Exhibit B

A Comprehensive Review of*:*

GREATER SAGE-GROUSE: ECOLOGY AND CONSERVATION OF A LANDSCAPE SPECIES AND ITS HABITATS

AND ADDITIONAL PAPERS OF RELEVANCE

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Table of Contents

List of Chapters and Papers Reviewed…….. 5

Chapters from ECOLOGY AND CONSERVATION OF GREATER SAGE-GROUSE: A LANDSCAPE SPECIES AND ITS HABITATS

Greater Sage-Grouse and Sagebrush Habitats: an Introduction to the Landscape.
(Greater Sage-Grouse and Sagebrush: an Introduction to the Landscape (2011 title))
Authors: Steven T. Knick and John W. Connelly
(Chapter 1 of the 2009 version; Introduction, Pages 1-9, of the 2011 version)
Review by Dr. Rob Roy Ramey II…… 8

Principal Federal Legislation and Current Management of Sagebrush Habitats: Implications for Conservation.
(Historical Development, Principal Federal Legislation and Current Management of Sagebrush Habitats: Implications for Conservation (2011 title))
Author: Steven T. Knick
(Chapter 2 of the 2009 version; Chapter 1, Pages 13-31, of the 2011 version)
Abstract……. 11
Review by Dr. Rob Roy Ramey II…… 12

The Legal Status of Greater Sage-Grouse: Organizational Structure of Planning Efforts.
Author: San J. Stiver
(Chapter 3 of the 2009 version; Chapter 2, Pages 33-49, of the 2011 version)
Abstract……. 14
Review by Dr. Rob Roy Ramey II…… 15

Characteristics of Greater Sage-Grouse Habitats: A Landscape Species at Micro- and Macro Scales.
Authors: John W. Connelly, E. Thomas Rinkes, and Clait E. Braun
(Chapter 4 of the 2009 version; Chapter 4 Pages 69-83, of the 2011 version)
Abstract……. 17
Review by Dr. Rob Roy Ramey II…… 18

Characteristics and Dynamics of Greater Sage-Grouse Populations.
(Chapter 5 of the 2009 version; Chapter 3, Pages 53-67, of the 2011 version)
Authors: John W. Connelly, Christian A. Hagen, and Michael A. Schroeder
Abstract……... 20
Review by Dr. Rob Roy Ramey II and Dr. Laura M. Brown……. 21

Harvest Management for Greater Sage-Grouse: A Changing Paradigm for Game Bird Management.
Authors: Kerry P. Reese and John W. Connelly
(Chapter 6 of the 2009 version; Chapter 7, Pages 101-111, of the 2011 version)

Abstract……... 22
Review by Dr. Rob Roy Ramey II and Dr. Laura M. Brown……. 23

Molecular Insights into the Biology of Greater Sage-Grouse.
Authors: Sara J. Oyler-McCance and Thomas W. Quinn
(Chapter 7 of the 2009 version; Chapter 5, Pages 85-94, of the 2011 version)
Abstract……. 25
Review by Dr. Rob Roy Ramey II…… 26

Predation on Greater Sage-Grouse: Facts, Process, and Effects.
Author: Christian A. Hagen
(Chapter 8 of the 2009 version; Chapter 6, Pages 95-100, of the 2011 version)
Abstract…… 28
Review by Dr. Rob Roy Ramey II and Dr. Laura M. Brown……. 29

Parasites and Infectious Diseases of Greater Sage-Grouse.
Authors: Thomas J. Christiansen and Cynthia M. Tate
(Chapter 9 of the 2009 version; Chapter 8, Pages 113-126, of the 2011 version)
Abstract…… 31
Review by Dr. Rob Roy Ramey II and Dr. Laura M. Brown…… 32

***West Nile Virus Ecology in Sagebrush Habitat and Impacts on Greater Sage-Grouse Populations.**
Authors: Brett L. Walker and David E. Naugle
(Chapter 10 of the 2009 version; Chapter 9, Pages 127-142, of the 2011 version)
Abstract…… 34
Review by Dr. Rob Roy Ramey II…….. 35

***Characteristics of Sagebrush Habitats and Limitations to Long-Term Conservation.**
Authors: Richard F. Miller, Steven T. Knick, David A. Pyke, Cara W. Meinke, Steven E. Hanser, Michael J. Wisdom, and Ann L. Hild
(Chapter 11 of the 2009 version; Chapter 10, Pages 145-184, of the 2011 version)
Abstract……... 42
Review by Dr. Vernon C. Bleich …….. 43

Pre-Euro-American and Recent Fire in Sagebrush Ecosystems.
(Chapter 12 of the 2009 version; Chapter 11, Pages 185-201, of the 2011 version)
Author: William L. Baker
Abstract……. 48
Review by Dr. Rob Roy Ramey II…… 49

Ecological Influence and Pathways and Land Use in Sagebrush.
Authors: Steven T. Knick, Steven E. Hanser, Richard F. Miller, David A. Pyke, Michael J. Wisdom, Sean P. Finn, E. Thomas Rinkes, and Charles J. Henny
(Chapter 13 of the 2009 version; Chapter 12, Pages 203-251, of the 2011 version)

Abstract…….. 50
Review by Dr. Rob Roy Ramey II……. 51

Influences of the Human Footprint on Sagebrush Landscape Patterns: Implications for Sage-Grouse Conservation.
Authors: Matthias Leu and Steven E. Hanser
(Chapter 14 of the 2009 version; Chapter 13, Pages 253-271, of the 2011 version)
Abstract……. 53
Review by Dr. Rob Roy Ramey II…… 54

Influences of Free-Roaming Equids on Sagebrush Ecosystems, With Focus on Greater Sage-Grouse.
Authors: Erik A. Beever and Cameron L. Aldridge
(Chapter 15 of the 2009 version; Chapter 14, Pages 273-290, of the 2011 version)
Abstract…… 56
Review by Dr. Rob Roy Ramey II…… 57

***Greater Sage-Grouse Population Dynamics and Probability of Persistence.**
Authors: Edward O. Garton, John W. Connelly, Jon S. Horne, Christian A. Hagen, Ann Moser, and Michael A. Schroeder
(Chapter 16 of the 2009 version; Chapter 15, Pages 293-381, of the 2011 version)
Abstract……. 59
Review by Dr. Rob Roy Ramey II…… 60
Review by Dr. John D. Wehausen …… 79
Review by Dr. Vernon C. Bleich……... 85

Influences of Environmental and Anthropogenic Features on Greater Sage-Grouse Populations, 1997-2007.
Authors: Douglas H. Johnson, Matthew J. Holloran, John W. Connelly, Steven E. Hanser, Courtney L. Amundson, and Steven T. Knick
(Chapter 17 of the 2009 version; Chapter 17, Pages 407-450, of the 2011 version)
Abstract…… 93
Review by Dr. Rob Roy Ramey II…….. 94

Connecting Pattern and Process in Greater Sage-Grouse Populations and Sagebrush Landscapes.
Authors: Steven T. Knick and Steven E. Hanser
(Chapter 18 of the 2009 version; Chapter 16, Pages 383-405, of the 2011 version)
Abstract…… 96
Review by Dr. Rob Roy Ramey II…….. 97

***Factors Associated With Extirpation of Sage-Grouse.**
Authors: Michael J. Wisdom, Cara W. Meinke, Steven T. Knick, and Michael A. Schroeder
(Chapter 19 of the 2009 version; Chapter 18, Pages 451-472, of the 2011 version)
Abstract…… 99

Review by Dr. Rob Roy Ramey II…….. 100
Review by Dr. Robert Zink…… 104

***Greater Sage-Grouse as an Umbrella Species for Shrubland Passerine Birds: a Multiscale Assessment.**
Authors: Steven E. Hanser and Steven T. Knick
(Chapter 20 of the 2009 version; Chapter 19, Pages 475-487, of the 2011 version)
Abstract…….. 109
Review by Dr. Robert Zink…… 110

***Energy Development and Greater Sage-Grouse.**
Authors: David E. Naugle, Kevin E. Doherty, Brett L. Walker, Matthew J. Holloran, and Holly E. Copeland
(Chapter 21 of the 2009 version; Chapter 20, Pages 489-503, of the 2011 version)
Abstract……...…….. 113
Review by Dr. Rob Roy Ramey II…… 115
Review by Dr. Mathew A. Cronin…… 118

***Energy Development and Conservation Tradeoffs: Systematic Planning for Greater Sage-Grouse in Their Eastern Range.**
Authors: Kevin E. Doherty, David E. Naugle, Holly E. Copeland, Amy Pocewicz, and Joseph M. Kiesecker
(Chapter 22 of the 2009 version; Chapter 21, Pages 505-516, of the 2011 version)
Abstract……. 122
Review by Dr. Rob Roy Ramey II…… 123
Review by Dr. Mathew A. Cronin…… 126

Response of Greater Sage-Grouse to the Conservation Reserve Program in Washington State.
Authors: Michael A. Schroeder and W. Matthew Vander Haegen
(Chapter 23 of the 2009 version; Chapter 22, Pages 517-529, of the 2011 version)
Abstract……. 130
Review by Dr. Rob Roy Ramey II…… 131

Restoring and Rehabilitating Sagebrush Habitats.
Author: David A. Pyke
(Chapter 24 of the 2009 version; Chapter 23, Pages 531-548, of the 2011 version)
Abstract and Review by Dr. Rob Roy Ramey II…….. 132

Future Trends for Greater Sage-Grouse and Sagebrush Habitats (2009 title)
(Conservation of Greater Sage-Grouse: A Synthesis of Current Trends and Future Management (2011 title))
Authors: John W. Connelly, Steven T. Knick, Clait E. Braun, William L. Baker, Erik A. Beever, Thomas J. Christiansen, Kevin E. Doherty, Edward O. Garton, Steven E. Hanser, Douglas H. Johnson, Matthias Leu, Richard F. Miller, David E. Naugle, Sara J. Oyler-McCance, David A. Pyke, Kerry P. Reese, Michael A. Schroeder, San J. Stiver, Brett L.

Walker, and Michael J. Wisdom
(Chapter 25 of the 2009 version; Chapter 24 Pages 549-563, of the 2011 version)
Abstract…… 133
Review by Dr. Rob Roy Ramey II……... 134
Review by anonymous reviewer…….. 138

ADDITIONAL PAPERS OF RELEVANCE

*Copeland, H.E., K.E. Doherty, D.E. Naugle, A. Pocewicz, J.M. Kiesecker (2009) Mapping Oil and Gas Development Potential in the US Intermountain West and Estimating Impacts to Species. *PLoS ONE* 4(10).
http://www.plosone.org/article/info:doi/10.1371/journal.pone.0007400
Abstract…… 140
Review by Dr. Rob Roy Ramey II…….. 141
Review by Dr. Vernon C. Bleich……... 146

*Schroeder, M.A., Aldridge, C.L., Apa, A.D., Bohne, J.R., Braun, C.E., Bunnell, S.D., Connelly, J.W., Deibert, P.A., Gardner, S.C., Hilliard, M.A., Kobriger, G.D., McCarthy, C.W., McCarthy, J.J., Mitchell, D.L., Rickerson, E.V., Stiver, S.J. (2004) Distribution of sage-grouse in North America. *Condor* 106:363–373.
Abstract……... 149
Review by Dr. Rob Roy Ramey II……... 150
Review by anonymous reviewer…….. 155

Literature Cited…… 157

Appendix 1. Review team bios……. 169

*Chapters given priority for review.

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Chapter 1 (2009), Introduction (2011):
GREATER SAGE-GROUSE AND SAGEBRUSH: AN INTRODUCTION TO THE LANDSCAPE

Authors: Steven T. Knick and John W. Connelly

No abstract

Review by: Dr. Rob Roy Ramey II

This is an introductory paper that provides background and context on the chapters that follow, as well as a brief presentation of previous research, including unpublished research on the sage grouse. The authors do not present new data or analyses.

1.1) The authors cite their own work frequently. Of 144 references in the text, 54 are of the author's own work (37.5%).

1.2) The peer review of this paper did not appear to meet the National Academy of Sciences criteria for independent peer review. Three peer reviewers on this paper were previous or current collaborators with the authors:
- Reviewer D.S. Dobkin was an author on Knick et al. (2003).
- Reviewer D.E. Naugle was author on three papers in the *Studies in Avian Biology* volume on sage grouse, and an author with Knick and Connlley on Chapter 25 in that volume.
- Reviewer J.T. Rotenberry was a coauthor on two papers with Knick: Knick and Rotenberry (1997) and Knick et al. (2003).

It is appropriate to ask what the rejection rate was on papers submitted to this volume. For comparison, *Frontiers in Ecology and the Environment*, published by the Ecological Society of America has a 53% rejection rate (Ecological Society of America 2004), and *Nature* has a >90% rejection rate.

Conflict on interest policies of the National Academies are available from their website: *Conflict of Interest Policy* under the sections: *Reviewing One's Own Work* and *Studies Related to Government Regulation.*

It is worth asking whether the review and publication process of this USGS-sponsored monograph was contrary to USGS peer review standards. (There may be more recent than the guidelines below.) The following excerpt is from the: "OMB, Final Information Quality Bulletin for Peer Review (December 16, 2004)" http://www.usgs.gov/usgs-manual/500/502-3.html

> *Even for these highly influential scientific assessments, the Bulletin leaves significant discretion to the agency formulating the peer review plan. In general, an agency conducting a peer review of a highly influential scientific assessment must ensure that the peer review process is transparent by making available to the public the written charge to the peer reviewers, the peer reviewers' names, the peer reviewers' report(s), and the agency's response to the peer reviewers' report(s). The agency*

selecting peer reviewers must ensure that the reviewers possess the necessary expertise. In addition, the agency must address reviewers' potential conflicts of interest (including those stemming from ties to regulated businesses and other stakeholders) and independence from the agency. This Bulletin requires agencies to adopt or adapt the committee selection policies employed by the National Academy of Sciences (NAS)[1] when selecting peer reviewers who are not government employees. Those that are government employees are subject to federal ethics requirements. The use of a transparent process, coupled with the selection of qualified and independent peer reviewers, should improve the quality of government science while promoting public confidence in the integrity of the government's scientific products.

1.3) The authors point out that declines of sage grouse are due to more than the loss of sage brush: *"Concluding that loss and degradation of sagebrush-dominated landscapes cause sage grouse population declines is deceptively simple, much like the ecosystems themselves."* Consequently, they present a long list of threats to sage grouse, some of which are speculative, poorly defined, or have not yet occurred. The effect size and relative importance of each of these factors is not quantified nor discussed. The list includes:

- *"Conversion to croplands which has eliminated or fragmented sagebrush in areas having deep fertile soils or irrigation potential."*
- *"Sagebrush remaining in these areas has been reduced to agricultural edges or to relatively unproductive environments."*
- *"Oil and gas resources are being developed primarily in the eastern portion of the sage-grouse range"*
- *"exploration and development of wind and geothermal energy"*
- *"Livestock grazing"* which has a *"diffuse influence"*
- *"Urbanization and human densities are increasing in the western US as people choose to live near wilderness and recreation areas."*
- *"new corridors proposed for energy transmission"*
- *"mapped roads"*
- *"recreation, including off-highway vehicles, is rapidly increasing on public lands"*
- *"human footprint influences the landscape structure of sagebrush-dominated habitats for sage-grouse."*

1.4) The authors suggest that harvest and predation do not have a range-wide effect. This assumption is questionable given the high harvest rates cited in Chapter 6 of this monograph: "*Predation (Hagen,this volume), harvest (Reese and Connelly, this volume), and disease (Christiansen and Tate, this volume) are significant to individuals or local groups but are not significant factors influencing population trends. Similarly, West Nile virus (Walker and Naugle, this volume) has the potential to significantly decrease sage-grouse numbers or eliminate relatively small peripheral populations but the effect on range-wide trends is less clear."*

1.5) The authors describe the rationale for their delineation of a *"Sage Grouse*

Conservation Area" **which encompasses a broader area than where sage grouse are currently or historically found.** It is notable that in Figure 4, "Core Areas" (favored by the authors) and "Management Zones" (developed by the Western Association of Fish and Wildlife Agencies (WAFWA)) are approximately equivalent in size and temporal scale (>50) years (note the log scales used in Figure 4).

1.6) The author's portrayal of patterns found in sagebrush ecosystems is more akin to a belief in natural design (teleology) than mainstream ecology and evolutionary biology, where such beliefs disappeared long ago (Mayr 1961; Gould and Lewontin 1979). Here is one example of the author's portrayal of the sagebrush ecosystem as complex, hierarchically organized, and with "integrated" structure and function: *"The model of sagebrush systems as a hierarchical organization arranged along spatial and temporal scales is one of the unifying concepts underlying the information presented in this volume (Fig. 4). This model presents ecological systems as an integrated assemblage of patterns and processes at smaller scales enclosed within successive levels at larger scales."* [My underlining for emphasis.]

1.7) The authors make a number of vague statements using undefined terms to describe upsets to the natural order of sagebrush ecosystems: *"Unbalanced dynamics of disturbance relative to recovery at smaller scales can change patterns observed at larger scales."* In this sentence, the terms "*unbalanced dynamics", "disturbance",* and *"recovery"* are undefined. Similarly, the term: *"human footprint"* is undefined.

1.8) In conclusion, the author's suggest that the sagebrush ecosystem can only be conserved through an integrated programmatic approach, providing a biological rationale for unified federal land use control, such as that under the Endangered Species Act: *"Conserving an area sufficiently large to contain an intact sagebrush system complete with disturbance and recovery dynamics is especially challenging because of the large spatial area and long temporal scale that will be required. Available financial and logistic resources limit the extent of conservation and management actions. However, core areas (Doherty et al., this volume), population units (Garton et al., this volume), or population components (Knick and Hanser, this volume) can help focus planning on large areas needed to sustain populations, thus avoiding a spatial checkerboard of unrelated actions that have less benefit to long-term conservation."*

Chapter 2 (2009), Chapter 1 (2011):
PRINCIPAL FEDERAL LEGISLATION AND CURRENT MANAGEMENT OF SAGEBRUSH HABITATS: IMPLICATIONS FOR CONSERVATION

Author: Steven T. Knick

Abstract from Knick (2009):
"The historical disposition and development of sagebrush (Artemisia spp.) landscapes has resulted in land ownership mosaics and differences in environmental qualities among land managers that influences today's conservation planning. Early land-use policies following major land acquisitions in 1776–1867 in the western US were designed to transfer the vast public resources to private ownership. Federal legislation enacted during the late 1800s and early 1900s encouraged development of arable regions, facilitated livestock grazing, created transportation corridors, and provided for access to minerals, coal, and petroleum. Productive lands characterized by deeper soils and access to water were transferred to private entities and converted from native habitats to agriculture. Privately owned lands are a major constituent of sagebrush landscapes in the Great Plains and Columbia Basin and are intermixed with public lands in other sage-grouse (Centrocercus spp.) management zones. The public still retains large areas and 70% of current sagebrush habitats. The USDI Bureau of Land Management has responsibility for almost half of the sagebrush habitat in the US; however, those lands are relatively unproductive and characterized by xeric environments and shallow soils. More recent legislation reflects changing public values to maintain or restore natural components, such as plants and wildlife, and minimize the impact of land uses in sagebrush landscapes. Multiple use dominates the management policy of most sagebrush habitat on public land; very little of the lands used by Greater Sage-Grouse (Centrocercus urophasianus) has protected status in national parks or reserves. Conserving sagebrush landscapes required by Greater Sage-Grouse and other wildlife will depend on engaging the mosaic of public agencies and private ownerships in management programs, understanding the broad diversity of habitat characteristics, and recognizing the limitations of environments supporting the majority of sagebrush habitat on public lands."

Review of:
PRINCIPAL FEDERAL LEGISLATION AND CURRENT MANAGEMENT OF SAGEBRUSH HABITATS: IMPLICATIONS FOR CONSERVATION

Review by: Dr. Rob Roy Ramey II

This "paper" is not a scientific paper. It is a review of legislation, ownership, and management of land considered by the author to be in sagebrush habitat. The history of land transfer from public to private ownership in the western U.S. and Canada receives lengthy treatment. The acreage of each land ownership category is quantified. The author's personal views on private land and federal land management policy are apparent.

2.1) The pre-European distribution of sage grouse is far more uncertain than the author and others suggest (e.g. Connelly et al. 2004; Schroeder et al. 2004).
Uncertainty stems from: an incomplete historic record, imprecise estimation of sagebrush extent (impossible to know from existing historic record) estimated from Kuchler's vegetation models, and the historic absence of sage grouse from northern Montana (absence of sage grouse from Lewis and Clark expedition and later records). A more extensive peer review of Schroeder et al. (2004) details these shortcomings.

2.2) The author makes a number of other statements suggestive of a preference for public ownership, a departure from multiple-use, and stricter regulation of private land:

"Almost two-thirds of the total sagebrush distribution in the US still remains within the public ownership."

"Multiple use dominates the management policy of most sagebrush habitat on public land; very little of the lands used by Greater Sage-Grouse (Centrocercus urophasianus) has protected status in national parks or reserves."

"Conserving sagebrush landscapes required by Greater Sage-Grouse and other wildlife will depend on engaging the mosaic of public agencies and private ownerships in management programs, understanding the broad diversity of habitat characteristics, and recognizing the limitations of environments supporting the majority of sagebrush habitat on public lands."

"The human footprint, a collective measure of anthropogenic use, was greatest in high productivity regions defined by deep soils, high precipitation, and shallow topographic terrain (Leu et al. 2008). This disproportionate loss of more productive regions to agriculture or from diversion of water for irrigation or other consumption, carries disproportionate impacts to sagebrush landscapes and their capacity to maintain themselves by leaving regions that are most sensitive to disturbance and less able to recover."

"Almost all sagebrush habitat in primary regions for Greater Sage-Grouse is undergoing

use and resource development (Knick et al. 2003, Wisdom et al. 2005, Holechek 2007, Knick et al. a, this volume). Wildlife conservation is not the exclusive or dominant objective on any major federal lands, except for the National Wildlife Refuge System (Bean and Rowland 1997). Consequently, conservation objectives often compete with commodity production and nonconsumptive uses, such as off-road vehicles for recreation, under the multiple-use mandate. Challenges to land uses increasingly are brought under the National Environmental Policy Act (1969) or to protect plants and animals through the Endangered Species Act (1973) (Bean and Rowland 1997, Quigley 2005). Petitions to list Greater Sage-Grouse (United States Department of the Interior 2005), restrictions on land use, and wilderness designations across sagebrush lands have significant implications for energy, national security, grazing, and recreation interests (Wambolt et al. 2002, Holechek 2007)."

"Land use would need to be restricted on 50,000 km^2 of sagebrush habitats if 10% of the total geographic area, a minimum target to conserve species distributions (Svancara et al. 2005), is to be protected across the SGCA or set aside in a reserve system (Bock et al. 1993). Much larger areas, ranging from 33–75% of the range-wide distribution, may be necessary to conserve biodiversity and ecosystem integrity (Soulé and Sanjayan 1998). Extensive restrictions are unlikely because of the resource value of these lands for non-consumptive and traditional uses. Thus, a large proportion of sagebrush habitat will continue to be managed for multiple purposes. Ultimately, our ability to develop long-term conservation strategies that maintain or increase populations of Greater Sage-Grouse will depend on involving a wide array of interests and perspectives in managing a broad diversity of uses for sagebrush habitats."

Chapter 3 (2009), Chapter 2 (2011):
THE LEGAL STATUS OF GREATER SAGE-GROUSE: ORGANIZATIONAL STRUCTURE OF PLANNING EFFORTS

Author: San Stiver

Abstract from Stiver:
"Range-wide conservation efforts to benefit Greater Sage-Grouse (Centrocercus urophasianus) began in 1954 with the Western Association of Fish and Wildlife Agencies' establishment of the Sage-Grouse Technical Committee. Contemporary conservation efforts expanded in the mid-1990s in response to increased concern about declining trends of sage grouse populations and habitats. Seven petitions have been filed with the USDI Fish and Wildlife Service to protect Greater Sage-Grouse under provisions of the Endangered Species Act (1973). Endangered species protection for Greater Sage-Grouse in the state of Washington was warranted but precluded. The 90-d finding determined that endangered species status was not warranted for the three petitions to protect Greater Sage-Grouse in Mono Basin, California and Nevada, the western subspecies of sage-grouse, and the eastern subspecies of sage-grouse. The remaining three petitions requesting range-wide protection for Greater Sage-Grouse were combined into one 12-mo finding. The USDI Fish and Wildlife Service completed a finding in 2005 and determined that listing was not warranted. This decision was litigated and remanded to the USDI Fish and Wildlife Service in December 2007 and currently is being reviewed. All western states and both Canadian provinces in the range of Greater Sage-Grouse have completed state or provincial strategic plans to manage Greater Sage-Grouse. Some conservation planning and conservation actions are being accomplished by local sage-grouse working groups. These groups are locally-based with membership composed of agency representatives and stakeholders in sagebrush (Artemisia spp.) ecosystems. More than 60 community-based sage-grouse conservation groups are active in the western US and Canada. Conservation actions are planned, coordinated, funded and accomplished by a partnership of state and federal agencies, landowners, industry, non-governmental organizations and the public."

Review of:
THE LEGAL STATUS OF GREATER SAGE-GROUSE: ORGANIZATIONAL STRUCTURE OF PLANNING EFFORTS

Review by: Dr. Rob Roy Ramey II

This is not a scientific paper. It is a legal and policy analysis that summarizes the petitions to list sage grouse under the ESA and summarizes non-ESA federal, state, community and NGO conservation efforts.

3.1) Environmental NGOs are mentioned by name but industries contributing to this effort are not:

> *"Traditional non-governmental organizations (NGO) and newly formed organizations have and continue to contribute to sage-grouse and sagebrush conservation efforts…NGOs including but not limited to the National Wildlife Federation, National Audubon Society, North American Grouse Partnership, The Nature Conservancy, Environmental Defense, and the Cooperative Sagebrush Initiative are working on sage-grouse and sagebrush conservation issues. Industries including mineral and coal mining, oil and gas exploration and production, renewable energy, energy transmission, ranching, and farming have a vested interest in the sagebrush steppe. These industries have and continue to support sage-grouse conservation within the constraints of conducting their business. Industries have provided funding and support for conservation actions, monitoring, and planning."*

3.2) Here and elsewhere, authors of this monograph refer to multiple ESA listing petitions that had been filed on sage grouse. Conspicuously absent from these discussions is any mention of *who* filed the petitions or the quality of their information. As shown below, all of the petitions to date have been filed by and litigated by activist organizations. This information is available from the USFWS sage grouse website.

