table 6.1.

Spatial scale (extent) domains of distribution provide habitat analysts with a means to attribute social meaning to the frequency counts used in the habitat models. These domains revealed a possible demographic context against which frequency data can be compared. I lack the directed field research evidence needed to conclude that the numerical estimates in spatial scale domain B were distinct populations. However, the population concept can still be used to discuss the intent of habitat analysts and the meaning of their results.

Home Range

Habitat analysis within the individual's home range fails to reveal the significance of the location of the home range. Differential use of areas within the individual's home range is interesting at the microhabitat level, but the location of the home range itself can be influenced by the location of the population and the social and demographic status of the individual (Van Horne 1983). An individual's home range can encompass low-quality habitat simply because it was forced to live there in order to participate as a member of the population (Stamps 1991). Leger et al. (1983) found that California ground squirrels (*Spermophilus beecheyi*) adjust their behaviors to access microhabitats that would otherwise pose increased risks of predation. Animals do not always go to a place because it provides the highest-quality habitat, but once at the place, they make the best of it. Reports of habitat studies based on telemetry locations of individuals often provide no information of the demographic status of the individual or of the individual's spatial relationship to the population.

Intrapopulation Numbers

Comparing numerical estimates made at the subpopulation level (scale domain A) can confound the habitat analysis with the proportional relationship between the estimated number of individuals and study area size (Smallwood 1999), unless study sites (plots) were chosen randomly or systematically from a region, used and unused sites were compared, or long-term observations were used. Habitat analysis at this numerical level risks observing proportional patterns of use simply because individuals or their home ranges are regularly distributed among microhabitat elements due to territoriality or other social interactions that partitioned the resources at this level. Habitat analysis at this level of organization may or may not contribute useful inference regarding habitat, as social interactions are driving the spatial distribution within a larger area of habitat. Unless home range size can be related to habitat quality, habitat identified from a home range or from within the bounds of a population was pseudoreplicated (Hurlbert 1984; Aebischer et al. 1993).

As an illustration of apparent habitat use based on intra-population numbers, I compared a measure of resource selection (Equation 6.9 in Table 6.1) to Botta's pocket gopher (*Thomomys bottae*) density to test whether measured selection for the edge of alfalfa (*Medicago sativa* L.) fields was density-dependent:

$$\frac{\textit{Observed gophers at edge}}{\textit{Expected gophers at edge}} = \frac{\textit{gophers along edge}}{\textit{gophers in field} \cdot \left(\dfrac{\textit{ha along edge}}{\textit{ha of field}} \right)}$$

The density estimates were from 134 counts of burrows across thirty-seven alfalfa fields in Yolo County, California, during 1992 through 1994 (Smallwood and Geng 1997).

At low density, gophers occurred along the field edge at nearly five times the number expected by chance (Fig. 6.7). However, increasing density reduced my ability to quantitatively represent gopher selection for the field edge ($r^2 = 0.71$, Root MSE = 0.49, $P < 0.001$):

$$\frac{\textit{Observed gophers at edge}}{\textit{Expected gophers at edge}} = 3.09 - 1.43 \log \textit{Density}$$

The obvious preference for the edge grew increasingly hidden as gophers invaded the interior of the field and approached a regular distribution due to saturation of territorial space, represented by a ratio of 1.0 between the observed and expected gophers at the field edge. I do not think that gophers favored the edge any less as density increased, but rather their preference for the edge grew increasingly less recognizable as the overall density in the field increased (Fig. 6.7). As gophers saturated the field interior, the clustering along the edge became hidden, just as Hansen and Remmenga (1961) found a clustered gopher distribution to vanish as the increasing density shifted the gopher population to a regular distribution. This density-dependence of distribution was recognized before (Taylor et al. 1978), but the density-dependence of measuring selection preference or avoidance will require habitat analysts to reconsider current reliance on measures and statistical tests based on use and availability of resources. Measured selection of environmental elements will be density- and therefore area- and time-dependent (also see Smallwood 1995), just as information theoretical measures (e.g., H', the composite index of species richness, $\log_2 S$ and evenness, J) are dependent on sample size (Cousins 1977) or on the spatial scale at which these measurements are derived.

This density- or scale-dependence also bears on the timing of habitat analysis. Measures of resource use will vary depending on the stage of ecological succession of a site or the stage at which a site is being colonized. Preference can be clearly observed during the early stages of colonization, but during the later stages the preferred resources can be hidden by overflow of individuals into relatively less-preferred conditions.

Interpopulation Numbers

Comparing numbers (densities) between populations poses two potential obstacles to recognizing true patterns of habitat association. The spatial shifting of aggregations summarized by Taylor and Taylor (1979) involves clustering at a subset of the available high-quality habitat patches. Taylor and Taylor (1979) proposed four hypotheses to explain the frequently observed spatial shifting of aggregations: (1) populations must move once they deplete their most limiting resources; (2) population members shift locations innately so as to prevent the exhaustion of resources; (3) dispersal and territory establishment of the next generation also establishes the location of the next aggregation, while the previous aggregation senesces; and, (4) a combination of hypotheses 1–3. Occupied habitat patches are either not different from many of those that are unoccupied at the time of the analysis or they are ephemeral in their quality. In the latter case, the results of habitat analysis are ephemeral. In the former case, habitat analysis might measure use as less than availability, and a strict adherence to the numerical comparison might mislead the investigator to conclude that the occupied habitat patches are less preferred.

Today's absence at a site can be tomorrow's presence, as the spatial distribution is unique for each generation (Taylor and Taylor 1979). Habitat measured at time t_1 can represent the species' habitat at times t_2 and t_3 only if the same generation and the same environmental conditions span times t_1 through t_3. If the species shifted locations between generations, as is common according to Taylor and Taylor (1979) and den Boer (1981), or if environmental conditions changed between times t_1, t_2, and t_3, then the analysis at time t_1 will be inadequate and misleading if the goal is to describe the species' habitat; it will contribute to a belief that the species relies on a narrower range of environmental conditions than it actually does. The exception will be constrained aggregations, which will be at the same locations at all times, so long as none of the aggregations go extinct. However, habitat analyses using constrained aggregations are predisposed to mislead simply due to the constraints on availability of suitable environmental elements.

An additional obstacle is the meaning we attach to numerical variations of each population. Population number can vary greatly, with increases and decreases lagging behind changes in conditions of resources. Interannual variability in N typically cannot be characterized adequately until monitoring has been conducted spanning multiple generations (Cyr 1997). Therefore, when comparing N or D from population X to that of population Y, and N or D differs, can we really conclude that habitat quality differs between the sites occupied by populations X and Y?

Presumably, sociality and energy and material resources maintain a carrying capacity just below which exists an optimal density. Within the spatial bounds of a population, density can vary somewhat but must be constrained by territoriality or the habitat element in most limited supply. Comparing locations occupied by populations, density might be nearly the same within the bounds of each population (Smallwood 1999). Comparing density within the spatial bounds of a population or between high-density populations is unlikely to reveal meaningful differences and is therefore relatively uninteresting. In other words, density needs to vary considerably to reveal possible differences in habitat quality, but intrapopulation regularity of distribution and mutually high-density populations will not vary sufficiently in N or D to provide inference regarding habitat selection.

The terms N and D in habitat models can be representative of whole populations, and the occurrence frequency or spatial areas occupied by these populations compared to the measured set of environmental elements within a region or the species' range. The N or D representing a whole population can simply be replaced by a 1 to denote presence or a 0 to denote absence, and little information would be lost so long as the populations are discrete and easily bounded. Representing whole populations, presence or absence might be just as informative as N or D. If this step were taken, then the boundary of the area that is occupied by the population would need to be identified. The area occupied by the population might be more reliable as a measure of use than either N or D.