1) Petition Date: May 14, 1999 (74 pages)
Petitioners: Northwest Ecosystem Alliance and Biodiversity Legal Foundation

2) Petition Date: January 25, 2000 (254 pages)
Petitioners: Mark Salvo, American Lands Alliance; Randy Webb, Net Work Associates; Andy Kerr, The Larch Company; Jasper Carlton, Biodiversity Legal Foundation; Susan Ash, Wild Utah Forest Campaign; Rob Edwards, Sinapu.

3) Petition Date: December 28, 2001 (493 pages)
Petitioners: Donald Randy Webb, Institute for Wildlife Protection

4) Petition Date: January 24, 2002 (468 pages)
Petitioners: Donald Randy Webb, Institute for Wildlife Protection

5) Petition Date: June 18, 2002 (7 pages)
Petitioners: Craig Dremann

6) Petition Date: July 3, 2002 (524 pages)
Petitioners: Donald Randy Webb, Institute for Wildlife Protection

7) Petition Date: March 19, 2003 (992 pages; this is a combination of the previous petitions for Western and Eastern subspecies)
Petitioners: Donald Randy Webb, Institute for Wildlife Protection

8) Petition Date: December 22, 2003 (218 pages)
Petitioners: Mark Salvo American Lands Alliance, Biodiversity Conservation Alliance, Center for Biological Diversity, Forest Guardians, The Fund for Animals, Gallatin Wildlife Association, Great Old Broads for Wilderness, Hells Canyon Preservation Council, The Larch Company, Northwest Ecosystem Alliance, Northwest Council for Alternatives to Pesticides, Oregon Natural Desert Association, Oregon Natural Resources Council, Predator Defense Institute, Sierra Club, Sinapu, Western Fire Ecology Center, Western Watersheds Project, Wild Utah Project, Wildlands CPR, and Center for Native Ecosystems

9) Petition Date: November 10, 2005 (87 pages plus appendices)
Petitioners: Submitted by Stanford Law School Environmental Law Clinic on behalf of The Sagebrush Sea Campaign (WildEarth Guardians), Western Watersheds Project, Center for Biological Diversity and Christians Caring for Conservation.

Chapter 4 (2009), Chapter 4 (2011):
CHARACTERISTICS OF GREATER SAGE-GROUSE HABITATS: A LANDSCAPE SPECIES AT MICRO AND MACRO SCALES

Authors: John W. Connelly, E. Thomas Rinkes, and Clait E. Braun

Abstract from Connelly et al.:
"Greater Sage-Grouse (Centrocercus urophasianus) depend on sagebrush (Artemisia spp.) for much of their annual food and cover. This close relationship is reflected in the North American distribution of sage-grouse, which is closely aligned with sagebrush, and in particular big sagebrush (Artemisia tridentata) and silver sagebrush (A. cana). This association is most pronounced in late autumn, winter, and early spring when sage-grouse are dependent on sagebrush for both food and cover. However, sage-grouse also rely on sagebrush at other times of year, especially for nesting cover during the breeding season. Other habitat characteristics may not be as obviously important as sagebrush, but may be nearly as essential. For example, herbaceous vegetation provides important food and cover during nesting and early brood-rearing seasons, and thus has a major role in the population dynamics of sage-grouse. Available evidence clearly supports the conclusion that conserving large landscapes with suitable habitat is important for conservation of sage-grouse. Moreover, natural variation in vegetation and the dynamic nature of mature sagebrush stands should be considered for all habitat descriptions and prior to any management action. Sagebrush habitats have been lost, fragmented, and degraded as a result of many different anthropogenic disturbances. Complicating matters, the traditional nature of seasonal movements by Greater Sage-Grouse suggests this species has little ability to adapt to habitat change. Therefore, land management agencies must establish sagebrush conservation as one of their highest priorities if remaining habitats are to be maintained. Additionally, these agencies must develop and implement effective habitat reclamation measures to offset unavoidable losses. Given the strong dependence of Greater Sage-Grouse on sagebrush habitats, failure to protect what is left and fix what is broken will likely result in extirpation of many populations of Greater Sage-Grouse."

Review of:
CHARACTERISTICS OF GREATER SAGE-GROUSE HABITATS: A LANDSCAPE SPECIES AT MICRO AND MACRO SCALES

Review by: Dr. Rob Roy Ramey II

This unremarkable paper is a compilation and review of existing information on sage grouse.

4.1) Absent from this paper is any discussion of one of the most ubiquitous hazards to sage grouse across their range: wire fencing. This is a hazard to sage grouse because they fly low and fast. Sage grouse also may avoid fences because of predation risk from perching raptors. The USFWS devoted an extensive discussion to this hazard in their 2008 Interim Status Update (USFWS 2008) and Environmental Defense has issued a white paper on the subject (Environmental Defense 2009)

Other obvious hazards found in sage grouse habitat, but not mentioned, include: hunting harvest, which occurs across most of sage grouse habitat, and predation, which only received one mention in the context of nest predation when nesting is at high densities. In contrast, the deleterious effects of energy development are mentioned three times.

4.2) The author claims that sage grouse are a "landscape species," citing Connelly et al. (2004). However, a search for "landscape species" in Connelly et al (2004) yields an ambiguous use of the term:

"Sage-grouse populations typically inhabit large, interconnect expanses of sagebrush and thus have been characterized as a landscape-scale species (Patterson 1952, Wakkinen 1990)."

"Although sage-grouse are considered a landscape species, conclusive data are unavailable on minimum patch sizes of sagebrush necessary to support viable populations of sage-grouse.

4.3) There are several references that suggest that sage grouse have a broader habitat tolerance than just sage brush. For example:

"Historically, the distribution of sage-grouse was closely tied to the distribution of the sagebrush ecosystem (Wambolt et al. 2002, Schroeder et al. 2004). However, populations of sage-grouse have been extirpated at places throughout their former range (Schroeder et al. 1999, Wambolt et al. 2002), concomitant with habitat loss and degradation, so that the species' current distribution is less closely aligned with that of sagebrush."

"Sage-grouse typically occupy habitats with a diversity of species and subspecies of

sagebrush, but may also use a variety of other habitats including riparian meadows, agricultural lands, steppe dominated by native grasses and forbs, shrub willow (Salix spp.), and sagebrush habitats with some conifer or quaking aspen (Populus tremuloides) (Patterson 1952, Dalke et al. 1963). These habitats are usually intermixed in a sagebrush-dominated landscape (Griner 1939, Patterson 1952, Dalke et al. 1963, Savage 1969). Sage-grouse have used habitats altered by man throughout the species' range including crested wheatgrass (Agropyron cristatum) seedings, and different agricultural crops (Patterson 1952, Gates 1983, Connelly et al. 1988, Blus et al. 1989, Sime 1991). Leks are often in altered areas including dirt roads and areas seeded with crested wheatgrass; however these areas are adjacent to sagebrush stands that provide nesting and early brood-rearing habitat. By itself, evidence of use does not imply importance. The value of these habitats to sage-grouse in meeting their seasonal habitat requirements is dependent on the juxtaposition of these habitats in relation to sagebrush and the hazards (Connelly et al. 2000a, Beck et al. 2006) to grouse using these areas."

Chapter 5 (2009), Chapter 3 (2011):
CHARACTERISTICS AND DYNAMICS OF GREATER SAGE-GROUSE POPULATIONS

Authors: John W. Connelly, Christian A. Hagen, and Michael A. Schroeder

Abstract from Connelly et al.:
"Early investigations supported the view that Greater Sage-Grouse (Centrocercus urophasianus) population dynamics were typical of other upland game birds. More recently, greater insights into the demographics of Greater Sage-Grouse revealed this species was relatively unique because populations tended to have low winter mortality, relatively high annual survival, and some populations were migratory. We describe the population characteristics of Greater Sage-Grouse and summarize traits that make this grouse one of North America's most unique bird species. Data on movements, lek attendance, and nests were obtained from available literature, and we summarized female demographic data during the breeding season for the eastern and western portions of the species' range. Lengthy migrations between distinct seasonal ranges are one the more distinctive characteristics of Greater Sage-Grouse. These migratory movements (often >20 km) and large annual home ranges (>600 km^2) help integrate Greater Sage-Grouse populations across vast landscapes of sagebrush (Artemisia spp.)-dominated habitats. Clutch size of Greater Sage-Grouse averages seven–eight eggs and nest success rates average 51% in relatively non-altered habitats while those in altered habitats average 37%. Adult female Greater Sage-Grouse survival is greater than adult male survival and adults have lower survival than yearlings, but not all estimates of survival rates are directly comparable. The sex ratio of adult Greater Sage-Grouse favors females but reported rates vary considerably. Long- term age ratios (productivity) in the fall have varied from 1.4–3.0 juveniles/adult female."

Review of:
CHARACTERISTICS AND DYNAMICS OF GREATER SAGE-GROUSE POPULATIONS

Review by: Dr. Rob Roy Ramey II and Dr. Laura M. Brown

The abstract accurately summarizes the chapter and gives the main conclusions. Data are from a very large number of studies, over many decades, and summary statistics are reported. No hypotheses were tested, as this was a summary of the population data collected from the different states over the years. Authors indicated where data could not be summarized because of inconsistencies in data collection. The most interesting findings are that sage grouse have generally high winter survival rates, relatively high annual survival rates, and are more migratory over greater distances than previously thought.

The authors are correct in suggesting that: *"Sage-grouse do not fit the commonly accepted paradigm of upland game bird demographics (Allen 1962)."* This is because sage grouse are long-lived birds that do not have a high rate of reproduction as found with many shorter-lived game birds. What escapes these authors is the implications of this for hunting: the intensity of sage grouse harvests in the past were a likely contributor to their overall decline.

The authors also suggest that sage grouse require "vast landscapes" (the term "landscape species" being an invention of the lead author). While sage grouse occupy a large range, they do persist in many semi-isolated populations interconnected by occasional dispersal (migration among populations). The seasonal migration distances (*"often >20km"*) and home ranges (*">600km"*) are not remarkable compared to other species (e.g. virtually all mobile species could be termed "landscape species", rendering the term meaningless).

Chapter 6 (2009, Chapter 7 (2011):
HARVEST MANAGEMENT FOR GREATER SAGE-GROUSE: A CHANGING PARADIGM FOR GAME BIRD MANAGEMENT

Authors: Kerry Paul Reese and John W. Connelly

Abstract by Reese and Connelly:
"Harvest of Greater Sage-Grouse (Centrocercus urophasianus) has occurred throughout recorded history, but relatively few studies addressed the impact of harvest on sagegrouse numbers. Harvest of Greater Sage-Grouse occurs in 10 of 11 western states in which they reside. Hunting seasons, and bag and possession limits have often become more conservative over the species' range during the past decade as states responded to changing population numbers and perceived threats to the birds, and then acted to reduce harvest opportunities. By 2007, hunting season lengths ranged from 2–62 d with a mean length of 10 d. Annual harvest estimates range from 10 birds in South Dakota to 10,378 in Wyoming. Total estimated annual harvest of Greater Sage-Grouse in the 10 states in 2007 was 28,180 birds. The effects of hunting on sage-grouse populations remains equivocal based on published literature, but the paradigm of harvest as compensatory may be shifting as evidence accumulates that populations of Greater Sage-Grouse require more conservative hunting regulations to reduce the potential for excessive harvest. Recent research suggests that because Greater Sage-Grouse normally experience low mortality over winter, mortality from hunter harvest in September and October may not be compensatory. Harvest mortality on most populations of Greater Sage-Grouse appears to be low, but both harvest levels and population abundance must be closely monitored in every population to improve management regulations for the harvest of the species. Biological data obtained from harvested birds is vital for continued management of sage-grouse populations. No studies have demonstrated that hunting is a primary cause of reduced numbers of Greater Sage-Grouse, and cessation of harvest in Washington 20 yr ago has not resulted in increasing population levels. Continued concern over general population declines in Greater Sage-Grouse populations from known (disease, habitat loss, and habitat fragmentation) and unknown origins, requires new research and continued routine collection of biological data for each population to optimize future harvest strategies."

Review of:
HARVEST MANAGEMENT FOR GREATER SAGE-GROUSE: A CHANGING PARADIGM FOR GAME BIRD MANAGEMENT

Review by: Dr. Rob Roy Ramey II and Dr. Laura M. Brown

This chapter gave a history of sage grouse hunting, and harvest rates from the states that allow hunting (only Washington State and Canada do not allow sage grouse hunting). It described the paradigm change that came about as a result of research that shows sage grouse are a K-selected species (high winter survival, longer lifespan than most upland birds (3-6 y), smaller clutch sizes (6-9). Reese and Connelly recommend that harvests should be 5-10% of the fall population, however, this recommendation is not based on a quantitative analysis and there are no reliable methods currently in use to determine population size. It is worth asking the question: With this species in decline, why are we allowing any harvest at all? If there is a desire to maintain harvest for cultural reasons then why not limit harvest to male sage grouse only, or to populations that are stable?

6.1) The annual harvest data of sage grouse from each state are shown in Table 3 (*Annual harvest estimates for greater sage-grouse by state, 2001–2007*). The population-level impact to sage grouse however, cannot be evaluated because there are no reliable estimates of annual population size. This represents a situation where there is a known source of sage grouse mortality, in the tens of thousands annually. Yet its effect is dismissed (here and by other authors in the volume) as unimportant, while other, hypothetical or undocumented sources of mortality are proposed to be regulated.

6.2) The paper cited by Reese and Connelly in support of hunt harvest not exceeding 10% of the fall population (Connelly et al. 2000c) was actually a qualitative assessment (no supporting quantitative analyses). In this case, the use of *"careful assessments"* of hunting harvest is undefined by the authors. It must be assumed that these *"careful assessments"* are qualitative because there are no reliable data to estimate population size.

> *"6) Hunting seasons for sage grouse should be based on careful assessments of population size and trends... sage grouse tend to have relatively long lives with low annual turnover (Zablan 1993, Connelly et al. 1994) and a low reproductive rate (Gregg 1991, Connelly et al. 1993). Consequently, hunting may be additive to other causes of mortality for sage grouse (Johnson and Braun 1999, Connelly et al. 2000a). However, most populations appear able to sustain hunting if managed carefully (Connelly et al. 2000a)."*

6.3) Reese and Connelly suggest that harvest quotas are "conservative," however they also admit that there is no data or reliable method with which to determine population size: *"Harvest regulations as currently structured (Table 1) tend to be conservative and may keep harvest (Table 3) <10% of fall population size. However,*

states do not presently measure fall population size of Greater Sage-grouse and no recognized protocol has been established to do so."

6.4) This paper identifies hunting as a major source of female sage grouse mortality. This would suggest a need for limiting the hunting of female sage grouse in order to avert population level declines that have occurred in combination with other factors. One study cited by Reese and Connelly reported that: *"Fall harvest caused 15% of known male mortality and 42% of known female mortality. Forty-six percent of all female mortality occurred during the hunting season (September–October) and harvest accounted for 91% of female deaths. In contrast, only 2% of the deaths of either sex occurred during the four post-hunting season months of November through February. The low over-winter mortality rate supports the contention that winter is not typically a difficult season for Greater Sage-Grouse (Beck and Braun 1978, Remington and Braun 1988, Sherfy 1992, Sika 2006)."*

"*Connelly et al. (2000a) concluded that for adult females hunting losses are likely additive to winter mortality and may result in lower breeding populations."*

6.5) The authors point out that there are no studies that indicate population-level impacts from sage grouse hunting, but they also point out that reliable population data is not available.
"No studies have demonstrated that hunting is a primary cause of reduced numbers of Greater Sage-Grouse. Many studies support habitat-based reasons for sage-grouse population declines (Swenson et al. 1987, Dobkin 1995, Connelly and Braun 1997, Connelly et al. 2000a, b: Leonard et al. 2000, Aldridge and Brigham 2002, Pedersen et al. 2003, Walker et al. 2007a, Doherty et al. 2008)."

The guesswork that typified decades of sage grouse harvest management can be summed up in the following statement in Reese and Connelly: *"An appropriate harvest rate has not been determined for Greater Sage-Grouse populations. Harvest equal to 5–10% of the fall population may be appropriate, but assumes detailed and specific knowledge of population size in September or October. Given the uncertainty in abundance estimates for breeding season populations, expecting any state to adequately determine size of any population of Greater Sage-Grouse in fall is not realistic."*

According to Reese and Connelly, *"The effects of hunting on sage-grouse populations remains equivocal based on published literature, but the paradigm of harvest as compensatory may be shifting as evidence accumulates that populations of Greater Sage-Grouse require more conservative hunting regulations to reduce the potential for excessive harvest."*

6.6) An aspect of hunting that is not mentioned by the authors is aversive conditioning, which can contribute to birds avoiding humans. Therefore, if there is concern that sage grouse avoid areas of human development, then limiting hunting in those areas has the potential to reduce aversive conditioning and thus the secondary effects of avoidance.

Chapter 7 (2009), Chapter 5 (2011):
MOLECULAR INSIGHTS INTO THE BIOLOGY OF GREATER SAGE-GROUSE

Authors: Sara J. Oyler-McCance and Thomas W. Quinn

Abstract from Oyler-McCance and Quinn:
"Recent research on Greater Sage-Grouse (Centrocercus urophasianus) genetics has revealed some important findings. First, multiple paternity in broods is more prevalent than previously thought, and leks are not comprised of kin groups. Second, the Greater Sage-Grouse is genetically distinct from the congeneric Gunnison Sage-Grouse (C. minimus). Third, the Lyon-Mono population in the Mono Basin, spanning the border between Nevada and California, has unique genetic characteristics. Fourth, the previous delineation of western (C. u. phaios) and eastern Greater Sage-Grouse (C. u. urophasianus) is not supported genetically. Fifth, two isolated populations in Washington show indications that genetic diversity has been lost due to population declines and isolation."

Review of:
MOLECULAR INSIGHTS INTO THE BIOLOGY OF GREATER SAGE-GROUSE

Review by: Dr. Rob Roy Ramey II

This paper represents a summary of genetic studies on Greater Sage-Grouse. Of the five main conclusions, the second and third are not supported by the data. That is because the Gunnison sage grouse was described as a new species based upon a low level of genetic divergence, a level typically found among nearly populations rather than species. The primary author of these studies, Oyler-McCance, uses a low threshold for considering species and populations as "distinct."

7.1) The following excerpts from a manuscript in preparation for publication (Ramey, in prep) provides a concise summary of the questionable basis of the recognition of the Gunnison sage grouse as a new species:

> *"The Gunnison sage grouse (Centrocerus minimus) was described as a new species by Young et al. (2000) based on body weight, courtship behavior, plumage, geographic isolation, and genetic data.*
>
> ***Size differences were based on selective use of data***
> *Although the body size of the Gunnison sage grouse is smaller than those reported for the greater sage grouse (Centrocerus minimus), Young et al. (2000) exaggerated the differences between the Gunnison sage grouse and greater sage grouse by not including intermediate populations in the comparison.*
>
> ***Qualitative descriptions were used to describe courtship displays***
> *Reported differences between the Gunnison sage grouse and greater sage grouse in plumage and courtship behavior are mostly qualitative: they rely on line drawings, artist renderings, or pictures of "typical" individuals; and they lack statistical analysis. No quantitative comparison of these traits was made across the range of variation found in the greater sage grouse.*
>
> ***Vocalization experiment not rigorous***
> *A vocalization playback experiment, used as evidence of reproductive isolation, lacked a reciprocal design, and the conclusions are unsupported by any rigorous hypothesis testing.*
>
> ***Geographic isolation not supported***
> *The presumed geographic isolation of Gunnison sage grouse due to mountainous terrain is contrary to observations of sage grouse in the alpine, nor a lack of isolating topography. MtDNA and microsatellite data, and a documented population located east of the Continental Divide also refute the presumption of geographic isolation.*

Extent of historic decline is speculative
The presumed loss of Gunnison sage grouse from >90% of its historic range is speculative (not supported by physical evidence).

No diagnostic genetic markers
Nine out of ten mtDNA haplotypes found in Gunnison sage grouse were also found in the greater sage grouse. The one unique mtDNA haplotype found in Gunnison sage grouse has only a single mutational difference from the most common mtDNA type found in both species. While frequency differences exist between the Gunnison sage grouse and greater sage grouse in microsatellite alleles, there were no unique microsatellite alleles in Gunnison sage grouse.

Taken collectively, much of the evidence used in support for species status for the Gunnison sage grouse is questionable."

Chapter 8 (2009), CHAPTER 6 (2011):
PREDATION ON GREATER SAGE-GROUSE: FACTS, PROCESS, AND EFFECTS

Author: Christian A. Hagen

Abstract from Hagen:
"Although Greater Sage-Grouse (Centrocercus urophasianus) face a suite of predators in sagebrush (Artemisia spp.) communities across the species' range, none of these predators specialize on sage-grouse. Greater Sage-Grouse are susceptible to predation from egg to adult leading to the hypothesis that predator control would be an effective conservation tool for sage-grouse populations. Therefore, I reviewed the literature pertaining to predator communities across the range of Greater Sage-Grouse and assessed the effects of predation on sage-grouse life history. I then provided a framework for evaluating when predator management may be warranted. Generally, nest success rates and adult survival are high, suggesting that on average predation is not limiting. However, in fragmented landscapes or in areas with subsidized predator populations predation may limit population growth. Few studies linked habitat quality to mortality rates, and fewer still linked these rates to predation. Predator management studies have not provided sufficient evidence to support implementation over broad geographic or temporal scales, but limited information suggests predator management may provide short-term relief for a population sink. Evaluating the need for predator management will require linking reduced demographic rates to habitat quality (fragmentation or degradation) or predator populations out of the natural range of variability (exotic species of subsidized populations). Alternatively, managers might consider predator management in translocation efforts to buffer recently released individuals from potentially elevated predation rates. Future work should quantify predator and alternate prey communities in habitats used by Greater Sage-Grouse."

Review of:
PREDATION ON GREATER SAGE-GROUSE: FACTS, PROCESS, AND EFFECTS

Review by: Dr. Rob Roy Ramey II and Dr. Laura M. Brown

This paper summarizes literature and unpublished research about predation on sage grouse. All of the predators on sage grouse are generalists, meaning that they prey on other species as well. Sage grouse eggs are preyed upon by red foxes, coyotes, badgers, common ravens, black-billed magpies. Common predators of juvenile and adult sage-grouse include golden eagles, prairie falcons (as well as other raptors), coyotes, badgers, and bobcats. Younger birds are thought to be preyed on by common ravens, red fox, northern harrier, ground squirrels, snakes, and weasels.

8.1) The author found fault with most studies that had reported a positive effect of predator management and concluded its effectiveness was generally short term. A single paragraph was devoted to *"Predator Control as a Conservation Tool."*
Important research not cited by the author includes Coates and Delehanty (2004), who reported a 73.6% nest success compared to a mean of 42.6% based on 14 studies from 1941-1997.

8.2) There is no discussion of potential benefits of utilizing anti-perch devices on powerpoles and fence posts to discourage raptors and ravens in sage grouse habitat.

8.3) There is no discussion of potential benefits of burying powerlines, thus eliminating perches for raptors and ravens.

8.4) There is no discussion of potential benefits of trash control measures to eliminate food subsidies to ravens, magpies, red foxes, and coyotes.

8.5) There was no discussion of using predator management in an adaptive management framework (e.g. to be implemented under specific circumstances when it would be most effective and on those species where it would have the greatest positive effect for sage grouse).

Instead, the author suggests an untested approach, for which supporting data are lacking: *"A more recent recognition is that the broader financial and political cost to removing predators at a scale and extent which may be effective is no longer socially or ecologically viable (Messmer et al. 1999). The most effective long-term predator management for sage-grouse population may be through maintaining connectivity of suitable habitats (Schroeder and Baydack 2001). However, most sage-grouse research has failed to quantify predator community structure or predation rates in relation to habitat variables, let alone within the landscape context. Thus, it is not currently possible to understand relationships among habitat structure, demographic rates of sage-grouse, and the predator community of an area and to incorporate these into a broad-based predator-management program for sage-grouse."*

While there may be merit in some aspects of the author's suggestion, experience has shown that reducing predation on a species of concern requires an *integrated predator management* approach. Such an approach was recently proposed by Coates and Delehanty (2010), specifically to reduce predation on sage grouse nests.

Chapter 9 (2009), Chapter 8 (2011):
PARASITES AND INFECTIOUS DISEASES OF GREATER SAGE-GROUSE

Authors: Thomas J. Christiansen and Cynthia M. Tate

Abstract from Christiansen and Tate:
"We report the parasites, infectious diseases, and non-infectious diseases related to toxicants found in the Greater Sage-Grouse (Centrocercus urophasianus) across its range. Documentation of population-level effects is rare although researchers have responded to the recent emergence of West Nile virus with rigorous efforts. West Nile virus shows greater virulence and potential population level effects than any infectious agent detected in Greater Sage-Grouse to date. Research has demonstrated: (1) parasites and diseases can have population- level effects on grouse species, (2) new infectious diseases are emerging, and (3) habitat fragmentation is increasing the number of small, isolated populations of Greater Sage-Grouse. Natural resource management agencies need to develop additional research and systematic monitoring programs for evaluating the role of micro- and macroparasites, especially West Nile virus, infectious bronchitis and other corona viruses, avian retroviruses, Mycoplasma spp., and Eimeria and associated enteric bacteria affecting sage-grouse populations."

Review of:
PARASITES AND INFECTIOUS DISEASES OF GREATER SAGE-GROUSE

Review by: Dr. Rob Roy Ramey II and Dr. Laura M. Brown

This paper is a summary of the literature and unpublished information on parasites and infectious disease in sage grouse. It does not report original research and no hypotheses were tested. Considerable attention is devoted to West Nile virus, although mostly summarizing existing information.

9.1) The authors note that research to date has not shown more than short-term population-level effects of disease and parasites. West Nile virus (WNV) is discussed as having a *potential* population-level effect and concern is expressed about the combined effects of WNV, climate change, and habitat fragmentation. This paper basically calls for increased surveillance on diseases, although no specific questions are laid out and no plan provided on how such data will be used to guide management decisions. If data were gathered in the context of an adaptive management framework with specific questions, sampling design, and triggers for management actions, it would be more likely to produce results than the authors' vaguely defined "integrated, multidisciplinary approach."

9.2) Christiansen and Tate paint a simplistic picture of WNV epidemiology and fail to discuss implications of relevant recent literature on WNV. They refer to susceptibility of sage grouse to WNV as *"extreme,"* citing a study of captive inoculated birds (Clark et al. 2006) and an unpublished personal communication. Here is what Clark et al. (2006) actually reported:

> "Although data on mortality rates for other species of birds are few, greater sage-grouse should be considered a highly susceptible species among birds since all greater sage-grouse died after being experimentally infected with WNV. Of the 25 species of birds experimentally infected by Komar et al. (2003), 8 showed mortality, and 4 of these showed 100% mortality: American crow (*Corvus brachyrhynchos*), black-billed magpie (*Pica hudsonia*), ring-billed gull (*Larus delawarensis*), and house finch (*Carpodacus mexicanus*). Mortality was high for other species as well (33–75%)."

Clark et al. (2006) reported mortality from experimental inoculation with $10^{3.2}$ PFU equivalents was 9/9 for unvaccinated birds and 4/5 for vaccinated birds. Mortalities were obviously high yet, as noted by Christiansen and Tate, the reported resistance of sage grouse to WNV in the wild was 1.8–10.3%. And the documented percentage of infection in the wild was low to moderate, 2.4–28.9%. Thus, while susceptibility of sage grouse to WNV is high at present, the mortality in the wild is not 100%, nor are all populations or all birds affected.

Experience with other species affected by WNV (or other flaviviruses) has shown that natural selection plays a role both in the evolution of WNV resistance and moderation of viral virulence over time (Ferguson et al. 2008; Brault 2009). This aspect of flavivirus

epidemiology is not acknowledged in the simplistic presentation of information by Christiansen and Tate (or other contributions in this monograph).

The distribution of documented infections at the Center for Disease Control (please refer to CDC data discussed in the review of Chapter 10) clearly show that infections have been highly variable across the West. Recent research shows that WNV does not affect all populations equally, because transmission is dependent upon a wide variety of factors. These include: the abundance of vector populations (mosquitoes) which depend both on climate perturbations and abundance of suitable water sources (not just coal bed methane ponds), population fluctuations and the spatial distribution of intermediate hosts that serve to spread WNV (e.g. corvids), spatial variation in species diversity (high diversity can dilute the spread within individual species), differences in passive transfer of maternal WNV antibodies to offspring, variation in virulence among WNV strains, introduction of novel strains introduced by rising global trade, and viral dose which has been shown to affect WNV viremia (Hahn et al. 2006; Ferguson et al. 2008; Artsob et al. 2009; Brault 2009; Weissenbock et al. 2010). The epidemiology of WNV in the wild is a far more complex situation that the simplistic presentation in this and other chapters in this monograph.

Chapter 10 (2009), Chapter 9 (2011):
WEST NILE VIRUS ECOLOGY IN SAGEBRUSH HABITAT AND IMPACTS ON GREATER SAGE-GROUSE POPULATIONS

Authors: Brett L. Walker and David E. Naugle

Abstract from Walker and Naugle:
"Emerging infectious diseases can act as important new sources of mortality for wildlife. West Nile virus (Flaviviridae, Flavivirus) has emerged as a potential threat to Greater Sage-Grouse (Centrocercus urophasianus) populations since 2002. We review the ecology of West Nile virus in sagebrush (Artemisia spp.) ecosystems of western North America, summarize the influence of the virus on Greater Sage-Grouse mortality and survival, use demographic models to explore potential impacts on population growth, and recommend strategies for managing and monitoring such impacts. The virus was an important new source of mortality in low and mid-elevation Greater Sage-Grouse populations range-wide from 2003–2007. West Nile virus can simultaneously reduce juvenile, yearling, and adult survival—three vital rates important for population growth in this species, and persistent low-level West Nile virus mortality and severe outbreaks may lead to local and regional population declines. West Nile virus mortality in simulations was projected to reduce population growth (i.e., finite rate of increase, λ) of susceptible populations by an average of 0.06–0.09/yr. However, marked spatial and annual fluctuations in nest success, chick survival, and other sources of adult mortality are likely to mask population-level impacts in most years. Impacts of severe outbreaks may be detectable from lek-count data, but documenting effects of low to moderate mortality will require intensive monitoring of radio-marked birds. Resistance to West Nile virus-related disease appears to be low and is expected to increase slowly over time. Eliminating mosquito breeding habitat from anthropogenic water sources is crucial for reducing impacts. Better data are needed on geographic and temporal variation in infection rates, mortality, and seroprevalence rangewide. Small, isolated, and peripheral populations, particularly those at lower elevations, and those experiencing large-scale increases in distribution of surface water may be at higher risk."

Review of:
WEST NILE VIRUS ECOLOGY IN SAGEBRUSH HABITAT AND IMPACTS ON GREATER SAGE-GROUSE POPULATIONS

Review by: Dr. Rob Roy Ramey II

The authors' stated objectives of this paper are:

> *(1) review the ecology of* [West Nile Virus] *WNV in sagebrush ecosystems of western North America,*
> *(2) summarize recent data on distribution of WNV mortality events, impacts on mortality and survival rates, and resistance to WNV disease,*
> *(3) use demographic models to explore potential impacts of WNV related mortality on population growth, and*
> *(4) recommend strategies for monitoring and mitigating impacts of the virus on sage-grouse populations.*

10.1) What is unusual about this paper is the fact that only ten lines of text are dedicated to results (#3 above) and those results are in no way mentioned in the discussion and recommendations (#4). Instead, the paper is primarily a summary of existing literature and unpublished information on WNV and sage grouse.

10.2) The authors have a narrow focus on WNV and sage grouse, and their paradigms are dated. They do not make use of the recent epidemiological literature on WNV and vector control. That literature would alter their conclusions and lead to more effective allocation of conservation effort.

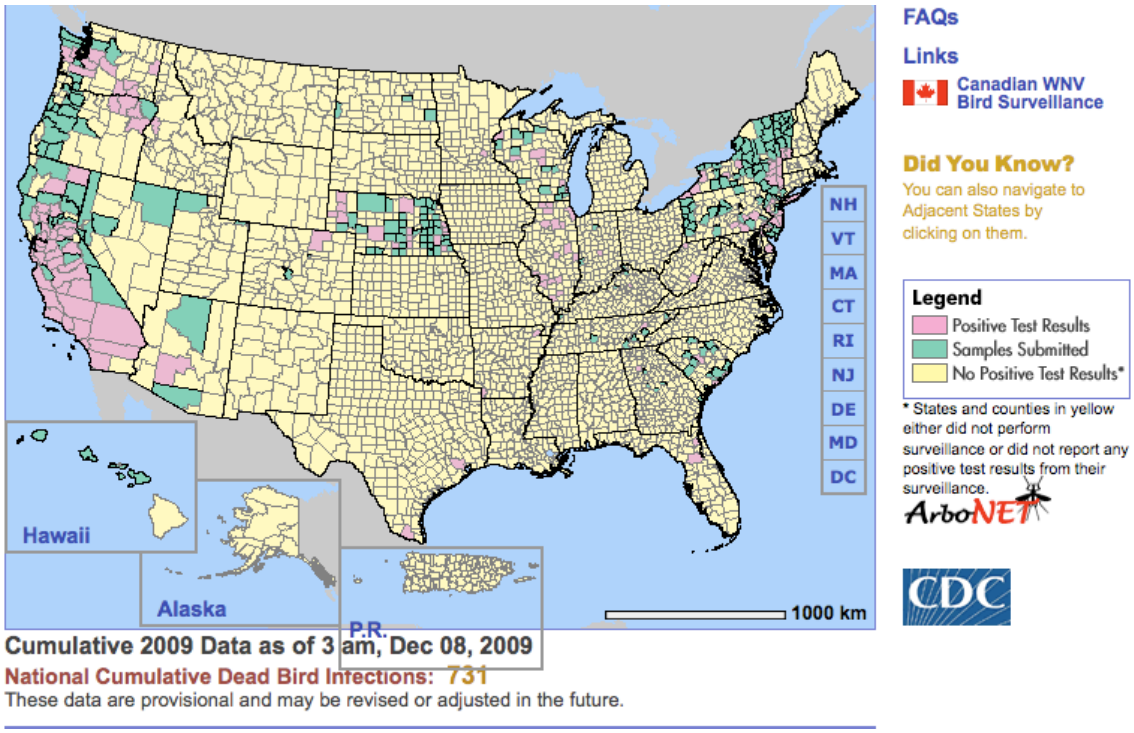
10.3) The role of hunting harvest on vital rates and demography are not mentioned.

10.4) The risk of artificial reservoirs for WNV is overstated, especially because current regulations (e.g. from the BLM) require mosquito control at ponds associated with energy development. In summarizing information from the literature, the authors overstate or misrepresent what the original authors wrote, which detracts from their presentation of valid information.

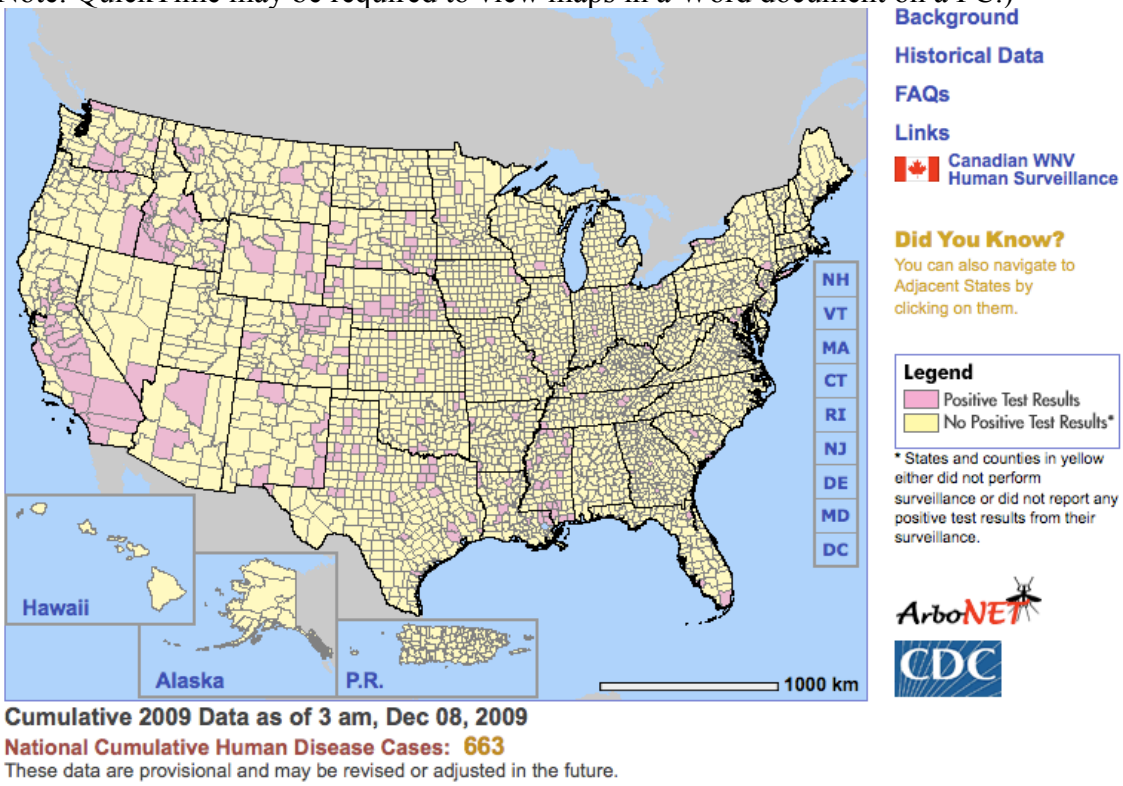
10.5) The authors are correct about the need to monitor populations for WNV mortality, however, there need to be more clear-cut criteria for what constitutes WNV mortality. While clearly WNV is a hazard, the authors do not provide adequate information to evaluate a number of qualitative statements and anecdotal information regarding mortality and extirpations.

10.6) The authors present information on the widespread nature of WNV, and their map (Figure 1) is a composite of multiple years of data. However, annual WNV results from the Center for Disease Control reveal a more heterogeneous pattern of WNV occurrence than portrayed by the authors. Below (and attached) are figures

showing reported WNV human and bird cases in 2009. Maps are available here: http://diseasemaps.usgs.gov/2006/wnv_us_bird.html



(Note: QuickTime may be required to view maps in a Word document on a PC.)



10.7) Hunting mortality not mentioned as an effect. One would think that hunting mortality would be of some importance to calculating vital rates, especially since 9% of the sage grouse population was estimated to have been harvested in 2007.

10.8) The modeling of spread of resistance to WNV among sage grouse is less than what would be expected from population genetic models. The authors' model predicts resistance would not exceed 18% in 20 years. However, from a a population genetics standpoint, if the fitness is due to a single gene, two allele system, and individuals possessing a resistant allele confers a survival advantage of as little as 20% over non resistant genotypes (assuming dominance), then approximately half of the population should be resistant in as few as 10 generations (30 years) given the initial frequency of resistant individuals in the population. A higher level of survivorship by resistant individuals would result in a "selective sweep" of resistant genotype(s) through a population at an even faster rate.

10.9) A conservation strategy to reduce mortality due to WNV, that is not mentioned by the authors, could involve translocating resistant individuals among populations. This would speed the spread of resistant genotypes and reduce the demographic impact of the virus to populations that are not resist.

10.10) The authors are cautious in their view of mosquito control measures but express more optimistic views elsewhere. For example, Walker and Nagle suggest that**:** *"It may also be possible to control mosquitoes with mosquitofish (Gambusia sp.) or native fish species that eat mosquito larvae, biological or chemical larvicides (BTI, Bacillus thuringiensis v. israelensis), or spraying for adults (Doherty 2007)."* And, *"Mosquito control programs appear effective for reducing WNV risk."*

However, the lead author (Walker) expressed a more positive view of the efficacy of these control measures when he testified in his official capacity as Avian Research at the Colorado Division of Wildlife, before the Oil and Gas Commission of the State of Colorado on DOCKET NO. 0803-RM-02. In that testimony, Walker spoke favorably about how the proposed rule would control mosquitoes that vector West Nile virus at water sources associated with energy development. Below are excerpts from his testimony:

> *"Q. HOW WOULD THE PROPOSED RULE HELP SOLVE OR ADDRESS THE PROBLEM**?***
> *A. The proposed rule, for operators to "treat waste water pits and any associated pit containing water that provides a medium for breeding mosquitos with Bti (Bacillus thuringiensis v. israelensis)" or to "take other effective action to control mosquito larvae" will reduce the distribution and abundance of mosquitos that vector West Nile virus and reduce the risk of West Nile virus transmission to wildlife (particularly birds).*
>
> *Mosquito control has proven effective in reducing the risk of West Nile virus transmission, but only when consistently and appropriately implemented by*

qualified mosquito control personnel (Gubler et al. 2000, Reisen and Brault 2007). Hiring qualified mosquito control personnel would ensure effective mosquito control from water sources associated with energy development. Because temperature and mosquito activity both decrease with elevation and mosquito development is temperature-dependent (Reisen et al. 2006), mosquito control would only need to be implemented at elevations where Culex mosquitos occur. Dusky (Blue) Grouse have been documented to have died from West Nile virus infection up to 2100 m (7000 ft.) in Wyoming (T. Cornish, Wyoming State Veterinary Laboratory, unpublished data). Sage-grouse in Colorado have died of West Nile virus up to 5,000 ft. (CDOW data)."

"Requiring control of mosquitos that vector West Nile virus from water sources associated with energy development will reduce risk of West Nile virus transmission for wildlife. This in turn will prevent population impacts to culturally, politically, or economically important bird species, particularly native grouse, and other sensitive and potentially threatened or endangered species."

In fact, the newly issued (Dec 2009) BLM guidelines require mosquito control specifically for WNV:

"Policy Statement 7: West Nile Virus
Artificial water impoundments will be managed to the extent of BLM's authority to prevent the spread of West Nile virus where the virus poses a threat to sage-grouse. This may include but is not limited to: a) the use of larvicides and adulticides to treat reservoirs; b) overbuilding ponds to create non-vegetated and muddy shorelines; c) building steep shorelines to reduce shallow water and aquatic vegetation; d) maintaining the water level below rooted vegetation; e) avoiding flooding terrestrial vegetation in flat terrain or low lying areas; f) constructing dams or impoundments that restrict seepage or overflow; g) lining the channel where discharge water flows into the pond with crushed rock, or use a horizontal pipe to discharge inflow directly into existing open water; h) lining the overflow spillway with crushed rock and construct the spillway with steep sides to preclude the accumulation of shallow water and vegetation; and i) restricting access of ponds to livestock and wildlife (Doherty 2007).
Field Offices should consider alternate means to manage produced waters that could produce vectors for West Nile virus such as injection under an approved UIC permit, transfer to single/centralized facility, etc.

This does not apply to naturally occurring waters. Impoundments for wildlife and/or livestock use should be designed to reduce the potential to produce vectors for West Nile Virus where the virus may pose a threat to sage-grouse. "

It is difficult to understand the full extent of mortality due to WNV and there are numerous unpublished sources and personal communications cited. A more compelling presentation of the evidence would be to collate these data into a table, especially the number of birds sampled and found infected, and the methods

used to determine of WNV caused mortality. The use of anecdotal evidence in some cases detracts from the author's case (e.g. the case of 60 carcasses found by landowners and a WNV hawk nearby, and the authors assuming the sage grouse all died of WNV.)."

10.11) The authors portray the ponds associated with energy development as uncontrolled breeding habitat for mosquitos. This ignores the fact that regulations currently exist to control mosquitos, thus greatly reducing or virtually eliminating this hazard. The authors also ignore literature showing the more pronounced effect of irrigation on mosquito populations. For example, the authors state: *"Man-made water sources may also facilitate the spread of WNV within sage-grouse habitats (Zou et al. 2006b, Doherty 2007, Walker et al. 2007b). For example, construction of ponds for water produced during coal-bed natural gas extraction increased larval mosquito habitat around pond edges by 75%, from 619 to 1,085 ha, during a 5-yr period of development (1999–2004) across a 21,000-km2 area of northeastern Wyoming (Zou et al. 2006b). These ponds support abundant Culex tarsalis, and they support them longer than natural, ephemeral water sources (Doherty 2007)."*

This thesis (Doherty 2007) is based on research conducted on WNV before the requirement for mosquito control programs in ponds associated with energy development (the study took place in 2004 and 2005). It showed that while coal bed methane production had increased pond water in the Powder River Basin of Wyoming, it was not the major contributing factor to the spread of the vector for WNV: *"Culex tarsalis, the vector responsible for transmitting WNV in northeastern Wyoming, is a native species of mosquito to the PRB (Hayes 2005, Turell et al. 2005); however their population levels have increased in some areas due to human development in both agriculture and CBNG fields. This in combination with my research data allows me to reject my hypothesis that CBNG development has increased mosquito production in the PRB including the WNV vector Cx. tarsalis. "*

Also, empirical data show that *"for every tenth of a percent rise in irrigated land, the incidence of disease is expected to increase by a factor of 1.50 for people and 1.63 for veterinary species"* (Gates and Boston 2009). As reported by Doherty (2007) irrigated land has had an effect on WNV, not just oil and gas production (see comment 11 above).

10.12) The authors rely on dated information regarding biological control of WNV mosquitos vectors. However, new recombinant bacterial larvicides (Federici 2010) significantly improve the efficacy of Bacillus thuringiensis subsp. Israelensis (Bti) currently used to control larvae of important mosquito disease vectors. According to this author, these *"combine the most potent insecticidal proteins from Bti, B. thuringiensis subsp. Jegathesan (Btj), and B. sphaericus (Bs) into new bacterial strains that are ten-fold more toxic than wild type species of Bti and Bs used in current commercial formulations."*

10.13) The authors suggest that: *"Increasing temperatures associated with changing climate may exacerbate WNV risk for sage-grouse"* however, they do not present

evidence as to predicted rate of temperature increase, nor the confounding effects of predicted precipitation changes resulting from climate change.

10.14) Empirical data have shown that host species diversity reduces the spread of WNV because the vector encounters fewer competent reserviors. This source of variation in attenuating WNV activity is not mentioned by the authors (Allan et al. 2009).

10.15) The authors suggest that little is known about the reservoirs for WNV, however, that is not true given the abundance of recent papers on the subject, not cited by the authors. For example, the authors state: *"Much is known about WNV vectors in sagebrush habitat, but reservoirs for WNV are poorly understood."* Juxtapose that statement with a recent (2009) paper on WNV epidemiology published in the Journal of Preventive Veterinary Medicine: "*Experimental studies have identified that the common grackle is a highly competent avian reservoir for WNV, with the second highest reservoir index ranking found among 25 bird species tested, with a higher resistance to WNV mortality among infected individuals than observed in most other passerine species tested."*

10.16) The authors twice reference repeated attempts for a sage grouse ESA listing. The authors of this paper make the point twice that there have been repeated attempts to list the sage grouse under the Endangered Species Act and they do not acknowledge that those petitions were made by advocacy organizations rather than scientific organizations, and that listing was denied based on evaluation of relevant scientific information.

"Previously widespread, both species of sage grouse have been extirpated from much of their original range (Schroeder et al. 2004) and experienced long-term population declines due to loss, fragmentation, and degradation of sagebrush habitat (Connelly et al. 2004). <u>This has precipitated repeated attempts to list the species under the Endangered Species Act and rangewide efforts to assess risks to populations (Connelly et al. 2004, Stiver et al. 2006, Aldridge et al. 2008)."</u>

"Historical population declines and range contraction and continued loss and degradation of sagebrush habitat have led to concern over the conservation status of sage-grouse (Schroeder et al. 1999, Connelly et al. 2004, Schroeder et al. 2004, Stiver et al. 2006) and <u>repeated attempts to list both species under the Endangered Species Act of 1973.</u> Understanding the impact of WNV on Greater Sage-Grouse populations is important for assessing this species' conservation status, but requires an updated synthesis of recent scientific data." However, the authors do not mention how that conservation status would be determined based on their analysis, and the only dedicate ten lines of text to their results.

Naugle et al. (2004) previously made the following provocative comment regarding an ESA listing: *"The emergence of WNV further complicates the difficult task of conserving sage-grouse in western North America. Efficacy of mosquito control with pesticides over vast areas of sage-grouse range remains untested, and the suggestion of land-use change*

only fuels conflict over water management in the west. Petitions to list sage-grouse under the federal Endangered Species Act are intended to force decisions on issues that could change the management of public and private lands. Regardless, if we are to prevent sage-grouse from going extinct on their remaining range, we must find a way to provide high-quality habitats that support robust, genetically diverse populations capable of withstanding stochastic disease events."

Chapter 11 (2009), Chapter 10 (2011):
CHARACTERISTICS OF SAGEBRUSH HABITATS AND LIMITATIONS TO LONG-TERM CONSERVATION

Authors: Richard F. Miller, Steven T. Knick, David A. Pyke, Cara W. Meinke, Steven E. Hanser, Michael J. Wisdom, and Ann L. Hild

Abstract from Miller et al:
"The distribution of sagebrush (Artemisia spp.) within the Sage-Grouse (Centrocercus spp.) Conservation Area (SGCA; the historical distribution of sage-grouse buffered by 50 km) stretches from British Columbia and Saskatchewan in the north, to northern Arizona and New Mexico in the south; and from the eastern slopes of the Sierra Nevada and Cascade mountains to western South Dakota. The dominant sagebrush (sub)species as well as the composition and proportion of shrubs, grasses, and forbs varies across different ecological sites as a function of precipitation, temperature, soils, topographic position, elevation, and disturbance history. Most important to Greater Sage-Grouse (Centrocercus urophasianus) are three subspecies of big sagebrush (Artemisia tridentata)—basin big sagebrush (Artemisia tridentata ssp. tridentata), Wyoming big sagebrush (A. t. ssp. wyomingensis), and mountain big sagebrush (A. t. ssp. vaseyana); two low (or dwarf) forms—little sagebrush (A. arbuscula), and black sagebrush (A. nova); and silver sagebrush (A. cana), which occurs primarily in the northeast portion of the sage-grouse range. Invasive plant species, wildfires, and weather and climate change are major influences on sagebrush habitats and present significant challenges to their long-term conservation. Each is spatially pervasive across the Greater Sage-grouse Conservation Area and has significant potential to influence processes within sagebrush communities. Cheatgrass (Bromus tectorum), the most widespread exotic annual grass, has invaded much of the lower elevation, more xeric sagebrush landscapes across the western portion of the Greater Sage-Grouse Conservation Area. A large proportion of existing sagebrush communities is at moderate to high risk of invasion by cheatgrass. Juniper (Juniperus spp.) and pinyon (Pinus spp.) woodlands have expanded into sagebrush habitats at higher elevations creating an elevational squeeze on the sagebrush ecosystem from both extremes. Number of fires and total area burned have increased since 1980 throughout the Greater Sage-Grouse Conservation Area except in the Snake River Plain, which has a long-term history of high fire disturbance. Climate change scenarios for the sagebrush region predict increasing trends in temperature, atmospheric CO_2, and frequency of severe weather events that favor cheatgrass expansion and increased fire disturbance resulting in a decline in sagebrush. Approximately 12% of the current distribution of sagebrush is predicted to be replaced by expansion of other woody vegetation for each 1 C increase in temperature. Periodic drought regularly influences sagebrush ecosystems; drought duration and severity have increased throughout the 20th Century in much of the interior western US. Synergistic feedbacks among invasive plant species, fire, and climate change coupled with current trajectories of habitat changes and rates of disturbance (natural and human-caused) will continue to change sagebrush communities and create challenges for future conservation and management."

Review of:
CHARACTERISTICS OF SAGEBRUSH HABITATS AND LIMITATIONS TO LONG-TERM CONSERVATION

Review by: Dr. Vernon C. Bleich

This very well written manuscript describes the distribution of sagebrush (*Artemisia* spp.) within the Greater Sage-Grouse (*Centrocercus urophasianus*) Conservation Area (SGCA; the historical distribution of sage-grouse buffered by 50 km). The distribution of the various species and subspecies of *Artemisia* are described in the context of edaphic, climatological, and topographic parameters. The authors note that, in general, ecological sites supporting sagebrush are among the most imperiled ecosystems in North America, and that few, if any landscapes remain intact and unchanged throughout the SGCA. It seems to be implicit in the descriptions of impacts already incurred that these ecosystems cannot remain functional and that, in order for functionality to persist, the systems must be restored. The authors further note that sagebrush habitats in the Columbia Basin, Northern Great Basin, Snake River Plain, Wyoming Basin, Southern Great Basin, and Silver Sagebrush floristic provinces are of primary importance to Greater Sage-Grouse. A strength of this paper is that it primarily is a compilation of the work of others investigators, and the authors have pulled information from a large number of sources together in a single contribution in their effort to describe sagebrush habitats.