Megapopulation or Regional Numbers

Demographic organization is again implicitly ignored by habitat models that compare densities to measure use and availability of elements between regions. Regions are usually large-enough areas to include multiple populations. Garshelis and Visser (1997) termed the collective abundance from regions as megapopulations because they did not know what else to call it. Metapopulations, on the other hand, theoretically organize within regions (Hanski and Gilpin 1997), but empirical evidence is lacking for knowing the bounds of a metapopulation or how many populations might compose a metapopulation. Regional patterns of distribution are still largely theoretical, and the term megapopulation indicates that this theory is weak.

Comparing densities or use of regions poses the additional problem of comparing the availability of disparate measured sets of environmental elements (Wiens and Rotenberry 1981b). For example, Smallwood and Fitzhugh (1995) compared the number of puma track sets to represent use of available vegetation complexes and topographic categories across all the sampled areas of California, which included areas spanning the full north-south and east-west extents of the state within the species' range. The topographic categories were common across the state, consisting of ridges, mountain peaks, basins, canyons, and so on. These macrohabitat categories were fairly comparable across California, much like Anderson's (1981) use of vegetation structural elements across the United States. However, sage-juniper forests are limited to north-central and eastern California, and chaparrals occur more to the west; they are not interspersed. Comparing use to availability of vegetation categories seemed uninformative on a statewide level, especially when puma populations occurred in both sage-juniper and chaparral. Knowing the range of environments used by pumas in California is useful, and knowing that pumas are more rare in the Mojave Desert than in the Klamath Mountains is also useful, but habitat analysis involving use and availability comparisons is probably best conducted within regions, within which key elements are more likely to be naturally interspersed.

Gradient of Abundance

Animal density is often represented with contours, which illustrate relatively smooth gradations in density from high to low across landscapes or regions (e.g., Taylor and Taylor 1977; Wiens and Rotenberry 1981b; Cody 1985a,b; Scott et al. 1986; Root 1988a,b,c; Price et al. 1995; Morrison et al. 1998). These gradients are mathematically derived using averaging and interpolation, whereas the actual spatial distribution may not be smoothly graded. This transformation of field observations into density contours expresses a world-view of distribution that acknowledges aggregation as the norm for animal species but also facilitates the potentially erroneous idea that animal density corresponds to habitat quality. Habitat quality is assumed to be energy-related, as discussed previously, so it is related to the world-view that the laws of thermodynamics govern the spatial patterning of animal species, also previously discussed. However, this world-view, that density grades smoothly across the landscape, can be supported by few examples, just as sharply bounded aggregations can be supported by few examples (Morrison et al. 1998). It may be that spatial distributions of animals can be better represented by categories, including population occurrence, trace activity (i.e., a few individuals in a large area), and absence. Lidicker (1995) also summarized categorizations of habitat quality, which were thought to bear directly on densities. Habitat analysts need to test whether animal aggregations are discrete or graded, whether population boundaries can be identified, and whether densities are categorical in correspondence with habitat quality. Empirical evidence should be the foundation of theory, and theory that bears on gradients of abundance also bears on habitat analysis.

It is well documented that individuals can occur in what are regarded as ecological sinks for the species. In fact, numerical estimates in ecological sinks can always be greater than zero just because dispersing animals are forced into the sink where the rates of recruitment outpace mortality. Habitat models based on numerical comparison will thus give ecological sinks some positive habitat value in such cases, whereas the true value should be negative (less than zero) with respect to the functionality of the population. Density estimates alone cannot inform of sink conditions (Lidicker 1975, Van Horne 1983), because density estimates cannot be negative—they range from zero to some presumed carrying capacity. Learning of sink conditions requires intensive study, making use of more information than numerical estimates (e.g., Morris 1989).