11.1) The authors describe in detail the distribution of sagebrush, and provide a general description of sagebrush alliances and plant associations, all of which will be useful to those with an interest in the conservation or restoration of these areas. Further, they focus on three (of approximately 25; Wisdom et al. 2005b)) stressors that represent significant threats to the maintenance of sagebrush ecosystems. These stressors are invasive plants, wildfire, and climate. The authors did not prioritize the importance of these stressors, but such might be inferred from the order in which they were discussed. Of the three, it is my personal opinion that invasive plants would be priority one, because their presence alters the entire fire regime in sagebrush systems, making them more prone to wildfire. The absence of fire in some areas, potentially an initial result of overgrazing, and later fire suppression, has decreased competition for coniferous species, primarily pinyon and juniper. I would place climate change in last among these threats because it is (1) speculative; (2) the effects can only be modeled; (3) if it occurs to the extent that ecosystem-level changes result, it probably will be well into the future; and, (4) it may not become reality.

11.2) Prior to exploring the impacts, whether real or potential, of invasive plants, changes in the fire regime, and climate change, the authors discuss the long-term and short-term dynamics of sagebrush habitats. Long-term dynamics that occurred prior to settlement by western Europeans were influenced primarily by long-term changes in climate or severe disturbances, likely occurred over periods ranging from centuries to several millennia, and resulted in changes in abundance between sagebrush and graminoids, and the distribution of pinyon, juniper, sagebrush, grassland, and salt desert communities. Short-term dynamics, which occur over years or decades, are largely a function of weather or

disturbance that result in fluctuation or permanent change in relative abundance of species and structure of plant communities. Representative of such changes is the widespread distribution of cheat grass (*Bromus tectorum*) among sagebrush ecosystems in western North America.

11.3) The authors attempt to convey some measures of changes in the distribution of sgebrush that have occurred by comparing the current distribution of sagebrush habitats (using high-resolution and sophisticated mapping techniques) to the potential vegetation map for Great Basin, sagebrush steppe, and wheatgrass-needlegrass shrubsteppe. The authors appropriately acknowledge the problematic nature of comparing the differences between the potential vegetation type that could occur, and what currently was present. Further, the authors also appropriately noted that their attempt to identify broad-scale differences between current and potential distribution of sagebrush was not intended to identify specific locations where habitat for sage-grouse had been lost. Nevertheless, the authors note substantial differences between the amount of area currently mapped as sagebrush and the potential distribution of sagebrush based on maps prepared by A. W. Kuchler (1970). Given that Kuchler's maps depicted "potential" vegetation, a downward bias in the estimate of the proportion of sagebrush habitat currently present, when compared to potential vegetation, is probable. If we assume that Kuchler's maps depicted only the potential for sagebrush vegetation (which is implied by the title), the actual amount of sagebrush may have been less than actually available. If such was the case, the proportion of sagebrush habitat remaining would actually be greater than if Kuchler's maps reflected the true amount of sagebrush habitat. Hence, there would be a downward bias in the amount of sagebrush remaining if maps of "potential" sagebrush habitat are the basis for the calculations.

11.4) The authors have summarized reports of invasive species and their impacts to sagebrush habitats, but their measure of the areas impacted by those invaders may be biased upwards because, in the database used, counties are considered occupied by an invasive plant if even one occurrence of a species has been verified in those counties. The authors acknowledge that invasive species may be widely distributed, but that infestations likely are localized because of the narrow ecological requirements of those invaders.

11.5) Cheatgrass and medusahead were identified as among the most problematic of invasive exotic plants in sagebrush habitats, and cheatgrass has been a major factor in the loss of Wyoming big sagebrush communities. Between them, cheatgrass and medusahead dominate, or have a significant presence, on approximately 20% (70,000 of 400,000 km^2) of public lands surveyed. The authors also correct a major overestimate of the proportion of lands now dominated by cheatgrass, an important point to make. Indeed, Whisenant (1990) had misinterpreted the report of Mack (1981), who had indicated that cheatgrass dominates on many rangelands within 400,000 km^2 of potential steppe vegetation, and wrote that cheatgrass was the major herbaceous species on more than 400,000 km^2 of the West. The true figure varies from area to area, ranging from about 70,000 km^2 in Washington, Oregon, Idaho, Nevada, and Utah (Pellant and Hall

1994) to about 20,000 km^2 in the northern Great Basin (Bradley and Mustard 2005, 2006; Peterson 2005).

11.6) As an original contribution, the authors modeled the probability of cheatgrass occurring among 5 floristic provinces of the intermountain west, and determined a moderate to high probability of cheatgrass presence throughout much of the intermountain west. They reported that approximately 54% of the 1,500,000 km^2 Great Basin ecoregion has environmental conditions suitable for cheatgrass invasion, and that 38% of existing sagebrush was at moderate risk, and 20% was at high risk. Impacts of cheatgrass to sagebrush ecosystems occur primarily as a result of the heavy fuel load that results from cheatgrass invasion, and the fact that, with few exceptions, sagebrush species are not fire adapted and are destroyed by fire. Moreover, fire affects the distribution of sagebrush seeds, with fewer, more widely dispersed seed sources in remaining unburned sagebrush islands. Thus, native species die, and are not replaced at the same rate they re destroyed; as a result, native species can eventually be eliminated from the species pool in areas dominated by exotic species that increase the frequency and severity of fire. The authors go on to discuss additional ecosystem-level changes associated with invasions of exotic annual grasses, citing largely reports by other researchers.

11.7) Substantial woodland expansion, specifically by pinyon (*Pinus* spp.) and juniper (*Juniperus* spp.) is occurring as those conifers encroach and infill large portions of sagebrush habitat, particularly at higher elevations. These expansions co-occurred with the introduction of livestock, and surface fire exclusion. As tree cover increases, cover of sagebrush and associated shrubs declines, and the authors cite evidence (Tausch et al. 1981, Johnson and Miller 2006, Miller et al. 2008) that as much as 90% of areas currently dominated by pinyon and juniper were predominantly sagebrush types prior to the late 1800s, with the majority of encroachment occurring at higher elevations. The level of uncertainty associated with that estimate is unclear, but they conclude that millions of hectares of potential sagebrush vegetation types are at high risk of displacement by conifer invasion. In this situation it should be noted that this value may be biased upward because the impacts are presented in the context of "potential" sagebrush habitat. Nevertheless, the authors note that pinyon and juniper currently occupy far less land than their potential under current climatic conditions, and that tree densities continue to increase, resulting in the continued loss of sagebrush habitat.

11.8) The authors describe the characteristics of fire and fire regimes and, to their credit, note that a clear picture of the complex spatial and temporal patterns of historic fire regimes prior to European settlement is unlikely. They note, however, that early explorers reported fires in higher elevations, but seldom reported fires in the sagebrush valleys at lower elevations. They also argue that prior to settlement most Wyoming big sagebrush communities in the Intermountain West generally did not carry fires except under extreme conditions of low humidity or high winds. Invasion of cheatgrass has facilitated the spread and intensity of wildfire, and is suspected to have substantially altered the historical fire regime.

Ironically, the absence of fire or the decline of fire frequency, has likely been an important factor in the expansion of coniferous trees into areas formerly dominated by sagebrush. The initial decline in fire frequency was associated with the early introduction of sheep, goats, and cattle, and occurred prior to fire suppression efforts that now are commonplace. Livestock grazing is thought to have reduced the availability of fine (native) fuels, and the authors speculate that heavy use by livestock reduced fire occurrence across western landscapes during the late 1800s, thereby facilitating the proliferation of pinyon and juniper species. They acknowledge, however, that evidence for a direct relationship between livestock grazing and woodland encroachment is difficult to document.

11.9) As an original contribution, the authors developed a database of fire statistics, and tested for changes in the frequency and size of fires, and in the total area burned in each of the geographic subdivisions of the SGCA. Among these areas, size of fires and total area burned increased in each, with the exception of the Snake River Plain, but average fire size increased only in the southern Great Basin. Within-year fire size decreased in all geographic regions except the Snake River Plain, which has a long history of presence of cheatgrass, and has been well-defined by fires. Consistent with the hypothesis that cheatgrass invasion affects the fire regime is the result that fires within the cheatgrass region have been more pronounced since 1980, but that fires in the eastern section of the SGCA have been recorded only in more recent years.

11.10) The authors speculate that the postulated changes in global climate will impact sagebrush dominated ecosystems, which are well-adapted to aridity, further altering those systems. If substantial alterations to climate occur, changes in the amount and timing of rainfall, and in increases in temperature are predicted to result in the displacement of sagebrush from large areas currently dominated by that species, as the competitive advantage of plants that currently are frost-sensitive is enhanced, and they are able to extend their ranges northward into areas not currently within their distributions. The authors also speculate that the ability of cheatgrass to compete in sagebrush ecosystems affected by increased annual precipitation or temperature will facilitate the spread of that annual plant, and further exacerbate the cycle of fire and cheatgrass dominance. Finally, the authors cite the report of Neilson et al. (2005) that, in the event of global warming, each 1° C increase in temperature is expected to result in the loss of 87,000 km^2 of existing sagebrush habitat, and that if a 6.6° C change occurs, only 20% f the current sagebrush distribution would remain.

11.11) The authors have presented a useful summary of information regarding the taxonomy, distribution, and composition of sagebrush dominated ecosystems occurring in western North America. They have described the primary threats to these systems as the presence and continued spread of exotic annual grasses, encroachment of pinyon and juniper into areas formerly dominated by sagebrush, and changes associated with hypothesized shifts in global climate. Shifts in fire regimes (in the form of more frequent and larger fires) are anticipated to continue to occur as a result of the spread of annual grasses, with resultant impacts to sagebrush-dominated ecosystems. Similarly, altered fire regimes (with fires occurring at lower frequencies) will continue to result in

encroachment of pinyon and juniper into areas currently dominated by sagebrush. Both scenarios of altered fire regimes that have resulted in conifer expansion at high elevation interfaces, and in exotic weed encroachment at lower elevations have caused significant losses in the amount of area dominated by sagebrush, with resultant losses of habitat potentially inhabited by Greater Sage-Grouse. The authors also note the potential for changes in global climate to exacerbate changes in fire regimes, and resultant losses of sagebrush habitat, but note that projections of the effects of global warming become increasingly unreliable as they extend further into the future.

11.12) Sagebrush habitats appear to have been impacted in modern times, and the authors postulate a continuing decline as a result of invasive annual species, invasion and displacement of sagebrush by coniferous species, shifts in fire regimes, and will be altered (potentially) further by climate change. These conclusions are based largely on the works of others, which are nicely summarized in this contribution. The paper will be useful to those concerned with changes that have occurred in the distribution and amount of sagebrush-dominated ecosystems, the mechanisms by which changes likely have occurred, and the potential for climate change to further exacerbate shifts in the composition and distribution of sagebrush. The authors have been open and forthcoming in noting the limitations of their original calculations and models, as described above, and which are appreciated by the reviewer. This paper is a potentially valuable addition to the literature, but it seems less than appropriate for the authors to have made reference to "accelerated declines on [sic] many sage-brush-dependent species..." given that their approach was almost entirely from a botanical standpoint. I think it would have been of greater value for other authors to use this review paper to emphasize the potential for the changes described by Miller et al. to make that point, especially in the context of other stressors associated with development or alteration of areas dominated by sagebrush.

Chapter 12 (2009), Chapter 11 (2011):
PRE-EUROAMERICAN AND RECENT FIRE IN SAGEBRUSH ECOSYSTEMS

Author: William L. Baker

Abstract from Baker:
"Sagebrush (Artemisia spp.) ecosystems are under threat from a variety of land uses, disturbance, invasive species, and are also thought by some to have been affected by fire exclusion and require burning as a part of restoration. To better understand the historical range of variation (HRV) of fire in sagebrush ecosystems and whether sagebrush fire regimes today have too much or too little fire, I estimate fire rotation (expected time to burn the area of a landscape) in sagebrush ecosystems under the HRV. Estimates derived from five sources are >200 yr in little sagebrush (Artemisia arbuscula), 200–350 yr in Wyoming big sagebrush (A. tridentata ssp. wyomingensis), 150–300 yr in mountain big sagebrush (A. tridentata ssp. vaseyana), and 40–230 yr in mountain grasslands containing patches of mountain big sagebrush with longer rotations in areas where sagebrush intermixes with forests. Landscape dynamics under the HRV were likely dominated in all sagebrush areas by infrequent episodes of large, high-severity fires followed by long interludes with smaller, patchier fires, allowing mature sagebrush to dominate for extended periods. Fire rotation, estimated from recent fire records, suggests fire exclusion had little effect on fire in sagebrush ecosystems. Instead, cheatgrass (Bromus tectorum), human-set fires, and global warming may have led to too much fire relative to the HRV in four floristic provinces within the range of sagebrush in the western US. Sagebrush ecosystems would generally benefit from rest from disturbance. Global warming is likely to increase fire, and widespread prescribed burning of sagebrush is unnecessary. Where cheatgrass occurs, fire suppression is sensible. In areas of depleted understories, restoration to re-establish native plants is needed if sagebrush ecosystems are to effectively recover from future disturbance".

Review of:
PRE-EUROAMERICAN AND RECENT FIRE IN SAGEBRUSH ECOSYSTEMS

Review by: Dr. Rob Roy Ramey II

12.1) This paper provides a summary of information on fire frequency and estimates periodicity of large wildfires based on historic information. As noted by the author, smaller fires may not be revealed by that record. A larger problem with attempting to estimate average fire frequency on a time scale greater that 150 years is that the climate has fluctuated over time, with cooler and drier climate from ~1400 to 1850 as a result of the little Ice Age. As noted by the author, over the long term, *"as the climate became wetter and sagebrush increased, so did the fire frequency (Fig. 2)."*Therefore, while such fire frequency estimates may be useful approximations, they must be viewed with caution. The author questions the use of prescribed fire as a restoration tool and suggests that fire frequency has increased as a result of invasive cheatgrass in combination with human-set fires and global warming.

12.2) The author's suggestion that "ecosystem rest" should be part of a management program, is undefined: *"If the goal is to mimic the disturbance regime in sagebrush under the HRV, <u>these ecosystems need rest and recovery</u> from past disturbances, particularly disturbances by land uses (Knick et al., this volume) and fire, not additional disturbance."* The length of time necessary for "rest and recovery" from past disturbance is not specified.

12.3) The most relevant finding to energy development has to do with the need to prevent wildfire and suppress it when it occurs. This was summarized in the final paragraph:

> *"Where the management goal is protection, active fire control is sensible wherever cheatgrass occurs. This includes much of the range of Wyoming big sagebrush and at least the lower elevations of the mountain big sagebrush zone. These sagebrush areas are vulnerable to potentially irreversible replacement by cheatgrass following fire, leading to sagebrush regeneration failure (Pellant 1990, Fig. 1c). Current fire rotations are likely too short in these areas to allow full recovery of Wyoming big sagebrush after fire. These areas warrant complete protection from fire until a solution is found to effectively control cheatgrass and until plant diversity can be sufficiently restored to allow natural recovery after fire."*

Chapter 13 (2009), Chapter 12 (2011):
ECOLOGICAL INFLUENCE AND PATHWAYS OF LAND USE IN SAGEBRUSH

Authors: Steven T. Knick, Steven E. Hanser, Richard F. Miller, David A. Pyke, Michael J. Wisdom, Sean P. Finn, E. Thomas Rinkes, and Charles J. Henny

Abstract from Knick et al.:
"Land use in sagebrush (Artemisia spp.) landscapes influences all sage-grouse (Centrocercus spp.) populations in western North America. Croplands and the network of irrigation canals cover 230,000 km2 and indirectly influence up to 77% of the Sage-Grouse Conservation Area and 73% of sagebrush land cover by subsidizing synanthropic predators on sage-grouse. Urbanization and the demands of human population growth have created an extensive network of connecting infrastructure that is expanding its influence on sagebrush landscapes. Over 2,500 km2 are now covered by interstate highways and paved roads; when secondary roads are included, 15% of the Sage-Grouse Conservation Area and 5% of existing sagebrush habitats are >2.5 km from roads. Density of secondary roads often exceeds 5 km/km2, resulting in widespread motorized access for recreation, creating extensive travel corridors for management actions and resource development, subsidizing predators adapted to human presence, and facilitating spread of exotic or invasive plants. Sagebrush lands also are being used for their wilderness and recreation values, including off-highway vehicle use. Approximately 12,000,000 s animal use months (AUM = amount of forage to support one livestock unit per month) permitted for grazing livestock on public lands in the western states. Direct effects of grazing on sage-grouse populations or sagebrush landscapes are not possible to assess from current data. However, management of lands grazed by livestock has influenced sagebrush ecosystems by vegetation treatments to increase forage and reduce sagebrush and other plant species unpalatable to livestock. Fences (>2 km/km2 in some regions), roads, and water developments to manage livestock movements further modify the landscape. Oil and gas development influences 8% of the sagebrush habitats with the highest intensities occurring in the eastern range of sage-grouse; >20% of the sagebrush distribution is indirectly influenced in the Great Plains, Wyoming Basin, and Colorado Plateau management zones. Energy development physically removes habitat to construct well pads, roads, power lines, and pipelines; indirect effects include habitat fragmentation, soil disturbance, and facilitation of exotic plant and animal spread. More recent development of alternative energy, such as wind and geothermal, creates infrastructure in new regions of the sage-grouse distribution. Land use will continue to be a dominant stressor on sagebrush systems; its individual and cumulative effects will challenge long-term conservation of sage-grouse populations."

Review of:
ECOLOGICAL INFLUENCE AND PATHWAYS OF LAND USE IN SAGEBRUSH

Review by: Dr. Rob Roy Ramey II

This lengthy (162 page) paper presents another cumulative effects analysis that covers nearly every conceivable deleterious human activity on sagebrush and sage grouse.

13.1) Notably absent from this analysis is any mention of the effects of hunting harvest, even though this is a major, documented source of sage grouse mortality (with 207,430 grouse killed just between 2001 and 2007, and higher annual take in the preceding years). Instead, the authors devote pages of attention to a number of hypothetical effects: *"Even activities, such as hiking and mountain biking, which often are perceived as low impact or benign, have an influence wildlife (Miller et al. 1998, Taylor and Knight 2003). Any human activity of high frequency along established roads or corridors, whether motorized or non-motorized, can affect wildlife habitats and species negatively through habitat loss and fragmentation, facilitation of exotic plant spread, population displacement or avoidance, establishment of population barriers, or increased human-wildlife encounters that increase wildlife mortality (Gaines et al. 2003). These effects appear to be common across a variety of habitats and species that span the full range of forested to arid terrestrial environments (Gaines et al. 2003, Ouren et al. 2007)."* However, when one looks closely at the cited literature, these supposed population-level effects are speculative. The omission of documented sources of mortality and inclusion of speculative sources, indicate a less than objective analysis by Knick et al.

13.2) To quantify the influence of human activities on patterns and processes of sagebrush habitats and sage-grouse populations, the authors rely on the previously designated Sage-Grouse Conservation Area or the pre-settlement distribution of sage-grouse buffered by 50 km (Connelly et al. 200; Schroeder et al. 2004). As noted in the reviews of Schroeder et al. (2004), the pre-settlement distribution was a subjective assessment of pre-European sage grouse distribution that included both habitat and non-habitat, and selectively excluded some areas of documented sage grouse occupancy. The widening of the pre-settlement range by a 50km "buffer" (by Knick et al.) inflates the size of the area affected by human activities, even though sage grouse may have never occurred there.

13.3) As with other disturbances in sage grouse habitat, Knick et al. quantify the "effect area" that surrounds any kind of development based on other studies. In the case of oil and gas wells, the effect area includes a 3km buffer around each well pad, and the affected area of a pipeline was 3km in total width because of presumed spread of invasive plants (although Table 16 shows in many cases the authors used a higher figure). A 3km effect area was also applied to all transmission lines. These effect areas were applied across the study area, substantially inflating the effects of these activities, even if mitigation, such as conservation offsets, had been implemented. However, the cited paper for oil and gas construction (Lyon and Anderson 2003) made no such 3km

recommendations. They simply recommended that the BLM regulations in place at the time be "reexamined." Knick et al. also misrepresented cited studies regarding the affected area of roads, pipelines, and transmission lines. Examples are provided below:

1) Lyon and Anderson (2003) also reported observations contrary to the one-size-fits-all effect areas used by Knick et al. For example, Lyon and Anderson (2003) reported that:

"On the Pinedale Mesa, potential disturbances associated with natural gas development were restricted to vehicular traffic on the pre-existing main haul road. All males from the 3 disturbed leks in our study strutted either on or within 15 m of this road. However, the mean number of vehicles using the mesa road in a 24-hour period during spring and summer of 1998 and 1999 was <12."

2) Instead of reporting a 3km effect area, Bradley and Mustard (2006) instead reported the following limited effects from roads and transmission lines:

"In 2001, cheatgrass was 20% more likely to be found within 3 km of cultivation, 13% more likely to be found within 700 m of a road, and 15% more likely to be found within 1 km of a power line."

3) Similarly, instead of finding a 3km effect area, Gelbard and Belnap (2003) reported:

"...we observed anecdotally that sites isolated (1000 m) from roads tended to contain fewer exotic species than sites near (50 m from) road."

"We found a significant effect of road improvement on both exotic and native species richness in interior communities 50 m beyond the edge of the road cut, suggesting that road improvement influences the distribution of both exotic and native species in lands beyond the influence of roadside disturbance. Exotic species richness tended to be greater and native species richness tended to be lower next to more improved roads, although we caution that our measurements of richness were a snapshot."

Knick et al. stated that *"We used an ecological rationale for estimating the area around points, lines, or polygons from which land use potentially influenced land cover or sage-grouse populations. Estimates for effect sizes into surrounding areas were based on foraging movements of human-subsidized predators, distance of exotic plant species spread, or on distribution data relative to land use."* However, because of the misrepresentations detailed above, this reviewer recommends that other "effect sizes" and "ecological rationale" used by Knick et al. be closely reexamined.

According to Knick et al. *"All nonproprietary and nonsensitive spatial data sets used in our analysis are available for download on the SAGEMAP website http://sagemap.wr.usgs.gov; United States Department of the Interior 2001a). Each data set is accompanied by a metadata record documenting original source and GIS procedures."* It is presently unknown how much of the data are proprietary or sensitive.

Chapter 14 (2009), Chapter 13 (2011):
INFLUENCES OF THE HUMAN FOOTPRINT ON SAGEBRUSH LANDSCAPE PATTERNS: IMPLICATIONS FOR SAGE-GROUSE CONSERVATION

Authors: Matthias Leu And Steven E. Hanser

Abstract from Leu and Hanser:
"Sagebrush (Artemisia spp.) ecosystems in the western US have changed in quantity and configuration from a variety of causes including agriculture and human population growth since Euro-American settlement. Activities sustaining human society can decrease or fragment land cover and alter ecological processes within sagebrush systems. The extent of these activities, cumulatively called the human footprint, within the range of sage-grouse (Greater Sage-Grouse [Centrocercus urophasianus] and Gunnison Sage-Grouse [C. minimus]) has not been evaluated. Using a recent human-footprint model of the western US, we evaluated human footprint intensity (1) across the sage-grouse range within seven sage-grouse management zones (SMZ), (2) across five sagebrush land-cover classes and a non-sagebrush land-cover class within SMZ, and (3) on landscape pattern of sagebrush land cover in relation to three scenarios differing in human-footprint effect area. Based on four criteria, we ranked SMZ from most to least human footprint influence as follows: Columbia Basin, Colorado Plateau, Wyoming basins, Great Plains, Snake River Plain, southern Great Basin, and northern Great Basin. Range-wide, black (Artemisia nova) and little (A. arbuscula) sagebrush land covers were least affected by the human footprint. Increasing human-footprint effect area decreased sagebrush land cover in the landscape between 33.5% and 97.0% and reduced mean patch size by 18.7–60.5%. A landscape pattern analysis, using a lacunarity index, or measure of sagebrush patchiness, revealed sagebrush landscapes to be multi-scaled, with dispersed sagebrush patches at small and clumped distributions at large scales, and organized at a scale between 4.5–9.0 km. This scale overlaps with published sage-grouse average dispersal and movement patterns. Our study supports growing evidence that sage-grouse respond to environmental factors at larger scales than those currently applied in management."

Review of:
INFLUENCES OF THE HUMAN FOOTPRINT ON SAGEBRUSH LANDSCAPE PATTERNS: IMPLICATIONS FOR SAGE-GROUSE CONSERVATION

Review by: Dr. Rob Roy Ramey II

This paper utilizes a complex spatial analysis to predict impact of the "human footprint" on sagebrush habitat (termed *"sagebrush landscape"* by the authors). This is the same approach used previously to describe the "human footprint" across the west, by two of the same authors as Leu et al. (2008). The third author of Leu et al. (2008), is Knick, also an editor and frequent contributor to this sage grouse monograph.

The paper contains considerable jargon, making a comprehensive read a time-consuming task.

14.1) The model used to study the "human footprint" is dependent upon the inputs of other models, but the error associated with these inputs, and their effect on results, were not addressed by Leu and Hanser. Use of the terms "error," "uncertainty," and "confidence interval" are absent from this paper. The authors did not appear to us statistical methods that deal with stochastic variation to estimate the magnitude of the error variance and propagate it through to the confidence intervals.

14.2) The significance of this paper lies in its likely utilization by the USFWS for a range wide or regional "cumulative effects analysis" of various human land uses and activities on sage grouse. Therefore, a more in-depth review of this paper may be desirable. The authors describe their approach as: *"The cumulative effects of human actions on landscapes, the human footprint, can be delineated as the physical and/or ecological human footprint."*

14.3) In this paper, as with Leu et al. (2008) no hypotheses are tested. Instead, the authors rely a *post hoc* interpretation of results and make recommendations derived from their complex spatial analysis. That paper interprets the results using a descriptive, story-telling approach. The authors recommend that certain landscapes in a given human footprint class be *"carefully evaluated,"* although the criteria by which such an evaluation would be objectively conducted is not described. The results are deemed supportive of those obtained by other authors in this volume, however no criteria were provided that would potentially falsify previous conclusions. The authors believe raven control to be ineffective and suggest that all future transmission lines follow existing high impact corridors, an expensive proposition to be based on surmise.

14.4) The size of the affected area surrounding each type of land use was developed from one or few studies, and applied across the range of sage grouse. This is a questionable one-size-fits-all approach to quantifying potential disturbance. For example, the corvid (e.g. raven, crow, and magpie) and domestic cat and dog predator risk models (regressions of probability of occurrance vs. distance from human habitations) were based on extremely limited data (4, 2, and 3 data points respectively)

and with no tests of significance or confidence intervals. Such poorly supported inferences cannot be viewed as reliable. (The impact of oil and gas wells is treated as a disturbance area around fixed points and their supporting infrastructure (roads and transmission lines) is quantified separately.) The authors provided a handful of citations including an unpublished masters thesis in support of data used to develop input models.

14.5) The authors analysis rests on the use of fractals (as opposed to Euclidean geometry) and modeled artificial landscapes, to summarize aspects of habitat fragmentation, including patch shape, edge, and size in terms of *lacunarity*. A concise definition of *lacunarity* used in ecology may be found in Halley et al. (2004): *"In general terms, however, lacunarity is an index of texture or heterogeneity [of a fractal object]. Highly lacunar objects possess large gaps or low-density holes, while low-lacunarity objects appear homogeneous. Thus, for example, in observations of vegetation cover using quadrats, lacunarity is low if we find very similar levels of cover in every quadrat (Plotnick et al. 1993). More precise definition of lacunarity has been problematic."*

Leu and Hanser's rationale for using this method is as follows:

> *"We analyzed artificial landscapes due to the lack of previous research evaluating lacunarity in natural landscapes demarcated by convoluted patch boundaries and to aid interpretation of lacunarity analyses from natural landscapes (Elkie and Rempel 2001)."*
>
> *"Lacunarity has several advantages over other more common fixed-scale landscape metrics because it consists of a single metric evaluated at multiple scales, is not influenced by edge effects, nor restricted to landscapes with high occurrence of habitat of interest (Plotnick et al. 1993). Lacunarity metrics can also be used to assess degree of relative fragmentation across diverse landscapes (Wu et al. 2000)."*
>
> *"Despite its ease in calculation, lacunarity analyses have been rarely used to study patterns of natural landscapes (but see Wu et al. 2000, Derner and Wu 2001, Elkie and Rempel 2001) perhaps, because interpretation of lacunarity curves can be difficult. However, we found that using lacunarity analyses of simulated landscapes, where degree of fragmentation and proportion of land cover reflect the range of values of landscapes studied, greatly aids in the interpretation of lacunarity functions of landscape patterns."*

Other authors have raised issues as to whether these models accurately represent real-world situations, and the conditions under which its use may be questionable. The uses and abuses of fractals in ecology are thoroughly discussed in Halley et al. (2004).

The original paper (Leu et al. 2008), a general description of the approach used in this paper, and data appendicies may be found at the following websites:
http://www.esapubs.org/archive/appl/A018/039/default.htm
http://sagemap.wr.usgs.gov/HumanFootprint.aspx

Chapter 15 (2009), Chapter 14 (2011):
INFLUENCES OF FREE-ROAMING EQUIDS ON SAGEBRUSH ECOSYSTEMS, WITH FOCUS ON GREATER SAGE-GROUSE

Authors: Erik A. Beever And Cameron L. Aldridge

Abstract from Beever and Aldridge:
"Free-roaming equids (horses [Equus caballus] and burros [E. asinus]) in the US were introduced to North America at the end of the 16th century, and have unique management status among ungulates. Legislation demands that these animals are neither hunted nor actively managed with fences and rotation among pastures, but instead constitute an integral part of the natural system of the public lands. Past research has elaborated that free-roaming horses can exert notable direct influences in sagebrush (Artemisia spp.) communities on structure and composition of vegetation and soils, as well as indirect influences on numerous animal groups whose abundance collectively may indicate the ecological integrity of such communities. Alterations to vegetation attributes and invertebrates can most directly affect fitness of Greater Sage-Grouse (Centrocercus urophasianus) and other sagebrush-obligate species; alterations of soils and other ecosystem properties may also indirectly affect these species. Across 3,030,000 ha of the western Great Basin, horse-occupied sites exhibited lower grass, shrub, and overall plant cover; higher cover of unpalatable forbs and abundance of cheatgrass; 2.2–10.0 times lower densities of ant mounds; and 2.9–17.4 times greater penetration resistance in soil surfaces, compared to to sites from which horses had been removed for 10–14 yrs. As is true for all herbivores, equid effects on ecosystems vary markedly with elevation, stocking density, and season and duration of use. However, they may be especially pronounced in periods of drought, which are forecasted to occur with increasing frequency in the southwestern US under climate change, and when they interact synergistically with livestock-grazing effects. Equids' use of sagebrush landscapes will have very different ecological consequences than will livestock grazing, at both local and landscape scales. Spatially, the addition of horses to sagebrush landscapes means more of the landscape receives use by non-native grazers than if domestic cattle alone were present. In spite of recent advances in ecological understanding of equid synecology, much remains to be learned. Life-history characteristics of Greater Sage-Grouse and other sagebrush-obligate species suggest the great value in evaluating equid effects more broadly than through a horses-vs.-livestock perspective, and in monitoring ecosystem components such as soil-surface hardness and ant-mound density that have ecological and management relevance yet data for which are relatively inexpensive to collect."

Review of:
INFLUENCES OF FREE-ROAMING EQUIDS ON SAGEBRUSH ECOSYSTEMS, WITH FOCUS ON GREATER SAGE-GROUSE

Review by: Dr. Rob Roy Ramey II

This paper reviews information and highlights the emerging conflict between the Wild Free-Roaming Horses and Burros Act of 1971 (that mandates the management of wild horses and burros on public lands) and sage grouse conservation, which likely to soon be under the Endangered Species Act of 1973. There is conflict because horses and burros are actually *invasive* species introduced to North American by Europeans and subsequently maintained on public lands for cultural reasons. The authors term these species *"free-ranging equids."*

15.1) While other papers in this monograph have pointed to roads, pipelines, well pads, and transmission lines as sources of invasive plants, this paper points out that wild horses and burros spread invasive species of plants, compact soils, trample nests, and affect sage brush community structure. For example:

> *"Domestic livestock consume an estimated 7,100,000 animal-unit months (AUMs) of forage annually (Table 3) within the current range of Greater Sage-Grouse. We estimate that free roaming equids consume an additional 315,000–433,000 AUMs annually within the current range of sage-grouse (Table 3). It is unknown whether effects of cattle grazing, horse grazing, and native-ungulate browsing are synergistic or simply additive."*
>
> *"Areas that have been managed for horses and/or burros during 1971–2007 constitute ~18% (119,703 km2) of the currently occupied Greater Sage-Grouse range. This estimate excludes dispersal and extralimital movements by equids (i.e., outside of management areas), which are difficult to quantify but may be widespread; considering these would appreciably increase the percentage of Greater Sage-Grouse habitats affected by equid grazing. About 12% (78,380 km2) of the current range of Greater Sage-Grouse is now managed for free-roaming equids (Table 1). Thus, there may be unmeasured consequences for a significant portion of sage-grouse habitat throughout the species' range, because of the aforementioned ways in which free-roaming equids can directly or indirectly impact sagebrush habitats.*
>
> *Burro movements overlap sage-grouse habitats in multiple areas across the southwestern US, and although the overlap is less extensive than is the overlap with horse habitats, burros tend to spend more time in lower-elevation habitats, as do sage-grouse."*

15.2) The authors propose more intensive research and management, including limiting population numbers of wild horses and burros:

> *"Free-roaming burros and especially horses are undeniably charismatic and enigmatic, and have been used to symbolize power, freedom, wildness, and toughness. Given the multiple stresses that interact to influence ecosystem dynamics across western North America, however, the benefits these non-native herbivores provide for various publics within society must be weighed against actual and potential ecological costs."*

15.3) This paper may be of significance to the argument that wild horses and burros are themselves invasive species managed to the detriment of sage grouse, yet more benign activities are potentially regulated.

The overlap. By state, of wild horse and burros with sage grouse habitat may be found in Table 1 (in 2007 there were 78,380km^2 of overlap, 19,290km^2 of which were within Wyoming).

Chapter 16 (2009), Chapter 15 (2011):
GREATER SAGE-GROUSE POPULATION DYNAMICS AND PROBABILITY OF PERSISTENCE

Authors: Edward O. Garton, John W. Connelly, Jon S. Horne, Christian A. Hagen, Ann Moser, And Mike Schroeder

Abstract from Garton et al.:
"We conducted a comprehensive analysis of Greater Sage-Grouse (Centrocercus urophasianus) populations throughout the species' range by accumulating and analyzing counts of males at 9,870 leks identified since 1965. A substantial number of leks are censused each year throughout North America providing a combined total of 75,598 counts through 2007 with many leks having >30 yr of information. These data sets represent the only long-term data-base available for Greater Sage-Grouse. We conducted our analyses for 30 Greater Sage-Grouse populations and for all leks surveyed in seven sage-grouse management zones (SMZ) identified in the Greater Sage-Grouse Comprehensive Conservation Strategy. This approach allowed grouping of leks into biologically meaningful populations of which 23 offered sufficient data to model annual rates of population change. The best models for describing changes in growth rates of populations and SMZs, using information-theoretic criteria, were dominated by Gompertztype models assuming density dependence on log abundance. Thirty-eight percent of the total were best described by a Gompertz model with no time lag, 32% with a 1-yr time lag, and 12% with a 2-yr time lag. These three types of Gompertz models best portrayed a total of 82% of the populations and SMZs. A Ricker-type model assuming linear density dependence on abundance in the current year was selected for 9% of the cases (SMZs or populations) while an exponential growth model with no density dependence was the best model for the remaining 9% of the cases. The best model in 44% of the cases included declining carrying capacity through time of -1.8% to -11.6% per year and in 18% incorporated lower carrying capacity in the last 20 yr (1987– 2007) than in the first 20 yr (1967–1987). We forecast future population viability across 23 populations, seven SMZs, and the range-wide metapopulation using a hierarchy of best models applied to a starting range-wide minimum of 88,816 male sage-grouse counted on 5,042 leks in 2007 throughout western North America. Model forecasts suggest that at least 13% of the populations but none of the SMZs may decline below effective population sizes of 50 within the next 30 yr, while 75% of the populations and 29% of the SMZs are likely to decline below effective population sizes of 500 within 100 yr if current conditions and trends persist. Preventing high probabilities of extinction in many populations and in some SMZs in the long term will require concerted efforts to decrease continuing loss and degradation of habitat as well as addressing other factors (including West Nile virus) that may negatively affect Greater Sage- Grouse at local scales. Key Words: carrying capacity, Centrocercus urophasianus, density dependence, effective population size, Greater Sage-Grouse, lek counts, management zones, models, N_e, probability of extinction, quasi-equilibrium, time lags."

Review of:
GREATER SAGE-GROUSE POPULATION DYNAMICS AND PROBABILITY OF PERSISTENCE

Review by: Dr. Rob Roy Ramey II

Introduction:
In the sage grouse lek mating system, males congregate in the spring at traditional sites and display in order to attract and mate with females. Of the western states that have sage grouse and conduct lek counts, all but one began counting the number of males on leks in the 1940's and 1950's. Garton et al. (in press) used count data of the maximum number of males observed on a lek each year (from thousands of leks in eleven states and three provinces) to estimate trends in population size and population persistence on 30 and 100 year time horizons. Modeling population change and testing for trend in data are challenges for conservation research (Edwards 1998) and is complicated when surveys were not designed to address present day questions.

In this study, Garton et al. used male lek count data that was collected by different state agencies without a standardized methodology or a random sample design. Relying on decades of non-standardized data of male lek couts is a fundamentally flawed approach to estimating sage grouse population number and trends. This problem has been long recognized. First, the data were not collected in a standardized way and lek counts were not randomly distributed or sampled. Second, there is no accounting for the number of females or juveniles in the populations sampled, their sightability, or how these differ across different sagebrush habitats. And third, there is no accounting for impact of hunter harvest on the the populations in question. Garton et al. make only passing mention of the fact that male lek counts are *"less than ideal"* and that improved methods exist but have not been adopted (Garton et al. 2007). Therefore, any analysis based on the current lek count data has a great deal of error and uncertainty associated with it. An ideal approach would involve redesigning leks counts to provide for a stratified random sampling approach to obtain an accurate estimate of grouse numbers, and then measuring trends over a multi-year period.

From a demographic perspective, what matters most in a polygynous mating system (like lekking) are *the females*. However, under the current method of lek counts, data on females (as well as juveniles) is not systematically gathered. Female sage grouse are cryptic in their plumage and behavior (to avoid predation) and therefore few are typically observed on a lek, whereas adult males are far easier to find and count because of their conspicuous lekking behavior in the spring. The following results from Walsh et al. (2004), who used radio tracking to quantify sightability in one population, illustrates this fundamental problem: *"On average, 42% of marked adult males, 4% of marked hens, and 19% of yearling males were observed on leks per sighting occasion with all 15 known leks being intensively counted."* With such low probabilities of detection (especially for females and juveniles) coupled with a lack of systematic data collection and non random sampling, lek counts of adult males, as currently conducted, are far from accurate for estimating sage grouse population trends.

Garton et al. used 30 and 100-year population forecasts to predict whether sage grouse populations would be extirpated. Predictions were based on whether the estimated effective population (N_e) sizes fell below 50 or 500, in which case populations were deemed quasi-extinct. (Simply put, effective population size is an estimate related to the level of inbreeding in a population.) There are a number of problems with this approach, not the least of which is the fact that many natural populations have fallen below these thresholds and they have persisted. There is no magic number that reliably predicts population persistence or extinction for any species. And finally, population the farther into the future population forecasts are made the more less accurate they become. A history of the derivation of the 50/500 rule of thumb is presented as part of this review.

The authors acknowledge few of the limitations of the data, statistical assumptions, and analyses and portray their results as having great precision and providing unbiased estimates. Here are unresolved issues related to how the data were collected, the criteria by which the data were filtered to produce the final data set, and the validity of assumptions and their effect on the analyses. Taken collectively, there are large uncertainties associated with the results and interpretation of information from this paper. Yet when Garton et al. acknowledge a limitation to their data or analysis, they immediately follow it with a more optimistic assessment. For example, after an incomplete four sentence discussion on the limitations of lek count data, the authors state: *"Nevertheless, long-term lek counts comprise the largest range-wide data base available for sage-grouse populations and provide the basis for reconstructing a remarkably precise index to minimum male abundance at a relatively broad spatial scale (Connelly et al. 2004)."* And after another brief acknowledgement that data were collected in a *"somewhat haphazard fashion, and permit no means of assessing the true magnitude of the population change"*, the authors conclude that *"Confidence intervals for population reconstructions for all populations and SMZs clearly show that precision of recent population indices are dramatically smaller than the earlier ones based on smaller samples of leks in the 1960–1980 decades."* The authors also propose that *" these forecasts will be useful in guiding decisions concerning the future of sage-grouse and the sagebrush communities upon which they depend."*

The authors reveal a *predetermined* bias towards federal threatened status in their introductory paragraphs: *"Ideally <u>populations threatened by extinction</u> should be monitored by censusing breeding males and females and their progeny annually…"*

DATA QUALITY ISSUES

16.1) Garton et al. used less stringent criteria for filtering data than the two previous studies relying on the same raw data:

The numbers presented by multiple authors (Conneley et al. 2004; WAFWA 2008; and Garton et al., in press) reveal that data from a large number of leks were eliminated in each study's filtering process, although the exact number of leks culled from the raw data is not reported by any of these authors, nor are their precise methods for rejecting some lek counts while keeping others. The following comparison gives some indication of the

shifting number of leks used in various analyses and that Garton et al. used less stringent criteria for filtering data than the two previous studies relying on the same data.

AUTHOR	YEARS	LEKS COUNTED	TOTAL COUNTS
Garton et al.	(1965-2007)	9,780 leks counted	75,598 lek counts total
2008 WAFWA	(1965-2007)	3,419 leks counted	34,441 lek counts total
2004 Conneley et al.	(1965-2003)	5,585 leks counted	not reported

16.2) Garton et al. overstated the number of leks with long term data (i.e. >30 years):
There is only one source of data used by this and the two previous population trend studies (Connelley et al. 2004 and WAFWA 2008). That data was collected by states and provinces, and updated annually.

Garton et al. report that data from 9,780 leks were used and 75,598 lek counts, including *"many having >30 yr of information."* However, it is clear that the majority of leks have less than 30 years data because 75,598 divided by 9,780 yields an average of only 7.73 years of data per lek. Additionally, the WAFWA 2008 report showed that fewer than 300 leks had been counted since 1968 (p12). Therefore, it is an overstatement for Garton et al. to claim that "many" of the leks used in their analyses had *" >30 yr of information."*

16.3) There are unexplained issues that influence how the data were filtered prior to analysis. Specifically, it is unknown how Garton et al. resolved the following data issues:
1) ambiguous locations, including those prior to the development of GIS technology;
2) how leks that had shifted in location over time were distinguished from new leks;
3) how the distances of observers from leks affected counts and what the observer distances were for each lek count;
4) the amount or type of disturbance (e.g. presence of raptors) that was required to drop a lek count because it would bias estimates downward;
5) although Garton et al. provide criteria for time of day for lek counts, they did not report use of any quantitative criteria for spatially defining a lek (e.g. the distance from a main lek for a satellite lek to be considered independent).
6) estimated number of uncounted leks.
Significance: The data set used by Garton et al. cannot be replicated because the methods used to include and exclude data were not adequately defined.

16.4) Vague assurances regarding data quality are not a substitute for detailed methodology. Example: *" We examined all lek data prior to analysis to ensure they were obtained following these procedures, and in some cases we had to assume that they were collected properly."* And "This allowed us to use all lek counts meeting our standards for quality…" the authors also suggest that "*problems* [with conducting lek counts] *generally seem to be related to disregarding accepted techniques."* However, the authors do not acknowledge that there are major issues with using lek count data to estimate population trends (see Walsh et al. 2004 and discussion below), nor did they provide specifics as to what their "standards of quality" actually were.

16.5) The lek count data analyzed by Garton et al. started in 1965, yet the lek count method papers cited in support of consistent methodology were published 13 and 19 years later (Jenni and Hartzler 1978, Emmons and Braun 1984). Additionally, states differed in how they gathered data or had missing location information (WAFWA 2008). For the reasons above, the same lek count methods were not applied across the entire data set. These issues, not acknowledged in Garton et al., are well illustrated by the following quotes from the WAFWA (2008) report:

1) *"Complete standardization was not possible, because most states provided summary data rather than raw data, and individual states may have had slightly different criteria."*

2) *"We assumed that if a state reported count data for a specific lek, those data were spatially associated with the location reported for that lek. In practice, the definition of a lek is more complicated. For example, individual males can shift among lek locations within and between years, smaller "satellite" leks can form near leks with large numbers of males, and observers sometimes report multiple activity centers within a large group of displaying males as separate leks, all of which can affect count data reported for a specific lek location."*

3) *"Other features of the dataset should also be noted. We excluded data from many leks in South Dakota because they lacked location information. We also excluded data from Colorado prior to 1986 because numerous errors in the state's database prior to 1986 could not be resolved in time for inclusion in these analyses. In North Dakota, all lek counts were conducted during the third week of April, but the state has used this approach >30 years."*

The two papers cited by Garton et al. in support of reassurances that the data they relied upon used accepted methods, are only descriptions and were inconsistent. These were published more than a decade after the initial high counts of the 1960s and 1970s. Jenni and Hartzler (1978) only evaluated Patterson's census technique and said nothing about a spatial definition of a lek. Jenni and Hartzler (1978) reported on:

1) <u>optimal time of day for peak counts</u> (i.e. 1/2 hour before and after sunrise);

2) <u>seasonal patterns of attendance</u> (i.e.*"Censusing the birds over the first 3 weeks after the peak of breeding does yield maxi- mum estimates of the number of males."*); and

3) <u>observation methods</u> (*"We censused the lek almost daily, and usually made several counts during each period of attendance. Two small igloo shaped tents were used as observation blinds. To minimize disturbance, the observer usually entered a blind before the evening lek, stayed overnight, and did not leave until after the grouse had departed the next morning. Observations were made through windows cut in the blinds."*).

4) Emmons and Braun (1984) described their criteria for including a nearby male as part of a lek and they started counting lek attendance earlier: *"Lek attendance was defined as being on or within 0.1 km of a lek between 0430 and 0730."* They also reported methods for data analysis which differed from the methods described by Garton et al. It is not known if Garton et al. used this 100m criterion for defining leks. If so, numerous ephemeral satellite leks would be coded as separate leks in the data, leading to a negative trend bias. This situation has been exacerbated in recent years as noted by Garton et al:, "*Sampling effort devoted to counting leks has varied from year to year and grew appreciably in the last 10 yr."*

Significance: The data analyzed by Garton et al. was *not* gathered using standardized methods, especially during early decades used in their study. This introduces error and bias not accounted for by Garton et al. For example, the number of grouse counted at a lek will depend in large part upon the spatial definition of a lek: a more inclusive definition will include satellite leks and result in a higher count while a more restrictive definition will result in more leks with fewer birds counted in each lek.

16.6) Garton et al. made no mention of bias in not estimating the number of unknown leks in their data set. As pointed out byWalsh et al. (2004): *"Estimating the number of unknown leks is a critical component in calculating detection probability and allowing lek counts to be properly related to trends in population size. Disregard for unknown leks does not allow for rigorous inference from lek-count data and will negatively bias estimates of total birds attending leks and cause possible misinterpretation of trend data (Anderson 2001)."*

16.7) The data used in Garton et al., as well as Connelly (2004) and the WAFWA (2008) report is not publicly available. Without the data used by these authors, the results cannot be replicated and many of the questions raised in this review will remain unanswered. To remedy the situation, I have made attempts to obtain the final data from Connelly,WAFWA, Garton, and others. Despite these repeated requests, the data are not publicly available. Answers to my requests have cited inconsistent reasons. Please refer to Appendix 1 for my correspondence in trying to obtain these data.

Significance: There is no way to independently verify the results of the analyses because the data are not publicly available. The authors do not provide a list of leks that were included in, or excluded from, the final data set. A partial list of data for Colorado obtained by this reviewer from the Division of Wildlife, however, it was provided with the locations of leks on private land deleted.

Even if the raw data were obtained from states, it is doubtful that the same final data set used by Garton et al. would be reproduced from it because the methods of Garton et al. were not adequately described. Simply put, the results cannot be replicated because the methods used to produce the final data set could not be replicated.

DATA ANALYSIS ISSUES

16.8) A logical first step in an attempt to estimate population trends would be to incorporate data on well-documented sources of mortality. However, this was not done by Garton et al. There is no accounting for spatial or temporal variation in hunter harvest, or its impact on sage grouse populations, even though these data are readily available.

Harvest rates change over time and from management unit to management unit, based on goals set from lek counts, wing counts, and aerial counts (depending upon procedures of each state/province). Monitoring to better manage the harvest of sage grouse is the reason that lek counts were initiated by states over 60 years ago. The harvest of sage grouse is not insignificant, as illustrated by the harvest numbers presented in Chapter 6 of this monograph (Reese and Connelly, *in press*). In 1992, 34,000 grouse were harvested from Wyoming and 30,000 from Idaho. Even in recent years, this harvest has been substantial: the total reported harvest from ten western states from 2001 to 2007 was 207,433 sage grouse. This does not include the number of grouse that were wounded and not recovered.

16.9) Garton et al. acknowledge that the lek count data violate statistical assumptions in that they are not random samples, yet they ignore other well known issues with the data. Instead, they state: "*yet when analyzed in a repeated measures framework,*[lek count data] *may provide unbiased and precise measures of the rate of change of populations."* The term "may" is speculative in this case. Garton et al. attempt to support their assertion by citing Connelly et al. (2004, Appendix 3) who 'tested' the lek count procedure using simulated populations. Garton et al. then summarize the simulation results of Connelly et al. (2004, Appendix 3): *"Precision of the estimates, measured by coefficient of determination of estimates with true simulated rates of change, increased with the simulated rate of population change from > 80% for populations with an observed annual rate of change of at least 0.03 and greater than 95% with rates of a least 0.07. Thus, while use of lek counts to assess change over a relatively large scale appears sound, we make no attempt to assess population dynamics at relatively small scales (e.g., harvest units, allotments) or estimate true population abundance using lek."* At face value, such statement gives an appearance of reliable estimates. However, Garton et al. failed to mention the other findings of Connelley et al. who clearly stated that such estimates had to be viewed with <u>caution</u>. These include:

1) *"An evaluation of accuracy suggested that accuracy increased with the observed rate of population change."* In other words, only large scale declines or increases (>0.07 per year) were significant.

2) Of the 41 populations and 24 subpopulations (65 total) subsequently evaluated

by Connelley et al., those authors stated that most *"fit at least one of the following categories: 1) too few years for analysis; 2) too few data for analysis; and/or 3) population changes during the last 20 years were not apparent or not significant."* In fact, only 11 of 65 population/subpopulations had significant trends (≥95% probability of being accurate).

3) In contrast to Garton et al's portrayal, Connelly et al. considered their results promising but preliminary. *"It is impossible to include every source of variation and there may always be effects such as annual variation in the number of counts per lek. Because most of these sources of variation have not been examined in detail, these results should be viewed with caution (see Walsh et al. 2004)."*

16.10) In their methods, Garton et al. stated: *"We made no attempt to ... estimate true population abundance using leks counts."* Yet, these authors violate this assurance by subsequently using lek count data for creating a series of population estimates including: *"index of historical abundance"*, *"population reconstructions,"* and a probability of extinction based on those estimated population sizes and forecasts on 30 and 100 year time horizons.

16.11) There is an undisclosed issue with the Wyoming data set. For example, 2008 WAFWA report state that the Wyoming data set was apparently "corrupted and had to be rebuilt from raw data". It is not clear what this statement means (i.e. whether the authors were using data provided by Wyoming, or a previously filtered data set produced and maintained by WAFWA). For example, Wyoming lek counts were eliminated before 15 March and after 15 May, whereas other leks were apparently included from March, April, and May. Also eliminated were leks with less than 2 visits per year, and other criteria. The effect of treating Wyoming differently was not discussed in the report.

16.12) The way the count data are coded, leads to a negative trend bias in the data. The authors of the 2008 WAFWA report raised an important issue relative to how the method of defining leks and handling missing data lead to negatively biased trends in their analysis and the same issue applies to Garton et al. On page 7 they identify the issue: *"One problem associated with missing values should be noted with this data set. Because the current sampling scheme is lek-based rather than area-based, locations are not considered a lek and therefore, not reported in databases, until grouse are found using them. Therefore, very few leks in the data set started with a zero. As a result, the initial establishment of a lek with a small number of male grouse and its concurrent increase from zero to a positive number of grouse is missing from these data, while long sets of zero counts often exist after a lek has become inactive.* ***This could lead to negatively biased estimates of trend in male count."*** Garton et al., however, say nothing about this issue or its implication for their analyses, yet as reported by WAFWA 2008, the data set is riddled with such incomplete counts. **Garton et al.'s averaging of rates of change across populations using counts from leks does not remove this bias.**

16.13) Garton et al. cited (Humbert et al. 2009) in support of their use of their exponential growth models. However, the data set analyzed by Garton et al.

represents a very different situation than that described in Humbert et al. (2009), making Garton et al.'s use of these models questionable. In that paper, the authors were tackling the problem of deriving trend estimates from incomplete data sets obtained from single locations that were the result of counts being skipped. In contrast, the lek count data analyzed by Garton et al. is much more complicated because:

1) the data were not randomly collected (spatially or temporally),
2) data collection was not uniform between states and sometimes within states,
3) sampling effort has changed over time,
4) the number of sage grouse leks being counted has increased over time,
5) the personel monitoring the leks changed over time,
6) the locations of sage grouse leks were imprecise prior to GPS,
7) ephemeral satellite leks are known to form adjacent to main leks,
8) smaller leks frequently are extirpated or abandoned, and
9) environmental factors cause fluctuation in sage grouse numbers.