Preconception of Habitat

The map of available environmental elements is made by the habitat analyst. Some a priori notion of habitat inevitably goes into construction of the incidence map and will affect the typology of the map and its spatial grain (Austin, Chapter 5). For example, Smallwood and Fitzhugh (1995) used typical home range size of puma to decide on the quadrat size and transect lengths for counting track sets in California. The volunteer biologists who selected the exact locations of the transect segments believed that roads along ridgetops would produce more tracks. We later learned that roads along streams were most productive. If we were to start a new sampling program for puma track sets, it would differ markedly from the

1985 design, and the results would likely differ as well.

Preconceived notions of habitat influence the study's location, spatial extent, grain of mapping, and ultimately the replication and interspersion of the environmental elements in the map. Habitat analysts often collect the use and availability data from locations where the species was known to occur, often in abundance. By siting habitat studies this way, analysts may often force results that conform to preconceived notions of habitat. Experimental design principles should be applied to mensurative as well as to manipulative studies (Smallwood 1993). The spatial extent and grain of the study should be appropriate to measuring the species' differential use of the environment. Replication and interspersion of treatments (i.e., environmental elements considered to be available to the species) also must be incorporated into mensurative studies (also see Otis 1998; Austin, Chapter 5). Along a transect or within a defined study area, replication of available elements is achieved through multiple occurrence of each environmental element in the measured set of elements (e.g., vegetation complexes). Those elements occurring once are pseudoreplicated (sensu Hurlbert 1984) and association of the species with that element should be considered dubious pending further research (e.g., environmental elements X and Z in Fig. 6.4 are pseudoreplicated, while Y is measured as two replicates). Interspersion is achieved by each element's occurrence between other elements in the measured set, such that gradient effects do not cause spurious relationships between measured use and availability. Thus, replication and interspersion of various environmental elements can be achieved when the program of observation includes two or more patches of each environmental element within the study area or along the transect.

Given that the analyst is constrained by observing extant environmental conditions, the observed use of the environment by an individual, population, or larger social unit need not reflect the species' perception of what constitutes habitat or high-quality habitat (Morrison et al. 1998). Much of what analysts may perceive as a numerical response to extant environmental conditions actually may be responses to the combination of both relic and current habitat and demographic conditions. Habitat models implicitly assume that deviations from the theoretical null patterns of distribution are immediate responses of individuals to resources that are available in the measured space. However, demographic organization can represent a long-term response to energy and material resources in the environment experienced by the species, and it can pose much of the information useful to the individuals. If a suite of environmental and demographic conditions typified the success of the species over long time periods, then the species likely developed perceptions of habitat and demographic organization that are relatively reliable evolutionarily. Such stability could be achieved by building the responsible neurons early in ontogeny (Coss and Goldthwaite 1995). How such perceptions would be manifested in the changed landscapes of modern times can affect our interpretations of habitat.

Habitat analysis is bound to include plenty of noise caused by conserved use of information from relic environments, which today may be missing from the studied environment. For example, after Hutto (1990) experimentally reduced food supplies from under the bark of specific trees, he found no difference in visits per tree or time spent at the tree by birds that depend on this food supply during the winter months in the boreal forest. In another example, the carrying capacity of pocket gophers in alfalfa fields is determined not by the food source, which is plentiful, but by the home range size. The home range size was established by natural selection in past environments where food supplies were typically more limited. Pocket gophers in alfalfa are so constrained by evolutionarily designed perceptions of space that they cannot fully exploit alfalfa stands, which are perhaps the most abundant food source these animals have ever encountered. Management or policy decisions should acknowledge a reasonable level of uncertainty in habitat analysis or assessment and should be very conservative.