16.14) The locations of leks used in the analyses (and their sampling period) is not reported. Without access to the underlying data, including the locations of leks, we cannot know to what extent the sampling effort is representative of the distribution or population density of sage grouse. For example, if lek counts are concentrated in a limited area of a management zone, those data would disproportionately influence results for the entire management zone.

16.15) Garton et al. cite Walsh et al. (2004) only once and with regard to the observation that some male sage-grouse may not attend a lek or may attend two or more leks. What Garton et al. did not acknowledge are the numerous biases and uncertainties of lek counts that were raised by Walsh et al. (2004). Several issues not addressed by Garton et al. are listed below:

1) Walsh et al. (2004) reported that*: "observer bias was a major source of variation and a major confounding variable associated with indices including lek counts (Bibby et al. 1992, Buckland et al. 1993, Anderson 2001). Observer bias arises from variation in observers' inherent characteristics such as visual acuity, interest, experience, and training (Bibby et al. 1992, Anderson 2001). Observer bias can also arise from counting effort and techniques, date and time of lek counts, and distance from observer to leks. Large distances affect ability to properly enumerate individual birds on leks which results in negatively biased counts that underrepresented yearling and nonstrutting cocks (Rogers 1964). Many wildlife managers in our study site historically viewed leks from extreme distances of up to 3.2 km for fear of disturbing lekking activities. Rogers (1964) recommended counting at a distance of <50 m in a vehicle when possible."* It is unknown if any of these variables are quantified in the raw data from states or whether they were used in the attempt to standardize data by Garton et al.

2) Walsh et al. (2004) provide evidence that a long standing assumption used in applying lek count data to population trends is not supported by data: *"Wildlife managers have long assumed that lek-attendance rates of adult male greater*

sage-grouse are high and constant (Patterson 1952, Emmons and Braun 1984). Findings of this study corroborate those of several other studies that provide evidence of much lower attendance rates than previously suspected, based on individually marked grouse." However, while the cited study involving a handful of lek counts (n=12) in southern Idaho may have used *"established guidelines"* (from Connelly 2004) it is clear that states have conducted tens of thousands of lek counts using *inconsistent* methods for long periods, both before and long after the establishment of these guidelines. The unpublished data cited by Garton et al. (J. Baumgardt) does not resolve this issue.

3) Multiple lek counts are typically conducted each year for each lek and the maximum number of males from these counts in a season are the data recorded. However, the extent to which males move between leks (which would lead to individuals being counted in more than one lek in a season) is unknown, as is the probability of detecting males in different environments or at different densities. The detection probabilities of male sage grouse has only been described in one paper Walsh et al. (2004) which addressed the fundamental issue of using lek counts to estimate population number

16.16) The data interval used in this report begins in 1965, a starting date selected at the request of the U.S. Fish and Wildlife Service for the WAFWA (2008) report, although most states have lek count data going back one or two decades earlier. Oregon began counting leks and recording data in 1944, Wyoming in 1948, Idaho and North Dakota in 1951, Montana in 1952, California and Colorado in 1953, Washington in 1954, Nevada in 1956, Utah in 1959, and South Dakota in 1971 (Connelly and Schroeder 2007). The effect of *not* including those earlier data are not mentioned by Garton et al., however, it is worth asking what difference(s) there would be if these data were included in the population level analyses.

16.17) A fundamental issue when comparing current population number and distribution to historic levels is *"how far back should we reach to set the baseline from which measurement will start?"* And does setting the historic baseline at 1965 result in biased trends? This is an important issue because ecological communities change over time for multiple reasons (e.g. climate, predation, invasive species) leading to shifts in the abundance and distribution of the species of interest. If a baseline for analysis is selected during a period of high abundance, then the results are likely to show a decline, whereas if the baseline is selected during a period of lower abundance, then the results may show an increase. Therefore, selection of a baseline year can influence results. Forty-two years is a very short run of data to establish long-term population trends for a system that has seen wide fluctuations in ecological and land use conditions.

The authors make no mention of the fact that that during the years 1965-1985 there were widespread sage brush eradication programs that could be expected to impact sage grouse populations. For example, during this period, sage brush was controlled or eradicated using mechanical removal (chaining, brush beating, disking and harrowing), prescribed fire, late-fall grazing, and herbicide application (including 2,4,5-T and 2,4-D). Therefore,

it is reasonable to expect evidence of a sage grouse decline during that period and in the most affected areas. Since the 1980s, however, management of sagebrush has moved toward conservation/reestablishment, which could be expected to benefit sage grouse populations and lead to a population increase.

Reaching farther back in time before 1965, it is worth asking, *"in what ways have historic conditions for sage grouse been different?"* For example, would predator eradication programs (particularly wolves and coyotes) and hunting of golden eagles, have reduced predation pressure on sage grouse, artificially allowing them to expand their range and numbers into the 1960's? By 2003 the golden eagle population in the west had expanded to an estimated 27,392 (with a 90% confidence interval of 21,352-35,140). And reaching farther back to the time of Lewis and Clark's expedition in the early 1800's, it is clear that sage grouse had a more limited northern distribution, as none were seen along the Missouri River or east of the Continental Divide. The Lewis and Clark expedition occurred during the climate anomaly called the Little Ice Age, which could account for a more limited northern distribution of sage grouse than present.

The farther back in time that comparisons are made, the greater the uncertainty in population sizes and the greater the potential bias in how data were collected (e.g. only from few large, and easy to find leks). The degree of this inherent uncertainty is found throughout the results of Garton et al. and is clearly illustrated in Figures 2 through 8. For example, the 90 percent confidence intervals for the population estimate from the Powder River Basin in 1968 was between 180,000 and near zero (Figure 2C). If a higher, 95 percent confidence intervals were applied, these confidence intervals would reveal even greater uncertainty.

16.18) There is reason to expect non-stationarity of the data over the time intervals examined. Population trends are influenced by changing conditions over time, including major shifts in the relative importance of environmental variables, or changes in their level of interaction (all three produce non-stationarity). In other words, external effects can change or reset the equilibrium properties of a system. In the case of sage grouse, multiple variables have been identified as affecting abundance and distribution over time, including: climate, sage brush control, hunting/harvest, predation (which also affects lek attendance by males), density dependence, invasive species, wildfire, parasites, infectious disease, and land use (Connelly et al. 2004). The effects of each of these variables cannot be expected to remain temporally or geographically constant across the range of sage grouse because natural conditions and land use practices change across management zones, states, and time periods. Changes in methods of data collection over time can also produce non stationarity.

Non-stationarity is a potentially significant issue here because there are relatively short runs of data between ecological shifts for most of the leks counted (including sage brush control and hunting harvest) and these are not likely to be equivalent across regions. This and the practice of combining data from multiple states that use different methods can violate statistical assumptions, obscure biologically meaningful management zone trends, and produce erroneous results. Non-stationarity is masked by use of procedures that

result in averaging rates of change across populations and management zones. Yet, without the underlying data, it is not possible to estimate the magnitude of this issue.

16.19) Confidence intervals may not adequately represent the actual uncertainty associated with the estimates and predictions of Garton et al.
Because of unknown biases in the data collection (the data are not publicly available, nor are the quantitative criteria used to filter the data), complexity of the analyses, and model selection uncertainty (associated with the generally poorly fitting models reported), the confidence intervals reported by the authors may not adequately represent the actual uncertainty associated with the estimates (Burnham and Anderson 2002). The fact is that sage grouse population dynamics is, as numerous authors in this volume attest to, a complex biological system with numerous inputs of varying importance operating at different spatial and temporal scales, and with far more inputs and uncertainty in estimation than considered or acknowledged by the Garton et al.

16.20) The authors make repeated references to the precision of their estimates but no reference to accuracy of those predictions appears anywhere in the text. The adjective "precise" is mentioned 11 times, and "precision" 14 times in the text, yet "accurate" and "accuracy" are not used. While Garton et al.'s population estimates may be precise, there is no way to know whether they are accurate for three reasons. First, the data that they are based upon is not publicly available; 2) the methods used to cull the data are inadequately described, and 3) predictions about future outcomes on such time-scales are untestable hypotheses about the future. **Significance**: Predictions on such long time-scales, while potentially useful heuristic tools, are notoriously inaccurate and can easily be overapplied (Pielke, Jr. and Conant 2003).

16.21) Model selection procedures were used by Garton et al. to select among alternative population models (based on ΔAIC values) but there was no biological justification as to why one model would be expected to perform better than another under a give set of circumstances (e.g. among populations or management zones).
The fitting of 26 population models to each SMZ and population, as described by Garton et al. under *"Fitting population growth models,"* is equivalent to a data fishing expedition to find statistical significance (using AIC) without questioning the biological relevance of these results. It is far more biologically relevant and objective to take a small subset of biologically meaningful models and treat those as alternative hypotheses and test those against the data. The approach of this paper is exemplified by the fact that the authors offered no tests in advance that could explain *why* one model succeeds over another in AIC value under a given set of ecological or demographic circumstances. Model selection results are presented yet not explained, so the reader is left without any meaningful understanding of *why* one model would be expected to fit a particular population better than another. And the predictions of future population sizes are *not* empirically testable, unless one is willing to embark on a monitoring program to test them over decades. No such program was proposed by Garton et al.

16.22) Several of the authors had previously suggested a new approach be used as the basis for future population estimates. However, that method has not yet gained

widespread acceptance. The recommended method was proposed by Garton et al. (2007) and subsequently implemented on a local scale. This reviewer concurs with this suggestion as it could produce statistically valid population estimates, although greater field effort would potentially be required. As described by Garton et al. (2007): *"We propose that lek counts be conducted with a 2-stage cluster-sampling approach embedded within a stratified random sample of geographic areas hierarchically structured to describe populations within metapopulations of greater sage grouse."* This approach counts both males and females, focuses on areas rather than specific lek sites, and most importantly, it employs a probabilistic sampling scheme - all of these are lacking in current lek count and analysis methodologies. Differences in sampling would preclude comparison to existing data sets, but would offer a more accurate picture of sage grouse population numbers and trends.

16.23) To date, no consistent, quantifiable link has been demonstrated between the male lek counts and *actual* population trends. Walsh et al. (2004) suggested a way this might be done using a different study design and lek counts of both males and females. They pointed out that: *"Until lek counts are calibrated to actual population parameters by estimating detection probability, managers must realize the limitations of lek-count data and should be cautious when reporting trend data based on them."* The authors of the WAFWA 2008 report clearly stated these shortcomings: *"Many assumptions and potential sources of error limit inferences that can be made from the data presented in this report. First, because the data are counts with no measure of detection probability and no probability-based sampling design, trends refer only to the maximum male count of sampled leks. Consequently, statistical inference does not extend to sage-grouse population size."* Despite these cautionary statements, Garton et al. estimate population trends, reconstruct population numbers, *and* forecast future population numbers.

16.24) Because of the spatial and temporal non-stationarity of sagebrush ecosystems (e.g. variation in rainfall, predation, hunter harvest, agricultural development, sage brush control) teasing out the relative importance of variables and their effect on sage grouse population size will continue to be a challenge. The limitations of lek counts will continue to hinder our understanding until such time that more biologically relevant and statistically defensible census methods are adopted. Garton et al. are correct regarding the need for states to adopt a uniform, biologically meaningful, and statistically valid data collection methodology for sage grouse. As noted by these authors, these methods have been field-tested but not widely accepted: *"Methods to replace this weak foundation of lek counts representing an unknown proportion of leks in a spatial region by a true probability sample of leks and breeding males and females in defined spatial areas have been proposed but not widely adopted at this time (Garton et al. 2007)."*

16.25) Garton et al. based their conclusion on analysis of data which was gathered over 42 years without standardization or a random sampling design. As a result, there are numerous limitations to the inferences in this study. If one were to design a study to monitor sage grouse population trends, the methods for data collection and analysis would be quite different from those used in Garton et al., (see suggestions by

Walsh et al. 2005, Garton et al. 2007, and the management recommendations of the WAFWA 2008 report). Such an effort would require a level of standardization in data collection that has not yet been achieved across states, and an intensive level of research and monitoring to reach the desired level of precision and accuracy. The data gathered would not be directly comparable to the non-standardized lek count data collected to date, but would provide a valid method of estimating sage grouse population trends.

16.26) Hunting harvest is the major documented source of adult mortality, with more than ten percent of total sage grouse number harvested annually. Yet this receives no mention from Garton et al. **From a demographic perspective, the harvest of female sage grouse will have a far greater impact than the hunting of males, yet only males are counted.** Garton et al. estimated 88,816* male grouse in 2007 or a total population size of 310,856 (using their assumption of 2.5 adult females per male to obtain total population). That was the count in the spring while leks were active. However, in the fall of 2007, a total of 28,180 sage grouse were harvested, or 9 percent of the estimated number of this species. And in four of the six pervious years, the take was even higher (up to 37,607 in 2006). To date, there has not been field-verified studies of maximum sustainable yield applied to this species and this intensity of harvest. The state of the science does not provide an empirical basis that is solid enough to forecast the future of sage grouse with any degree of accuracy, especially when known sources of mortality are not included.

Significance: While the estimates developed by Garton et al. are of questionable accuracy**, it is clear that hunting harvest is at a level that cannot reasonably be assumed to be insignificant to the population as a whole. There is no mention of hunting in Garton et al. even though this documented source of mortality could reasonably be expected to have an effect on trend analyses and extinction probabilities.

*In their abstract, Garton et al. estimated 88,816 male grouse in 2007, however, when totals are summed across SMZs, the total is different (87,3376). A reason for this discrepancy is not explained.

**The reason that no one has reported a negative effect from hunting harvest may be attributable to: 1) lek count data are an unreliable index of population size and trends, 2) the harvest of male vs. female sage grouse can be expected to vary by location and year, introducing noise into demographic data, 3) non-stationarity, and 4) the confounding effects of other variables, including environmental variation.

16.27) In contrast to Garton et al. the authors of the WAFWA 2008 report clearly stated their shortcomings: *"Many assumptions and potential sources of error limit inferences that can be made from the data presented in this report. First, because the data are counts with no measure of detection probability and no probability-based sampling design, trends refer only to the maximum male count of sampled leks. Consequently, statistical inference does not extend to sage-grouse population size."*

16.28) It is notable that Garton et al. lacks any mention of hypothesis testing and validation (against independent empirical data). Nor does the word 'hypothesis' appear anywhere in this paper. The approach used by Garton et al. involved the basic elements of data exploration: data filtering, model selection, estimation, prediction, description and interpretation. The approach used by Garton et al. was inductive (data exploration), as opposed to a hypothetico-deductive approach. The latter is the epistemological paradigm that has been the primary driver of modern scientific advances.

If Garton et al. had followed their data exploration with development of refined hypotheses *to be tested*, rather than a series of untestable predictions, it would have crossed this threshold. A fundamental epistemological problem with predictive modeling, such as that presented in Garton et al., is that it is not falsifiable. Garton et al.'s approach to predicting extinction is equivalent to making quantitative predictions as if they were certain and then never testing them. This has major consequences for policy intervention. A preferred method would be to test these models as proposed by Corkett (2002): *"The inductive approach… should be replaced by a critical rational approach in which management decisions would be based, not on those nonfalsifiable models best supported by the facts, but on those falsifiable models that have been tested by the facts."*

Significance: The use of model selection by Garton et al. introduces data exploration and inductive reasoning into policy decisions such as the proposed ESA list of the sage grouse. This represents a departure from the hypothesis testing (deductive reasoning) that has been the hallmark of scientific advances, successful problem solving, and informed decision making.

EXTINCTION PREDICTIONS

16.29) Garton et al. misapply the 50/500 effective population size "rule of thumb" to make erroneous predictions on the probability of population and metapopulation extinction on 30 and 100 year time scales. It is clear that Garton et al. is unfamiliar with the derivation or validity of these rules of thumb, despite numerous published papers on this subject. This ignorance is exemplified by Garton et al. calling this a "rule" instead of a rule of thumb.*"Thresholds of 20 and 200 were chosen to correspond approximately to the standard 50/500 rule for effective population sizes (Ne; Franklin 1980, Soulé 1980)"*

Garton et al's use of model averaging to forecast future population abundances and probablility of extinction based on those forcasts and the 50/500 rule of thumb is laid out below*: "In other words, forecasting future probability of a local population or a SMZ declining below effective population size of 50 breeding adults (N_e = 50 corresponding to an index based on minimum males counted at leks of 20 or less) identifies populations or SMZs at short-term risk for extinction (Franklin 1980, Soulé 1980) while a local population or SMZ declining below effective population size of 500 breeding adults (N_e = 500 corresponding to an index based on minimum males counted at leks of 200 or less)*

indentifies populations or SMZs at long-term risk for extinction (Franklin 1980, Soulé 1980). Most populations and SMZs, based on our comparison of AIC_c values, had >one model that could be considered a competing best model by scoring within the 95% set. This generally meant AIC_c <3. We projected future population abundances using each of the 26 models and used model averaging to incorporate model selection uncertainty into forecasts of population viability (Burnham and Anderson 2002) to generate an overall, based on all fitted models,"

Minimum viable population size is a frequently used term in the conservation biology literature. The basic idea behind the concept and population viability analysis is that there must be some "minimum conditions for the long term persistence and adaptation of a species or population" (Soule' 1987). Despite the disclaimer by Soule' (1987) that there *"is no single value or 'magic number' that has universal validity"* in the estimation of population viability, two numbers are cited frequently and without question in the conservation biology literature (like Garton et al.) and in management plans. An effective population size (N_e) of 500 has been used to describe the minimum number of individuals necessary to maintain the long term adaptive or survival potential of a closed population. And a N_e of 50 has been suggested as the minimum population to prevent the effects of inbreeding depression that would lead to a high risk of extinction.

16.30) Background on the N_e <500 rule of thumb

The suggestion that a minimum effective population size of 500 individuals is necessary to maintain the adaptive potential of a population is based on Franklin's (1980) quantitative genetic model. Franklin reasoned that most important adaptive changes are the result of selection on continuously varying characters and therefore a quantitative genetic approach, rather than a population genetic approach, was needed for the long-term conservation of genetic variation. The model that Franklin presented described the conditions under which an equilibrium could be maintained in a finite population between the loss of additive genetic variance through genetic drift (no selection operating) and the amount gained via mutation.

Phenotypic variance in a population can be apportioned into three components: environmental variance, genotypic variance, and the genotype-environment interactive variance (Franklin 1980, Falconer 1981). The proportion of genotypic variance can be further broken down into additive, dominance, and epistatic variance components. Of these, the additive genetic variance is the most important to the genetically determined characteristics of a population that will respond to natural selection. The ratio of additive genetic variance to phenotypic variance is referred to as the narrow sense heritability of character (h^2).

In very small populations, the loss of genetic variability through genetic drift will be greater than that gained through mutation. According to Franklin (1980) and Falconer (1981), if all of the genotypic variance in a population is additive (no dominance or epistatic variance) then the rate of loss of additive genetic variance will be equal to the rate of loss of heterozygosity or $1/2N_e$ per generation. To determine the rate of gain for additive genetic variance from mutation, Franklin (1980) relied upon estimates based on

a handful of studies of mice, maize and *bristle hair number* in Drosophila that had been summarized by Lande (1976). In these studies of highly inbred lines, the proportion of additive genetic variance from mutation was found to be approximately 10^{-3} that of the environmental variance. From this information, Franklin concluded that if the loss and gain of additive genetic variance is to be equal in a population, or $1/2N_e = 10^{-3}$, then N_e must equal 500. This is the extent of Franklin's derivation. The empirical evidence used to derive this was minimal and gathered over thirty years ago,

Franklin's (1980) model dealt with the maintenance of neutral variation and did not formally consider the effects of stabilizing or directional selection on heritable variation. Although the influence of selection on additive genetic variance was treated qualitatively, Franklin (1980) nevertheless concluded on the basis of artificial selection experiments that *"the major detriment of the level of genetic variance in natural populations is the balance between genetic drift and mutation."* Lande and Barrowclough (1987) subsequently considered the effects of stabilizing selection on additive genetic variance in a quantitative genetic model and tentatively concluded "that an $N_e > 500$ can maintain as much genetic variance in typical quantitative characters as an indefinetly large population" although they cautioned that *"this figure cannot be regarded as being very precise."*

16.31) Background on the $N_e < 50$ rule of thumb

I has been generally assumed that populations with an N_e of less than about 50 is vulnerable to the effects of inbreeding depression and are at high risk of extinction. An N_e of 50 is thought to be a minimum number for short-term survival of a population to minimize inbreeding depression (to less than 1% per generation). However, field data and theory show that even smaller N_e s are not necessarily deleterious nor a good predictor of extinction unless the duration of the bottleneck is both severe and prolonged (Luikhart and Cornuet 1997; Ramey et al. 2000). Empirical support for the N_e 50 rule of thumb came in 1990 from a widely cited study by Berger (1990) who found that this rule predicted the disappearance within 50 years of all mountain sheep populations in California estimated to number 50 or fewer. The applicability of this rule of thumb to bighorn sheep was subsequently questioned and falsified by Krauseman et al. (1993, 1996), Goodson (1994) and Wehausen (1999). Krausman et al. (1993, 1996) conducted an independent test of Berger's predictions using data from Arizona and found that 6 of 20 populations had increased from below 50 to over 100 and that extinctions were found across all population size classes. Goodson (1994) reported similar results contrary to Berger's predictions for Colorado. And finally, Wehausen (1999) reported that: *"I tested Berger's (1990) model using the complete data set from California and found-contrary to his results-that, for all size classes of population estimates, at least 61% of the populations persisted for 50 years. Also, two predictions from Berger's model were not consistent with the data from California: (1) 10 populations have increased from estimates of 50 or fewer animals to over 100, whereas the Berger model predicted that these populations would only decline to extinction; and (2) of 27 extant populations with long enough records, 85% were estimated at least 50 years ago to be 50 individuals or fewer and should therefore be extinct by now. Berger's model has now failed tests in three states and therefore does not support the strong population size effect on extinction*

probability that it first appeared to provide, and it may serve conservation poorly through misdirected effort if it is used as the basis for setting policies or taking actions."

16.32) The 50/500 rule of thumb: recent critiques and empirical data
Boyce (1997) summarized on the supposed universal applicability of the 50/500 rule, *"Likewise, there is no solid basis for the often-cited rule of thumb that five hundred individuals may be sufficient to maintain long-term viability of a species. Unfortunately, the 50/500 rule does not have a sound genetic or demographic basis (Lande and Barrowclough 1987, Ewens 1990). And there is no theoretical or empirical justification for basing MVP [minimum viable population size] on an estimate of Ne."* And, *"Further empirical evidence is needed to justify the use of rule of thumb for MVP. But until such evidence becomes available, reliance on rules of thumb, such as the 50/500 rule is arbitrary and capricious."*

More recently, Frankham (2005) underscored the absence of empirical data to support the 50/500 rule of thumb: *"The second deleterious genetic effect of small population size is expected to be the loss of evolutionary potential, the ability to evolve especially in response to environmental change. I am not aware of any field data that make a scientifically supportable connection between loss of genetic diversity and extinction risk."* In that study, Frankham (2005) had only found a measurable effect by using an experimental population of *Drosophila* and putting them through extremely small population bottlenecks of two individuals as founders for several generations and subjecting offspring to different levels of salinity . They reported: "*However, in the laboratory we have tested whether population size restrictions affect extinction risk under condition of increasing levels of a stressful environment viz. increasing levels of NaCl. Single pair population size bottlenecks for one or three generations resulted in extinctions at lower NaCl concentrations than in nonbottlenecked base population control populations (Frankham et al., 1999)."* Frankham (2005) also noted the poor correlation between estimates of genetic diversity and quantitative genetic variation: *"Correlations between molecular and quantitative genetic variation across populations are typically low, averaging 0.2, and they do not differ significantly from zero for life-history characters (Reed & Frankham, 2001). The low correlations could be due to different patterns of change in genetic variation for the molecular and quantitative characters, or could simply be a reflection of large sampling errors around estimates of genetic diversity, especially those for quantitative traits."* These experiments underscore the fact that there is no universal population genetic number that can predict extinction probability. And while the loss of genetic variation in populations can reduce adaptive potential to changing environments, the specific adaptive traits affected, and magnitude of those responses, are rarely quantifiable.

From a functional perspective, it is not the effective population size *per se* that leads to an accumulation of deleterious mutations, it is genomic processes involving errors in replication. These include: point mutations, insertions, and deletions, endogenous retroviruses, a suite of mobile elements in the genome (long interspersed elements, short interspersed elements, and transposons), gene duplication and subfunctionalisation, exon shuffling, and intron splicing. Each of these mutational processes are going to act

differently upon lineages and how the act depends upon their unique evolutionary history. Without detailed knowledge of a species genome or its evolutionary history, the fitness "burden" imposed by these mutational processes, or how that might be purged or diluted, will be highly uncertain. For this reason, empirical data has been lacking from the much touted concept of "mutational meltdown" (Lynch and Gabriel 1990), (i.e. the accumulation of slightly deleterious mutations in closed populations) as a contributing factor in extinctions. As a result, there is no universally applicable magic number that can be applied to extinction probabilities. We know more about the genomics of *Drosophila* than other eukaryotes, yet we consistently fail to make reliable predictions regarding extinction probabilities. Other serious correspondence problems between the real world and the 50/500 rule of thumb include: ignoring the influence of migration, population structure, mating system, natural selection, genetic drift, recombination, and over dominance, as well as environmental factors influencing demography.

Significance: Application of the 50/500 rule of thumb to predicting extinction probabilities is speculative.

16.33) Garton et al. grossly underestimate the importance of current sage grouse migration rates among populations and SMZs to maintaining genetic variation and potential for recolonization. In the section titled "Metapopulation Analysis", Garton et al. reported that: *"Estimated dispersal rates among SMZs were generally low, never exceeding 5% of the SMZ's abundance dispersing to any other SMZ (Table 71)."* However, these rates are actually high from a population genetic perspective. Even the lowest reported rate of 0.1% migration between the Snake River Plain and the Southern Great Basin, is more than adequate to prevent divergence in allele frequencies, local inbreeding, and thus extinction under the 50/500 rule of thumb. For example, Garton et al report that in 2007 there were an estimated 15,761 males attending leks in the Snake River Plain and 12,165 in the Southern Great Basin. When their estimate of 2.5 adult females per adult male at leks are added to these numbers, it yields a total population of 55,163 in the Snake River Plain and 42,577 in the Southern Great Basin, or 97,740 total adult grouse. An annual migration rate of 0.001 (0.1%) between these populations would result in a net exchange of approximately 97 individuals annually or nearly 291 sage grouse per generation among populations (assuming a 3 year generation time). This exceeds, by a factor of nearly 300, the rate of migration necessary to prevent genetic divergence and avoid deleterious effects of inbreeding (Wright 1931; Tallmon et al. 2004).

Theoretical population genetic and empirical studies have consistently shown that low levels of immigration are sufficient to maintain genetic variation and increase fitness in populations. The influence of migration and genetic exchange can also be seen in sage grouse genetic data. For example, a recent study by Bush (2009) produced the following conclusions, contrary to the extinction predictions of Garton et al.'s erroneous application of the 50/500 rule of thumb: "*Using historic (1895 – 1991) and contemporary samples, I documented that sage-grouse in Canada have not experienced reduced genetic diversity, increased population structure, or genetic bottlenecks despite significant demographic declines in the last 40 years. Both effective population size and effective number of breeders decreased with time, but the effective number of breeders was high given the*

estimated population size. This is likely due to lower than expected variance in reproductive success and gene flow from the rest of the northern Montana population. Presently, it appears that genetic variability in Canada is being maintained through migration from southern parts of the northern Montana sage-grouse population and the low expected decline in genetic diversity based on simulations using historic genotypes."

Significance: Garton et al. grossly overestimate the impact of population number on extinction probabilities. Current estimated rates of migration among populations is sufficient to render Garton et al.'s extinction predictions invalid.

16.34) Garton et al. propose that their extinction predictions guide policy decisions on sage grouse, even though these are based on: 1) flawed data, 2) invalid assumptions, 3) population forecasts that are untestable, and 4) the false premise that there are magic numbers that can predict extinction in sage grouse. *"We have attempted to improve upon the classic approaches by including models which are based upon estimates of both long-term changes (time or year effects) in carrying capacity (our terminology for the quasi-equilibrium), recent changes in rates of change in the last 20 yr (period effects) and a variety of forms of density dependence (linear vs. log-linear and 0–2 yr time lags) that have increased the coefficients of determination of the models dramatically, thereby improving our confidence that these forecasts will be useful in guiding decisions concerning the future of sage-grouse and the sagebrush communities upon which they depend."*

Review of:
GREATER SAGE-GROUSE POPULATION DYNAMICS AND PROBABILITY OF PERSISTENCE

Review by: Dr. John D. Wehausen

This review compares Garton et al. with a recent report on the same subject issued by WAFWA.

16.35) A. WAFWA Report

In 2008 WAFWA put out a report on a commissioned analysis of lek counts of greater sagegrouse for the period 1965-2007. Those analyses partitioned the data along two dimensions: three time frames (1965-2007, 1965-1985, 1986-2007); and four geographic scales (range-wide, management zone, population, and state) and estimated trends in maximum male counts at leks using a set of linear mixed-effects models to test whether there was no trend, a linear trend (increasing or decreasing), or a quadratic trend in an information theoretic (AIC) context.

Those analyses had laudable and questionable aspects. The laudable aspects were:

1. Authors had a potential biological basis for the time periods used in the analyses.

2. A definition of a lek was generated in a way that might eliminate some confounding variation in lek count data.

3. Authors were forthcoming in presenting what they considered shortcomings of their analyses, which included:

> **(a)** Admission that insufficient time was available to do the analyses optimally.
>
> **(b)** Recognition that the relationship between lek counts and population size is unknown with the result that there is potentially substantial bias in any trend analysis with lek count data, regardless of analysis method, and that area based data would be preferable to lek based data. Walsh et al. (2004) stated: "Until lek counts are calibrated to actual population parameters by estimating detection probability, managers must realize the limitations of lek-count data and should be cautious when reporting trend data based on them"
>
> **(c)** Acknowledgment of the high variance in male attendance at leks, that bias could enter into their results because their ultimate measure, highest lek count, is a function of number and timing of lek counts, that methods for lek counts have not been consistent over time and space, and that they were not able to standardize the data entirely.
>
> **(d)** Recognition of inconsistency in the reporting of zeros (new leks vs inactive leks) that could lead to negatively biased trends.

(e) Acknowledgment that their assumption that the many missing data are random and bring no bias to the analyses is likely not always valid, and that they lacked any way to test this.

4. The authors discussed reasonable ways that data collection on sage grouse could be improved in the future to resolve questions on the usefulness of lek counts.

Some key questionable aspects were:

1. The tables at the end of this report list for every sampling unit and time period only one best model. It is hard to believe that there was always only one best model in terms of AIC. One of the values of AIC is that it rates models relative to each other and often ends up with multiple models that cannot be distinguished from each other in terms of explanatory power, i.e. are statistically equivalent. As such, on the surface it seems that the presentation of results in that report effectively ignored the AIC results and the basic underlying concept of that method. Further, there were many situations in which the confidence intervals around the coefficient for the quadratic term included zero, which casts considerable question as to whether the quadratic model was really supported statistically. It is likely that in those situations one or more models with fewer parameters were equivalent to the quadratic one in terms of AIC, but it appears that the reader was not provided that information.

2. In this regard, the graphs of linear mixed effects model results seemed questionable and appeared to be an artifact of using a quadratic model for relationships that are not parabolic and would be more accurately modeled with a different function. There were a number of sampling units for which the plots of means and medians showed clearly no trend over the total time period, but for which the quadratic mixed effects models presented consistently showed a clear curvilinear declining trend, despite the sometimes wide initial confidence intervals (e.g. MZ I, MZ III, MZ V, Montana, Oregon). What this suggested was that the quadratic model used had an inherent bias to produce a declining trend for most data sets. Perhaps this was somehow driven in part by the steep increase in leks counted over time for most sampling units. Given such an apparent declining tendency of the mixed effects models, it was hard to consider any of those results credible. This left only the plots of means and medians as potentially useful. The finding of no clear change between 1965 and 2007 for the combined data appears acceptable given the limitations of the data used. The declining, then increasing, pattern in means between 1965 and 2007 for the combined data (Figure 2) may be a real pattern in the data. However, given that these results are derived from a mixture of different data sets, most of which changed considerably over that time frame, and all the potential biases associated with those changes, there is no way of knowing whether that pattern represents a biologically meaningful pattern or is an artifact of the data set.

3. The presentation overstated the interpretation of results, beginning with the title, in suggesting that the reputed trends in their analyses of lek counts implied population trends. This was disappointing given the discussions of the limitations of the data.

Whether their results translated to actual dynamics of the sagegrouse populations awaits the development of data that will allow an analysis of the linkage between those two variables.

B. Garton et al.

Garton et al. in a manuscript titled "Greater Sage-grouse Population Dynamics and Probability of Persistence" provide a new analysis of the same data used in the WAFWA analyses. Here I compare and contrast those two analyses to ask what new insights Garton et al. offer relative to this question. Garton et al. add considerable criticisms of the WAFWA analyses.

16.36) Population units analyzed. The WAFWA report presented data at 3 geographic scales: state, management zone, and range wide. They list in Appendix C 38 different "breeding" populations they used for the analyses that were one or more leks separated geographically by >20 km from other breeding populations in accordance with a definition of breeding populations provided by Connelly et al. (2003). Garton et al. increased that threshold distance to >30 km or other significant barriers, and used only 30 breeding populations having what they considered sufficient data, of which 23 offered sufficient data to model annual rates of population change. In arriving at their population definitions Garton et al. "carefully examined each state's and province's data base and removed questionable data, e.g., leks for which no count data could be provided, and replicate locations (>two separate but nearby locations that represented the same lek)". Two large populations (Great Basin core and Wyoming Basin) were split by Sage-Grouse Management Zone (SMZ) boundaries and each was split into three and two respective smaller populations to allow more meaningful analysis. Thus, population definition in Garton et al. was somewhat different from the WAFWA report, but they used the same larger management zones.

16.37) Time periods considered in analyses. Because previous analyses found apparent differences in lek count dynamics before and after 1987, Garton et al. also used 1987 to define 2 time periods. Additionally, Garton et al. grouped the data in 5 year blocks, using averages and associated statistics for each block.

16.38) Data limitations. One of the laudable aspects of the WAFWA report was the open recognition that the relationship between lek counts and population sizes is unknown and the recommendation that a better understanding of this is needed to interpret the results of lek counts. Garton et al. wrote the following:

"The same leks, or leks within the same area, have been counted by agency biologists for many years (Connelly et al. 2004). These leks were likely selected because they held many males, their accessibility, or for both reason. Although some states and provinces attempt to monitor all known leks, leks surveyed in most states and provinces are not a random sample of those available, yet when analyzed in a repeated measures framework, may provide unbiased and precise measures of the rate of change of populations".

Their approach was to treat the data as minimum counts, and to use the available data in a way that accounted for what might appear as data limitations, which the following quotes clarify:

> *"We assessed monitoring effort within individual SMZs and populations by examining the average number of leks and number of active leks censused over 5-yr periods. This allowed evaluation of overall monitoring effort—the number of leks counted. We calculated the change in number of leks censused to describe the manner in which monitoring effort grew exponentially over time. Methods were developed to estimate trend and annual rates of change ... that would not be biased by this increasing monitoring effort."*

> *"We developed an approach to analyzing this index which treats lek counts as a cluster sample of males within leks and applies ratio estimators to paired counts of males at leks in succeeding years to obtain unbiased estimators of $\lambda(t)$ the finite rate of change from the previous year to the present year and $\theta(t)$ its reciprocal. Population reconstruction using these unbiased estimators provides remarkably precise estimates of the rates of change for reconstructing the index in previous years and is not biased by changes in the number of leks counted in different years. These rates of change are the basis for our modeling efforts. Unfortunately the final year count of males attending leks is not based on a probability sample and cannot be used to infer the true number of males attending leks within the spatial region sampled, nor the true number of males present within the region, nor the breeding population of both males and females present within the spatial region sampled. Methods to replace this weak foundation of lek counts representing an unknown proportion of leks in a spatial region by a true probability sample of leks and breeding males and females in defined spatial areas have been proposed but not widely adopted at this time (Garton et al. 2007)."*

Thus, WAFWA and Garton et al. laid out clearly the potential limitations of the data used. The difference is that Garton et al. identified a sampling approach that the data appeared to fit and applied a statistical approach seemingly appropriate to this type of sampling.

16.38) Analytical approaches. As the above discussions indicate, Garton et al. and the WAFWA report used somewhat different data bases, but quite different analytical methods. The apparent problems with the WAFWA approach are listed above. The analytical approach of Garton et al. had multiple procedures:

> **(a)** careful use the lek count data to develop yearly (unbiased) estimates of $\lambda(t)$ for each lek with successive counts, which were then averaged for each population with precision of estimates.

> **(b)** use of the reciprocal of those $\lambda(t)$ estimates to back calculate (reconstruct) breeding population sizes beginning with a recent year in which the most leks

were counted. This effectively estimated how many male sage-grouse would have been counted in earlier years if the maximum number of leks counted had been counted every year. Garton et al. present a formula for estimating the compounding error of such a procedure and apply it to their reconstructed population data.

(c) use of those reconstructed populations to find best fit stochastic models. They considered 26 exponential and density-dependence growth models of varying numbers of parameters, including year, the 2 time periods, and time lags, employing AIC to evaluate models relative each other.

(d) use the models developed in (c) to project each population and management zone into the future for 30 and 100 years as population viability analyses (PVA). Pseudo-extinction levels considered were 500 and 50, based on concepts of levels at which genetic drift will cause high rates of loss of heterozygosity.

(e) Garton et al. also performed a metapopulation analysis which allowed migration of sage-grouse between populations up to 27 km distance.

16.39) Shortcomings of Garton et al. results.

16.39(a) Apparent low resolution of *λ(t)*. Inspection of the many tables of data by 5 year intervals indicates a high variance of *λ(t)* values within those time intervals such that among all the populations it appears that in all but a few 5 year periods the 95% confidence intervals (CI) will include 1.0, i.e. the data appear to lack resolution to detect if the population is increasing or decreasing with any confidence.

16.39(b) Low resolution of population reconstructions. Figures 2-8 are plots of the reconstructed populations with 90% confidence limits. For most plots those asymmetric CIs are so wide that no trend can be supported at that confidence level. At 95% the CIs would be yet much wider. Nowhere in the text was there any discussion of why 90% might be more appropriate. My guess is that at 95% the CIs were all outrageously wide and would have resulted in nothing to discuss about a data base with no resolution.

16.39(c) Lack of accounting for error. The large error around the reconstructed population data apparently did not enter in the attempt to model that population history. That modeling itself had seemingly poor resolution without consideration of the resolution of the population history. In Appendix 1, Garton et al. list data for best AIC models of their reconstructed population data. In that table the 26 r^2 values range from 0 to 0.682, the highest of which is for a population with data only for 1996-2007, and the next closest value was 0.498. Average r^2 was only 0.257. What this indicates is that the models on average did not explain 75% of the variation of data sets that themselves had low resolution. That large unexplained variation would have been used as stochastic variation in the PVA

projections based on those models, the results of which would have been substantially driven by that high stochastic variation. Garton et al. discuss the poor resolution of their growth models, but seem to accept the idea of a highly stochastic system: "the inherently stochastic nature of population changes of sage-grouse will require use of stochastic growth models to forecast future potential for persistence of the species". In actuality, the unexplained variation in their population growth models is probably a combination of a variety of sources of variation, including relatively crude mathematical constructs to model a complex system. As such, stochastic variation used in the PVAs is likely exaggerated, perhaps considerably. Adding to that the low statistical resolution of the reconstructed populations for which the models were developed suggests that a great deal of error accompanies the PVA forward projections. Similar to the population reconstruction, that error will compound and grow exponentially. Garton et al. discuss this potential, but ultimately emphasize the literature that better supports their analyses. In reality, given the poor resolution of the reconstructed population data base and the growth models based on it, the PVA projections probably incorporate a great deal of compounding error that likely renders projections at even 30 years as meaningless.

16.39(d) This leaves almost no clearly useful analytical results in what Garton et al. produced. What may be useful results are a couple of the reconstructed population histories (Summit-Morgan Counties, UT; Snake River Plain Management Zone) which had narrow enough 90% CIs to indicate a statistically supported negative trend over time. Even those trends are probably not supported at the 95% level. As with the WAFWA report, probably the most useful aspect of Garton et al. is the discussion of how population data collection on sage-grouse should be improved to develop statistically more useful data.

Review of:
GREATER SAGE-GROUSE POPULATION DYNAMICS AND PROBABILITY OF PERSISTENCE

Review by: Dr. Vernon C. Bleich

In this paper, the authors accumulated and analyzed counts of male Greater Sage-Grouse (*Centrocercus urophasianus*) at 9,780 leks identified since 1965, and totaling 75,598 individual counts. Data streams for many leks were substantial, with some counts having occurred over more than 40 years; the final year for which data were included in the analyses was 2007. The authors grouped leks into 30 populations, which were distributed among 7 previously identified sage-grouse management zones. The authors stated that this approach allowed grouping of leks into "biological meaningful populations", and 23 of the 30 populations were sufficiently data rich to model annual rates of population change. The authors then developed models of annual rates of population change, and used information-theoretic criteria to select the best model for each population for which sufficient time series of data were available. Overall, models predicted declining carry capacity through time ranging from -1.8% to -11.6%, and 18% of the models incorporated lower carrying capacity from 1987 – 2007 when compared to 1967 – 1987.

The authors established quasi-extinction thresholds (effective populations sizes) of 50 birds ($N_e = 50$) per population and 500 birds ($N_e = 500$) per sage grouse management zone (SMZ), and then used model forecasts to suggest that 13% of the populations, but none of the SMZs, may decline below 50 birds within 30 years, while 75% of the populations and 29% of the SMZs were likely to decline below 500 birds within 100 years if current conditions persisted. They then concluded that *"preventing high probabilities of extinction … will require concerted efforts to decrease continuing loss and degradation of habitat as well addressing other [potentially limiting] factors … at local scales."*

Specific Comments

16.40) This entire paper represents a retrospective analysis of data collected by dozens of individuals, over > 40 years, at hundreds of locations, using sometimes unconfirmed or unsubstantiated methodologies that may or may not have been consistent with accepted criteria for counting male sage grouse on leks. The authors state that they examined all such data prior to analysis to ensure that they were obtained following appropriate procedures, *"… but in some cases … had to assume that they were collected properly."* This begs the question of why the authors did not discard those questionable data but, rather, included them in the analyses.

16.41) The authors acknowledge that counts of male sage grouse on leks represent only an index to the minimum number of breeding males in a local area; there has been, however, no verification of the relationship between lek count data and population sizes, a major weakness in this index. Based on results reported by others (Connelly et al. 2004), they concluded that use of lek counts to assess change over a relatively large scale

was a sound technique, and they made no attempt to assess population dynamics at relatively small scales (e.g., harvest units) or estimate true population abundance using lek count data. I conclude that the models presented really represent relative changes over time, and at broad scales.

16.42) The authors established analysis periods of 5-year blocks of time over which data were averaged, and that they state correspond with "typical planning and assessment periods for management agencies), but included 8 years (2000-2007) of data into the final analysis period. They also divided the assessment period into two approximately equal time frames (1967- 1987 and 1987 – 2007) because previous analyses of similar data (Connelly et al. 2004) indicated that populations declined more steeply during the first period than during the latter. As such, there appears to have been some biological reason for establishing those two periods. Nevertheless, there appears to be no biological reason for combining data into 5-year blocks, and I suggest that results may have differed had they, say, used a different length of time to cut the data. In most cases, they also did not use the initial 2 years of data, which is understandable because it allowed models to be built with 1 and 2 year delays that reflected potential density dependent effects.

16.43) In an attempt to minimize problems, the authors did not include aerial lek count data, information from leks with only a single count per year, or data from "leks" that could not be confirmed as leks, but continued to assume that other data were collected following established procedures, which seems like a leap. Again, it is unclear why they would not have eliminated data that were *assumed* to be OK, and worked with a smaller, yet more robust, data set.

16.44) The authors acknowledged that information on genetic structure, movements, habitat boundaries, and demographic correlations are necessary to delineate local breeding concentrations, or demes, as pointed out by Garton (2002). For the purposes of their analyses, however, the authors assumed that breeding populations could be defined as a group of sage grouse using one or more occupied leks in the "same geographic area" but separated from other such leks by > 20 km (Connelly et al. 2003). In some situations, delineations of demes or metapopulations based on spatial criteria (e.g., Bleich et al. 1996) ultimately have been demonstrated to be correct (Epps et al. 2007), but such has not, to the best of my knowledge, been the case with sage grouse. Hence, what truly represents a breeding population remains subject to question.

16.55) Despite employing the definition of Connelly et al. (2003) for a breeding population, the authors further defined breeding populations if they consisted of concentrated areas of leks separated from the "nearest adjacent concentration of leks by at least 30 km" or were separated from other such concentrations by unsuitable habitat (Connelly et al. 2004). Thus, it is unclear to this reviewer just what the authors ultimately considered to be breeding populations, which were then grouped into 7 SMZs defined by previous investigators (Miller and Eddleman 2001, Connelly et al. 2004, Stiver et al. 2006).

16.56) The authors elected to treat each SMZ as a subpopulation of a metapopulation (that apparently consisted of the entire range of sage grouse) because of high correlations in demographic parameters (growth rates), "little genetic differentiation amongst populations", and the enhanced value of large sample sizes of leks to enhance precision of estimates of abundance. The decision to do so raises the following issues.

16.57) Subpopulations comprising metapopulations are expected to have the potential to be independently dynamic, but it does not necessarily mean that dynamics must be independent, as would be the case if environmental influences were affecting subpopulations across a large geographic area. Moreover, genetic similarities among subpopulations would be expected if interchange among those subpopulations were occurring, and such movement would meet the assumption that the potential for colonization of areas from which birds had been extirpated (e.g., leks or populations within limited geographic areas). Finally, enhancing sample size to generate more precise estimates of abundance seems to be a contrived rationale for defining the SGMs as metapopulations. As such, I question the merit of combining all leks within each SMZ into a single subpopulation of the metapopulation, and then defining the metapopulation as combinations of the 7 SMZs. I question the appropriateness of defining the SMZs as a subpopulation of a "metapopulation" of sage grouse that includes the entire range of the species, and suggest that the authors should not have attempted an analysis based on their definition of the metapopulation.

16.58) The authors divided two large populations (Great Basin and Wyoming Basin), which were split by SMZ boundaries into three and two smaller "populations", respectively, to allow more "meaningful" analysis. It is unclear why those populations were included in multiple SMZs in the first place, and doing so presents the appearance that the authors modified their original definition of populations provided earlier in the text.

16.59) There is an inconsistency between the statement that combing all lek counts within an SMZ allowed them to use all lek counts meeting standards for quality within each SMZ, and the previous acknowledgment that the authors were forced to assume in some cases that data were gathered correctly. Either lek counts were conducted properly, or they were not. The authors need to clarify that "meeting standards for quality" includes some data that are assumed to have met those standards.

16.60) Methods used to reconstruct populations back through time are complex, but seem reasonable if the limitations of the data are acknowledged. The authors also considered the potential influence of varying sampling effort and attempted to control for that bias by applying a ratio estimator to estimate the finite rate of population change between leks counted in consecutive years, and considered the ratio of males counted in a pair of successive years to be estimates of the finite rate of population in that interval. They then combined ratios across leks within a population to generate finite growth rate for the population, and across all leks within an SMZ to estimate the finite growth rate between successive years. These data were used to reconstruct the estimated numbers of male

sage grouse back through time, allowing the investigators to generate information about relative changes that occurred over time.

16.61) Population modeling was a complex process, and the authors fit a total of 26 models, evaluating their relative merit using an information theoretic approach. Models included a surrogate for density-dependent effects in the form of time delays ranging from 0 to 2 years, a period effect (first 20 years vs. second 20 years), and time trend in population carrying capacity, as represented by year. Ricker and Gompertz models both were evaluated and the authors argue that both provide an objective approach to estimate carrying capacity, defined as the population size at which the finite rate of growth is zero, and represents a threshold in abundance below which population size tends to increase, and above which population size tends to decrease.

Population projections were derived using parametric bootstraps on minimum population size in 2007 and projecting 100,000 replicate abundance trajectories for 30 and 100 years into the future for each qualifying population, and for SMZs. The authors estimated the probability of "quasi-extinction" (see below) by determining the proportion of replications in which population abundance declined below 20 or 100 males at some point during the 30 or 100 year time horizon for populations or SMZs, respectively.

16.62) The authors selected population sizes of 50 and 500 birds to represent quasi-extinction levels for sage grouse representing local populations (50) or SMZs (500) in the short term and long term, respectively. These values were derived from previously published estimates that an effective population size ≥ 50 and an effective population size ≥ 500 are necessary for short and long-term population persistence (Franklin 1980, Soule 1980), respectively, because they would minimize the negative influences of inbreeding depression and subsequent impacts to individual fitness. Nevertheless, the appropriateness of selecting values of 50 and 500 is questionable, given the uncertainties of just what constitutes an effective population, and whether or not those values really have any biological meaning in a conservation context given the importance of factors affecting demography when compared to genetic issues in small populations.

16.63) Based on the work of others (Patterson 1952, Schroeder et al. 1999, Aldridge 2001, Bush 2009), the authors concluded that a population containing 20 breeding males would represent an effective population size (N_e) of 50 birds (necessary for short-term population persistence), and that 200 breeding males would represent an effective population size of 500 birds (necessary for long-term population persistence). From the information provided, it was not possible for me to ascertain if these values (50, 500) were in reference to birds on leks (unlikely, given the range in counts of males provided in the tables), comprising populations (that is what I assumed), or SGMs (also what I assumed); nevertheless, the authors indicate that a local population or SMZ declining below effective population size of < 50 or < 500 breeding adults are at short and long-term risks of extinction. This seems to be nothing more than clarifying the obvious, and I again question the appropriateness of the selection of those values.

16.64) The authors use projected population levels to estimate the probability that the number of sage grouse occurring in a population or an SMZ will fall below 50 adults or 500 adults in both short term (30 years) and long term (100 years) projections. This fails to recognize that, although N_e can be based on current population levels, it is affected by the past history of the population. If a population has undergone one or more bottlenecks in its history, N_e may already have been reduced relative to what it might have been had those bottlenecks not occurred. Again, the authors have assumed that there is something magical about the values of 50 and 500, and did not adequately explore the derivation of those values, and how N_e can be interpreted in differing contexts (e.g., bottlenecks on effective breeding population size).

16.65) Although the authors elected to use values of 50 and 500 birds as thresholds for quasi-extinction, I am not sure those values are appropriate or their use justifiable; the numbers originated in the concept of the deleterious effects of inbreeding depression. Impacts to demographic processes are of much greater concern than are genetic issues when populations are small (Lande 1988), and the use of population sizes of 50 and 500 individuals to determine probabilities of persistence seems questionable. Nevertheless, the authors present data suggesting a high probability of populations (and even SGMs) declining below those thresholds both in short term (30 years) and long term (100 years) projections. I suppose they had to establish some threshold below which populations were expected to become endangered with extinction, but the arbitrary selection of those values, across such a large area, remains questionable.

16.66) The authors also examined the potential for metapopulation persistence, but relied on estimated dispersal rates among the SMZs that composed the metapopulation and determined the probability of dispersal between every pair of leks using graph theory, based on distance between known leks, the difference in size between adjacent leks, with dispersal distances limited to 27 km between any pair of leks. The methodology used is inadequately explained to be meaningful to this reviewer, but it appears that it involves the inclusion of parameters that are largely speculative in nature (or based on models of what would be anticipated to occur) and a number of assumptions that seem to be not fully justified. The best Gompertz models and Ricker models appeared to provide conflicting results. The Gompertz models suggested a low probability that sage grouse in the metapopulation would fall to < 30,000 males within the next 30 years, or to < 5,000 males within 100 years, and the mean final abundance was 45,870 and 39,817 males after 30 and 100 years, respectively; the mean minimum abundance was 6,965 and 5,998 males after the same lengths of time. In contrast, mean projections based on the best Ricker models suggested a low probability that sage grouse would fall below 3,000 males within 30 years, but a 100% chance of extinction within 100 years. Mean final abundance was 5,652 and 0 males after 30 and 100 years, and the mean minimum abundance was 5,577 and 0 males after 30 and 100 years, respectively, according to the Ricker models. These projections assume that current levels of risk to sage grouse remain unchanged, and whether or not that is a reasonable assumption is questionable.