Conclusions

Comparing numerical terms for use and availability cannot reliably characterize habitat without considering the environmental and demographic contexts of the numbers. The measurements and resulting associations are determined largely by the attributes of the field study design, as well as those of the analytical, interpretive design. These attributes are themselves determined largely by the analyst's preconception of habitat, demographic organization of the species, how the species is patterned spatially and temporally, and the reasons for animals to congregate or associate with environmental elements out of proportion to the element's availability (Fig. 6.2). At our current state of knowledge, world-views may bear more heavily on habitat analysis than does the procedure for measuring occurrence frequencies in environmental elements. Some of these world-views can be tested as hypotheses by directed field research. For example, how often do animals aggregate for reasons that do not bear directly on the availability of a food resource? Are all aggregations truly populations? How sharply bounded are animal populations? Answers to these and other related questions can provide habitat analysts with meaning for numerical comparisons that the models do not provide.

The effectiveness of habitat analysis based on numerical comparisons is largely dependent on knowing how and why animals distribute themselves. The models in Table 6.1 will be prone to inappropriate application and erroneous interpretation until analysts largely agree on the extent to which animal species respond numerically to energy availability, information presented by the past and current environments, or to predators and competitors. Each model has a theoretical root or history, and we need to decide whether the theoretical foundation is consistent with what we are attempting to measure and how we should interpret the resulting patterns.

Van Horne's (1983) argument that density is a misleading indicator of habitat quality remains valid, and based on recent research on density, it is all the more clear that numerical comparisons are currently of dubious utility to habitat analysis. Density estimates are sensitive to whether they are derived from population "isolates" or from sampling of the statistical universe (Preston 1962a), and they are sensitive to the size of the study area examined (Smallwood and Schonewald 1996). Until much more basic research on animal distribution has been conducted, the models in Table 6.1 should be used cautiously because their use can translate into inappropriate management decisions, sometimes with possible dire consequences for the species.

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Article

Map-Based Repowering and Reorganization of a Wind Resource Area to Minimize Burrowing Owl and Other Bird Fatalities

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Abstract: Wind turbines in the Altamont Pass Wind Resource Area (Alameda/Contra Costa Counties, California, USA) generate about 730 GWh of electricity annually, but have been killing thousands of birds each year, including >2,000 raptors and hundreds of burrowing owls. We have developed collision hazard maps and hazard ratings of wind turbines to guide relocation of existing wind turbines and careful repowering to modern turbines to reduce burrowing owl fatalities principally, and other birds secondarily. Burrowing owls selected burrow sites lower on slopes and on smaller, shallower slopes than represented by the average 10 × 10 m^2 grid cell among 187,908 grid cells sampled from 2,281,169 grid cells comprising a digital elevation model (DEM) of the study area. Fuzzy logic and discriminant function analysis produced likelihood surfaces encompassing most burrowing owl burrows within a fraction of the study area, and the former corresponded with burrowing owl fatalities and the latter with other raptor fatalities. Our ratings of wind turbine hazard were more predictive of burrowing owl fatalities, but would be more difficult to implement. Careful repowering to modern wind turbines would most reduce fatalities of burrowing owls and other birds while adding about 1,000 GWh annually toward California's 33% Renewable Portfolio Standard.

Keywords: Altamont Pass Wind Resource Area; burrowing owls; fatality rates; fuzzy logic; GIS; raptors; rating system; ground squirrels; Vasco Caves Regional Preserve; wind turbines

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1. Introduction

Policy-makers have hoped that wind energy generation in the Altamont Pass Wind Resource Area (APWRA) will contribute to meeting both California's growing energy demand and its recent mandates on renewable energy use. California's Governor Arnold Schwarzenegger issued Executive Order S-14-08 (17 Nov 2008), requiring that 33% of the energy delivered to California consumers shall come from renewable sources by the year 2020. Executive Order S-14-08 was termed the 33% Renewable Portfolio Standard (33% RPS). Given the energy used in 2006 and assuming the California Energy Commission's projected 1.2% annual growth in consumption, the renewable resource gap is 80,690 GWh. For perspective, the APWRA's installed capacity of 580 MW generated 730 GWh in 2006, or 0.9% of the renewable resource gap, but it has been killing thousands of birds per year since its development in the 1980s [1-2].

The locations for APWRA wind turbines were selected without considering patterns of bird flights, perching or nest locations, all of which influence collision risks. Most wind turbines were mounted on short towers, which positioned the rotor planes at heights above ground that are often used specifically for foraging by golden eagle, red-tailed hawk, American kestrel, and burrowing owl, and these species have subsequently been killed at high rates in the APWRA. Of particular concern are burrowing owl fatalities, estimated at 99 to 380 [3] and 440 per year in 1998–2003 [4], and 737–1,488 per year in 2005–2007 [2]. Whereas the more recent estimate might be inflated due to a possible bias in the scavenger removal adjustment [2], all of these fatality rates and those of other species [2] indicate the APWRA has been in excessive violation of the international Migratory Bird Treaty Act, Federal Bald and Golden Eagle Protection Act, and California Fish and Game Code 3503.5 (Birds of Prey). Burrowing owls have been declining in California [5-7], and are listed as a second-priority species of special concern in California [8], therefore, there is urgent need to sharply reduce burrowing owl fatalities caused by the APWRA's wind turbines.

After several years of research of bird behaviors and fatalities in the APWRA, it was concluded that careful repowering would reduce fatalities more so than the 16 mitigation measures formulated for continued operation of the old-generation turbines [9]. Careful repowering would replace old-generation wind turbines with modern turbines on taller towers and located where collision risk is minimized. Two small repowering projects were installed since the earlier recommendation for repowering. These were the 20.5 MW Diablo Winds Energy Project and the 38 MW Buena Vista Wind Energy Project, which began operations in 2004 and 2007, respectively. Compared to old-generation wind turbines in the APWRA, raptor fatalities per MW per year were 54% lower at Diablo Winds [2] and 91% lower at Buena Vista [10]. Since repowering, burrowing owl fatalities have not been found at Buena Vista and declined 24% at Diablo Winds compared to concurrently operating old-generation wind turbines [2].

Given that repowering of the APWRA is moving forward slowly and in piecemeal fashion a burrowing owl collision hazard map could help reduce burrowing owl fatalities immediately by showing which old-generation wind turbines pose the greatest risk to this species and where these turbines might be relocated to reduce collision risk [11-12]. A collision hazard map could reduce burrowing owl fatalities more substantially over the longer term by guiding where modern wind turbines are sited as repowering expands. One caveat to this hazard mapping approach is inter-specific variation in avian behavior in response to environmental and wind turbine attributes such that a least-hazards map developed for burrowing owls could potentially increase the collision risk for other species.

Earlier research established a positive relationship between burrowing owl fatalities and the number of burrowing owl burrows within 55 m of wind turbines [13], and a model based on a set of turbine and landscape attributes correctly predicted 71% of the burrowing owl fatalities documented in 1998–2003 [3]. Thus it is reasonable to assume that predicted burrowing owl burrow locations would correspond with wind turbine collision hazard, and this information could be used to develop a burrowing owl collision hazard map. In the APWRA, burrowing owls usually commandeer burrows from burrow systems constructed by California ground squirrels (*Spermophilus beecheyi*), so to develop a map predicting burrowing owl burrow locations we needed to know which portions of the landscape are selected by ground squirrels and which of this portion selected by ground squirrels is also selected by burrowing owls.

The goal of our study was to develop a predictive model of burrowing owl burrow locations in the APWRA to be used as a map-based indicator of collision hazards for relocating existing, old-generation turbines and guiding new-generation turbine siting in repowering. Our objectives were to: (1) develop predictive models of burrowing owl burrow locations; (2) validate the models by relating predicted burrowing owl burrow locations to documented locations, both within the spatial areas used to develop the predictive model and within an area that did not contribute to model development, i.e., Vasco Caves Regional Preserve, (3) validate the usefulness of the models by comparing burrowing owl fatality rates between turbines within areas predicted to be occupied by burrowing owls and turbines outside these areas, (4) compare the usefulness between a landscape-based hazard model and a wind turbine rating system based on environmental and wind turbine attributes associated with individual wind turbines, and (5) compare the tradeoffs of impacts of a burrowing owl management focus on other bird species, especially other raptors that often collide with the APWRA wind turbines.

2. Study Area

The APWRA encompassed about 16,450 ha of mostly non-native annual grassland in eastern Alameda and southeastern Contra Costa Counties in central California. The study area ranged 78 to 470 m above MSL, composed of hills, ridges and valleys, including ephemeral streams, stock ponds, seasonal ponds, and marshes. Most ridges were oriented northwest-southeast, increasing in size westward. Landowners grazed livestock and leased land to wind companies.

The APWRA included 580 MW of wind turbines of various models (Figure 1) during the 1998–2003 study [2], numbering about 5,400 in 1998 and 5,301 in 2003. In 2005, the Diablo Winds

Project replaced 105 150-kW and 25 250-kW Flowind vertical axis wind turbines with 38 Vestas 660-kW turbines (Figure 2). By the end of 2006 the Buena Vista Wind Energy project replaced 170 Windmaster, Nordtank, and Danwin turbines with 38 1-MW Mitsubishi turbines (Figure 2).

Data for model development were gathered at multiple locations around wind turbines throughout the central, eastern, and southern portions of the APWRA. Data for model validation were also gathered from Vasco Caves Regional Preserve near the northern boundary of the APWRA, including 52 330-kW Howden turbines and 20 65-kW Nordtank turbines (Photos A and F in Figure 1). Vasco Caves Regional Preserve was managed by East Bay Regional Park District, and consists of five relatively large, steep hills. Wind turbines occupied only one of the large hills and a plateau on the northern aspect of the Preserve [14].

3. Methods

3.1. Estimating Fatality Rates

Fatality search effort varied in frequency and number of turbines searched throughout the study period 1998–2003 [4]. During fatality searches biologists searched for bird carcasses within 50 m of each wind turbine, walking parallel routes at about 8–16 m intervals along rows of turbines. From March 1998 through September 2002, groups of turbines were added to the search rotation as access was granted and ultimately a total of 1,526 wind turbines arranged in 182 rows were searched at various intervals (mean = 53 days). From November 2002 to May 2003 another 2,548 turbines arranged in 380 rows were searched twice at intervals of 90+ days. Carcasses found in 1998–2003 were used for developing predictive models. Carcasses found during another monitoring period, from March 2005 through March 2007 [2], were used for model validation. Searches for these carcasses were performed with an average interval of 41 days at 2,650 wind turbines in stratified random plots, using comparable field methods to the earlier fatality monitoring program.

All carcasses or body parts found were examined to assign species, age, sex, and probable cause of death. Cause of death was determined by evidence of injuries, when available, such as burn marks or singed feathers typical of electrocution, and cut or twisted torsos, dismemberment and other forms of blunt force trauma typical of collisions with wind turbine blades. Unless assigned another cause of death, carcasses were assumed to have been killed by a wind turbine if found within 125 m of a wind turbine. Carcasses were used in fatality rate estimation if the estimated time since death was ≤90 days, because we did not want to include fatalities caused prior to the study period. The number of days since death was estimated after assessing carcass condition (e.g., fresh, weathered, dry, bleached bones) and decomposition level (e.g., flesh color, presence of maggots, odor).

Figure 1. Some of the various old-generation wind turbine models in the APWRA during 1998-2007, including Nordtank 65-kW (A), KCS56 100-kW (B), Bonus 150-kW (C), Polenko 100-kW (D), Windmatic 65-kW (E), Howden 330-kW (F), Micon 65-kW (G), Enertech 40-kW (H), Flowind 150-kW (I), and KVS-33 400-kW (J). These photos are not to scale.