16.67) The authors have included tables that provide detailed summaries of the results of lek counts across 7 SMZs, and for 30 populations, and were able to reconstruct

populations back to 1967 for 23 populations and 6 SMZs. Moreover, the results of their calculations are presented in detail for each of the 30 populations and 7 SGMs, with the exception of those that lacked sufficient data to allow populations to be reconstructed. These tables are potentially useful summaries of the history of each population from roughly 1967 to 2007, and allow the reader to quickly compare through time the mean number of leks counted, mean males/lek, mean active leks, mean percent active leks, mean males/active lek, mean finite rate of population growth (λ), and the SE of λ by five-year periods. They also provide graphics depicting population reconstructions and associated confidence intervals for the same time period. These tables provide an easy means by which interested parties can review historical information, and the graphics readily convey population trends based on population reconstructions. As such, they will be useful to individuals concerned with the dynamics, management history, and relative population dynamics of sage grouse comprising specific populations or occurring in SMZs, and really represent the most useful part of this paper.

16.68) The authors state that their analyses are based only on attempted censuses of males that met their standard for quality, but (as noted earlier), include data based on the assumption that their standards were met.

16.69) The absence of a probability based sampling scheme for the data analyzed precluded an unbiased estimate of the proportion of leks that disappeared during the sampling period (1965 – 2007), as well as an estimate of newly established (or discovered?) leks, and precluded the modeling of impacts of habitat changes or other factors that could affect sage grouse abundance, distribution, or population dynamics. Recognizing the shortcomings in the data used in their analyses and, to their credit, the authors readily advocated for establishment of range-wide, standardized methodologies based on probability sampling of leks and breeding males and females that would allow more meaningful analyses in the future, as pointed out by Garton et al. (2007). This is probably the second most useful part of this paper, but only if such methods are adopted range wide, and implemented in a manner that will yield meaningful results in the future, as advocated by the Sage- and Columbian Sharp-tailed Grouse Technical Committee (2008).

16.70) The results presented in this paper are consistent with results of other less sophisticated analyses (e.g., Connelly et al. 2004, Sage- and Columbian Sharp-tailed Grouse Technical Committee 2007) that inferred a long-term downward trend in sage grouse numbers in western North America. Neither of those previous analyses offered a means of assessing the true magnitude of population change, nor does the current analysis, although the precisions of recent population indices are markedly tighter than those based on earlier counts involving smaller samples. That the pattern repeats itself suggests that there is something to the conclusions reached by these authors; nevertheless, this is a retrospective analysis that incorporates sophisticated models of population dynamics, and the results are subject to the limitations of the data used to develop the models.

16.71) Where it was available, data, including those on population structure at harvest, chicks per hen, relative abundance during summer, chicks per adult, and mean brood size, hunting opportunity, and harvest rates from appropriate periods as evidence seem to corroborate conclusions that populations of sage grouse were high in the 1960s and early 1970s, consistent with the population reconstructions presented in this paper.

16.72) The statement that the multi-model predictions of the likelihood of individuals populations of sage grouse declining below 50 and 500 within 30 years are underestimates because they are based solely on the 23 populations for which sufficient data existed to build stochastic population models seems suspect. In the absence of data adequate to build those models, it does not appear to be appropriate to reach that conclusion. Indeed, absence of evidence does not equate to evidence of absence.

16.73) The authors readily acknowledge that not even their best models explained 50% of the variation in annual rates of change, and emphasize that decreasing error in lek counts using probability sampling approaches and incorporating meaningful predictive factors (i.e., environmental characteristics of lek sites or populations) into growth models will reduce the unexplained variation associated with growth models. Again, this is a plea for establishment of a probability based sampling approach to monitoring sage grouse populations.

16.74) The authors acknowledge the problematic nature of population indices exceeding the projected carrying capacities of specific populations, and offer three potential explanations for this phenomenon: (1) estimated carrying capacity represents a quasi-equilibrium rather than an upper limit, and that growth rates tend to be negative above that equilibrium point, and positive below it; (2) carrying capacity, as calculated, characterizes the median abundance rather than mean abundance; and (3) the cyclic nature of populations as indicated by the significance of delayed density dependence in models. I concur that each of these seems like a plausible explanation, and it is appropriate for them to have attempted to explain why population indices might exceed projected carrying capacities.

16.75) The authors acknowledge the shortcomings associated with attempting to project population viability for conditions outside of the range of the variables used to develop the model(s), but acknowledge that some of the dominant influences on sage grouse populations can change in the future, thereby altering future trajectories to the benefit of sage grouse populations. In emphasizing this shortcoming, they appear to be cognizant of the potential for their predictions, as written, to be utilized to justify "writing off" certain populations, because extinction is preordained or predicted to occur (this is my interpretation of their cautionary note). A similar concern was voiced a few years ago, when advocates of bighorn sheep conservation voiced concern that minimum viable populations (as defined by the Bureau of Land Management) could lead managers to make decisions contrary to the best interest of the conservation of those ungulates, because populations were not deemed to be viable.

16.76) Future trajectories of sage grouse populations will be affected by already well established processes, including the continued invasion of exotic species, particularly cheatgrass (*Bromus tectorum*), expansion of coniferous species into upper elevations, altered fire regimes and, potentially, the influences of global climate change that will further modify existing sagebrush rangelands. Additionally, uncertainties exist with respect to the influences of West Nile Virus and expanded modification of habitat resulting from energy development, and these uncertainties will influence future population trajectories, but the authors acknowledge that the ultimate influence of these unprecedented landscape changes are not well understood for sage grouse. Nevertheless, 44% of models presented in this paper indicate continued declines in carry capacities for sage grouse through time, and 18% incorporated a lower carrying capacity during 1987 – 2007 when compared to 1967 – 1987. The authors interpret these results as consistent with those of other investigators that have concluded a continuing decline in the quality and quantity of habitat for sage grouse. I concur that this conclusion is reasonable, but again subject to the limitations of the data used to reach those conclusions. The overall relative trend is downward, but trying to associate absolute numbers with declining populations is a reach.

16.77) In summary, models can be useful when making predictions regarding the future, but the accuracy of such models is questionable because the predictions are based on incomplete information obtained in the past. In the analyses presented in this paper, the authors incorporated time lags, estimates of carrying capacity, recent changes in rates of change, and surrogates for density dependence in an effort to increase the coefficients of determination of the models, with a resultant increase in confidence that the forecasts will be useful to land mangers making decisions that affect sage grouse or sage brush habitat.

Identification of 50 and 500 individuals as thresholds of quasi-extinction does not seem to be well founded, and likely are not very meaningful in the overall interpretation of the probability of population persistence. However, if projections are taken to be relative estimates of the probability of populations remaining viable into the future, larger populations will persist longer than smaller ones, and extirpations will take place over a longer period of time, assuming that factors currently affecting sage grouse and their habitat remain unchanged.

The authors used retrospective analyses of long-term data streams that they felt met their expectations in terms of data quality to synthesize population trends of 23 populations of sage grouse and 6 sage grouse management zones into the future. They likely used an inappropriate metric (N_e) as a measure of quasi-extinction, with the assumption that grouse populations declining below thresholds of 50 in the short term and 500 in the long term were destined for extinction. Nevertheless, the models generated are more robust than, and are consistent with, previous reports that concluded there has been an overall decline in sage grouse abundance over the past 4 decades.

Chapter 17 (2009), Chapter 17 (2011):
INFLUENCES OF ENVIRONMENTAL AND ANTHROPOGENIC FEATURES ON GREATER SAGE-GROUSE POPULATIONS, 1997–2007

Authors: Douglas H. Johnson, Matthew J. Holloran, John W. Connelly, Steven E. Hanser, Courtney L. Amundson, and Steven T. Knick

Abstract from Johnson et al.
"The Greater Sage-Grouse (Centrocercus urophasianus) is endemic to western North America and of great conservation interest. Its populations are tracked by spring counts of males at lek sites. We explored the relations between trends of Greater Sage-Grouse lek counts during 1997–2007 and a variety of natural and anthropogenic features. We found that trends were correlated with several habitat features, but not always similarly throughout the range. Lek trends were positively associated with proportion of sagebrush (Artemisia spp.) cover, within 5 km and 18 km. Lek trends had negative associations with the coverage of agriculture and exotic plant species. Trends also tended to be lower for leks where a greater proportion of their surrounding landscape had been burned. Few leks were located within 5 km of developed land and trends were lower for those leks with more developed land within 5 km or 18 km. Lek trends were reduced where communication towers were nearby, whereas no effect of power lines was detected. Active oil or natural gas wells and highways, but not secondary roads, were associated with lower trends. Effects of some anthropogenic features may have already been manifested before our study period and thus not have been detected in this analysis. Results of this range- wide analysis complement those from more intensive studies on smaller areas. Our findings are important for identifying features that could threaten Greater Sage-Grouse populations."

Review of:
INFLUENCES OF ENVIRONMENTAL AND ANTHROPOGENIC FEATURES ON GREATER SAGE-GROUSE POPULATIONS, 1997–2007

Review by: Dr. Rob Roy Ramey II and Dr. Laura M. Brown

This paper seeks to determine whether specific activities are correlated with population level declines in sage grouse, as determined from lek count trend data. The idea is to identify quantifiable threats to sage grouse populations.

17.1) The authors examined 62 different variables (Table 1) using only 11 years of lek count data for the response variable in seven different management zones. This study is an example of an extremely weak approach to statistical inference and a poorly planned data-fishing expedition. There are simply not enough years of data to support inferences with single variables, much less several variables. By chance alone, several variables should show correlations with lek count trends. The problem is compounded by the fact that many of the lek counts had *only four years of data* associated with them.

17.2) From the "Conservation Implications" section at the end of this paper you would not know that lek counts have generally increased over the 10-year period that this study looked at (Figure 2), although the authors have several convenient caveats to explain this away.

17.3) Basically, the figures tell the story, that there are no significant correlations between predictor and response variables. These are essentially random clouds of points. The authors resort to loess smoothing to search for patterns in the data that do not have obvious statistical significance. Despite this, the authors report on "trends" and discuss the potential importance of these in the paper.

Consequently, the resolution of the data and the methods applied to them is extremely limited. The authors admission of limitations and caveats is not enough to salvage the results or redeem weak inferences based on them. Had this paper undergone a rigorous and independent peer-review, it would have almost certainly been rejected. It is doubtful that this paper would be considered publishable in most reputable scientific journals.

17.4) The authors used data from 9,844 leks but *"only the 3,679 leks with at least four annual counts during the 11-yr period were included* [in analyses]." In comparison, Garton et al. (Chapter 16 used data from 9,780 leks. The reason for the difference between two studies using the same data, in the same monograph, with many of the same coauthors is not explained.

17.5) In the last lines of the paper, the authors voice a number platitudes that are consistent with the message of other papers in this monograph: *"No single factor is responsible for declining sage-grouse populations, and no single action may be sufficient to restore them. Conservation of the species will initially require a recognition of the intrinsic value of sagebrush-dominated landscapes, followed by the development of a*

comprehensive approach to sagebrush habitat conservation that involves commitments and partnerships among state and federal agencies, academia, industry, private organizations, and landowners; Knick et al. (2003:627) affirm that only through this concerted effort and commitment can we afford to be optimistic about the future of sagebrush ecosystems and their avifauna."

Chapter 18 (2009), Chapter 16 (2011):
CONNECTING PATTERN AND PROCESS IN GREATER SAGE-GROUSE POPULATIONS AND SAGEBRUSH LANDSCAPES

Authors: Steven T. Knick and Steven E. Hanser

Abstract from Knick and Hanser:
"Spatial patterns influence the processes that maintain Greater Sage-Grouse (Centrocercus urophasianus) populations and sagebrush (Artemisia spp.) landscapes on which they depend. We used connectivity analyses to: (1) delineate the dominant pattern of sagebrush landscapes, (2) identify regions of the current range-wide distribution of Greater Sage-Grouse important for conservation, (3) estimate distance thresholds that potentially isolate populations, and (4) understand how landscape pattern, environmental disturbance, or location within the spatial network influenced lek persistence during a population decline. Long-term viability of sagebrush, assessed from its dominance in relatively unfragmented landscapes, likely is greatest in south central Oregon and northwest Nevada; the Owyhee region of southeast Oregon, southwest Idaho, and northern Nevada; southwest Wyoming; and south central Wyoming. The most important leks (breeding locations) for maintaining connectivity, characterized by higher counts of sage-grouse and connections with other leks, were within the core regions of the sagegrouse range. Sage-grouse populations presently have the highest levels of connectivity in the Wyoming Basin and lowest in the Columbian Basin management zones. Leks separated by distances >13–18 km could be isolated due to decreased probability of dispersals from neighboring leks. The range-wide distribution of sage-grouse was clustered into 209 separate components (units in which leks were interconnected within but not among) when dispersal was limited to distances <18 km. The most important components for maintaining connectivity were distributed across the central and eastern regions of the range-wide distribution. Connectivity among sage-grouse populations was lost during population declines from 1965–1979 to 1998- 2007, most dramatically in the Columbia Basin management zone. Leks that persisted during this period were larger in size, more highly connected, and had lower levels of broad-scale fire and human disturbance. Protecting core regions and maintaining connectivity with more isolated sage-grouse populations may help reverse or stabilize the processes of range contraction and isolation that have resulted in long-term population declines."

Review of:
CONNECTING PATTERN AND PROCESS IN GREATER SAGE-GROUSE POPULATIONS AND SAGEBRUSH LANDSCAPES

Review by: Dr. Rob Roy Ramey II and Dr. Laura M. Brown

This was a modeling and data fishing project looking at different factors (lek connectivity, sagebrush presence, fire, edge, and human footprint at four different spatial scales) leading to the disappearance of leks. Basically, the results were as expected: leks on the periphery, without connectivity, in areas that had been burned, or were closest to human development, were extirpated first: *"In our study, fire within a 54-km radius and human activity within 5 km of a lek influenced the probability of persistence over 40 years."*

This analysis relies on the same lek count data as other chapters in this monograph, except that they are used a subset of the data and only the examined factors are correlated with the loss of leks. The authors used a subset of known lek locations , ones *"that had been surveyed at least once within each interval from 1965–1974, 1980–1989, and 1998–2007 to avoid confounding analyses caused by increases in sampling effort that added new lek locations."* This produced a limited data set (Table 2).

18.1) A fundamental problem with this analysis is that lek persistence data are used in lieu of actual population data, and the analysis rests on the critical assumption that population persistence and lek persistence are strongly correlated. For example, if leks had simply moved because of disturbance (e.g. fire) then the analysis would treat the lek as extirpated when the subpopulation birds that comprise it were not extirpated.

18.2) Although the data were originally at a 30m resolution, the authors resampled at a 540m resolution, claiming that they *"were able to detect relatively fine-scale patterns at this resolution when considered at the spatial extent of the SGCA."* The authors do not acknowledge that this rescaling could be expected to inflate the effects of disturbance.

18.3) The stated objectives were to: *"(1) characterize the hierarchical pattern of sagebrush landscapes that results from natural and human disturbance, and (2) identify spatial scales perceived by greater sage grouse and other wildlife."* The second objective is unusual in that it suggests a belief that sage grouse have a spatial awareness, a property that is only found in animals of higher intelligence. The authors also describe populations and landscapes in terms of their being hierarchically structured. What is not clear, is whether the authors believe that the structure they describe is an emergent property or an artifact of their analysis.

18.4) The authors' belief that "little is known about the connectivity and ability for spatially structured populations to exchange individuals," is contrary to the abundant field and genetic data showing ongoing long distance dispersal (>18km). (This aspect is discussed extensively in the reviews of Chapter 16 of this monograph,

Garton et al.)

18.5) The authors were *"unable to identify a specific source of human disturbance because the score represented a summed influence of all anthropogenic features."* Thus, they concluded that *"the cumulative effect of human activities may have a greater influence on persistence of sage-grouse populations than single land uses."* This ignores the relative influence (effect size) of specific types of disturbance on sage grouse populations and assumes that they all contribute to sage grouse decline, when in fact some do not. This is not a sound epistemological basis for informed management decisions.

18.6) A more robust analysis would include a logistic regression approach to model population presence/absence. If lek presence/absence data were substituted, then the analysis could only refer to factors leading to the extirpation of leks, and that would best be done at a more limited, regional scale (e.g. sage grouse management zone). Results would be compared to a range wide analysis. Ideally, the variables selected for analysis should be winnowed down on the basis of plausible cause and effect mechanisms, and those likely to have the largest effect sizes. In that way, variables can be treated as testable hypotheses.

Chapter 19 (2009), Chapter 18 (2011):
FACTORS ASSOCIATED WITH EXTIRPATION OF SAGE-GROUSE

Authors: Michael J. Wisdom, Cara W. Meinke, Steven T. Knick, and Michael A. Schroeder

Abstract from Wisdom et al.:
"Geographic ranges of Greater Sage-Grouse (Centrocercus urophasianus) and Gunnison Sage-Grouse (Centrocercus minimus) have contracted across large areas in response to habitat loss and detrimental land uses. However, quantitative analyses of the environmental factors most closely associated with range contraction have been lacking, results of which could be highly relevant to conservation planning. Consequently, we analyzed differences in 22 environmental variables between areas of former range (extirpated range), and areas still occupied by the two species (occupied range). Fifteen of the 22 variables, representing a broad spectrum of biotic, abiotic, and anthropogenic conditions, had mean values that were significantly different between extirpated and occupied ranges. Best discrimination between extirpated and occupied ranges, using discriminant function analysis (DFA), was provided by 5 of these variables: sagebrush (Artemisia spp.) area; elevation; distance to transmission lines; distance to cellular towers; and land ownership. A DFA model containing these 5 variables correctly classified >80% of sage-grouse historical locations to extirpated and occupied ranges. We used this model to estimate the similarity between areas of occupied range with areas where extirpation has occurred. Areas currently occupied by sage-grouse, but with high similarity to extirpated range, may not support persistent populations. Model estimates showed that areas of highest similarity were concentrated in the smallest, disjunct portions of occupied range and along range peripheries. Large areas in the eastern portion of occupied range also had high similarity with extirpated range. By contrast, areas of lowest similarity with extirpated range were concentrated in the largest, most contiguous portions of occupied range that dominate Oregon, Idaho, Nevada, and western Wyoming. Our results have direct relevance to planning. We describe how results can be used to identify strongholds and spatial priorities for effective landscape management of sage-grouse."

Review of:
FACTORS ASSOCIATED WITH EXTIRPATION OF SAGE-GROUSE

Review by: Dr. Rob Roy Ramey II

This chapter uses discriminant function analysis (DFA) on 22 environmental variables to model the environmental variables that best predict extirpated vs. extant sage grouse populations. Discriminant analysis is used to select variables and develop models that discriminate between two or more groups. With bootstrap resampling procedures and posterior probabilities calculated for each observation, the model can be tested to see how well it performed with classifying the observations used to develop it.

There are a number of statistical issues with the analysis that are not addressed by the authors. Also, the definition of historic habitat (which is used for selecting locations of extirpated populations) is based on circular reasoning because historic locations outside of existing sagebrush habitat were excluded. Published analyses that are of higher quality address the similar questions, making this study superfluous.

19.1) A weak threshold was used for Discriminant Function Analysis classifications. The authors developed a DFA model containing five variables that correctly classified >80% of sage-grouse historical locations in extirpated and occupied ranges. It is assumed, because it was not reported otherwise, that the authors used the default settings in the statistical program SAS for classifying locations as either "extirpated" or "occupied." The SAS subroutine PROC DISCRIM computes posterior probabilities for membership in each group. The default setting is 0.5 for posterior probabilities and by default, PROC DISCRIM classifies an observation into a group based on the larger of the two posterior probabilities for each observation. In other words, a value of 0.51 would result in a correct assignment while a value 0.49 would result in an incorrect classification. To use more discriminating posterior probabilities, ones that would result in more certain assignments (e.g. posterior probabilities of $\geq$0.95), additional steps are required. Specifically, the THRESHOLD= option must be specified. If the posterior probability for an observation fails to meet the specified threshold it is classified as "OTHER" (SAS Institute, pers. comm.) If the authors had applied a higher threshold for posterior probabilities such as 0.95, their percent of correct classifications would have been much lower (but would have been made with greater certainty).

Willingness of the authors to accept such poor discriminant analysis assignments (some differing little from flips of a coin in terms of discrimination ability such as a posterior probability of 0.51) as a basis for setting policy is highly questionable.

19.2) At least three of the variables found by the authors to provide the best discrimination between occupied and extirpated areas were not independent. The authors did not acknowledge that transmission line towers and cell phone towers have a tendency to be placed on high points, and thus these two variables and the elevation variable are not independent. For example, transmission towers must be placed hilltops and ridges in order for transmission lines to make large spans. Even small

elevation gains make for longer spans, reducing the number of towers needed. Cell towers are typically placed at higher points where they can cover broader areas. They are frequently built on private land because of regulatory and leasing considerations.

It is unclear whether the authors distinguished between cell towers and *cell antenna arrays* that are placed on existing towers, including radio and transmission line towers, and buildings. Such an error would increase cell tower density in urban areas. The fact that transmission and distribution lines frequently follow roads was not acknowledged by the authors. Both of these contribute to the non-independence of variables.

19.3) The authors advance several far-fetched and pseudoscientific explanations regarding the potential effects of transmission lines and cell towers. For example: *"The strong association between distance to cellular towers and sage-grouse extirpation was an especially intriguing result, given that no previous studies of sage-grouse have evaluated this variable. Whether cellular towers function in a cause-effect manner or simply are aligned with other detrimental factors cannot be addressed without additional research. Recent studies, however, suggest possible cause-effect relationships between high levels of electromagnetic radiation within 500 m of cellular towers and reduced population or reproductive performance of a limited number of bird and amphibian species (Balmori 2005, 2006; Balmori and Hallberg 2007, Everaert and Bauwens 2007). These negative effects are similar to those documented for bird species exposed to electromagnetic radiation generated by power lines (Fernie and Reynolds 2005). Cellular towers also are likely to cause sage-grouse mortality via collisions with these structures or influence movements by visual obstruction, but no research has investigated these issues."* A problem with the studies cited is their speculative basis and lack of repeatability. This reviewer does not share the author's view that cell towers represent a significant collision hazard for sage grouse.

19.4) The authors did not distinguish between different types of electrical transmission lines even though these would be expected to have different effects on sage grouse. The failure of the authors to distinguish between different types of transmission lines confounds their effects and leads to erroneous conclusions. Electricity is transmitted at high voltages to reduce the energy lost in long distance transmission. Therefore, long-haul transmission line voltages are typically 230kV and higher, and placed on towers approximately 50m in height. This puts their cables above the usual flight height of sage grouse. Transmission sub-lines are typically in the 69 to 169kV range, and placed on towers ranging in height from 20m to 30m. Distribution lines, commonly referred to as "powerlines" are in the 12 to 34kV range and 10 to 20m in height. The greatest hazard to sage grouse is posed by older distribution lines that are low-lying. Beck et al. (2006) reported powerline collisions of this type as well as other sources of mortality: *"Of total mortalities avian predation was the cause of death for 36% of grouse, followed by mammal predation (27%), power-line collisions (18%), legal harvest (9%), and unknown cause (9%; Fig. 2)."* Because sage grouse rely on explosive bursts of speed, and short, ground-hugging flights to avoid predators, the maximum height of their flight is quite limited.

19.5) The assumption of independence among variables is not convincing. The most significant variables within each group of the 22 environmental variables are obviously correlated. For example, the significant "biotic variables" are all related to sagebrush coverage (*sagebrush area, patch size of sagebrush, proximity of sagebrush patches, size of sagebrush core areas, and distance to the boundary between occupied and extirpated ranges*); "abiotic variables" are all related to elevation (*elevation, soil water capacity, and soil salinity*), and "anthropogenic variables" to development and roads (*agriculture, human density, road density, distance to highways, distance to electric transmission lines, distance to cellular towers, and land ownership*). One must question why the authors did not recognize these interactions among variables and question whether they would violate statistical assumptions. Prior to embarking on discriminant analysis, many of these variables should have been eliminated. Instead the authors simply stated that: *"Correlation coefficients among all discriminatory variables were <0.35, positive or negative, indicating that stepwise procedures could be used."* The authors should have asked whether those correlations were statistically significant.

Many of the issues above could have been avoided if the authors had simply put effort into testing for correlations among variables (e.g. bivariate plots and regression procedures).

It is surprising that the authors did not present scatterplots of Mahalanobis distances from their discriminant analysis output. These are useful for visualizing the discriminating ability of the model and the certainty of individual classifications. Instead, the authors simply report the percent "correctly" classified without considering weak thresholds for discrimination (see comments above regarding posterior probabilities).

Because testing for multivariate normality is difficult, most researchers substitute univariate tests of normality. Instead, the authors relied on a qualitative approach: *"Examination of the frequency distributions of each variable showed that data were normally distributed for all variables within both classification groups, thus meeting this assumption."*

The authors did not utilize tests for detecting and eliminating outliers, such as the Grubb's and Dixon's tests (Sokal & Rohlf, 1981). Outliers can violate assumptions of significance tests of discriminant analysis, producing erroneous results. This is why univariate descriptive statistics and bivariate plots for outlier detection are important steps prior to undertaking discriminant analysis. This aspect of the paper is notably deficient which raises questions regarding the validity of results.

19.6) The authors rely on circular reasoning to claim *"these results support past studies that identified sage grouse as a sagebrush obligate, dependent upon sagebrush for persistence."* The author's analysis (as well as Schroeder et al. 2004) is based on the subjective exclusion of observations and specimens *outside* of sagebrush habitat. How can it be denied that these were *not* sage grouse habitat, if sage grouse were living in them? The presence of these observations and specimens falsifies the dogma that sage grouse are *sagebrush obligates*. Clearly sagebrush is the preferred habitat of sage grouse

and where the majority are currently found, however, historic observations show that it is *not* essential for their survival.

19.7) The hypothesis that human structures (e.g. transmission line towers, distribution line poles, and cell towers) serve as perches that facilitate raptor predation on sage grouse deserves further exploration. Raptors and ravens regularly perch on power poles, as any driver down a country road will attest. And the common feature shared by transmission line towers, power poles, cell towers, and drill rigs is their *height*. Therefore, it is important to ask 1) whether habitat near powerlines represents an increased predation risk compared to similar habitat farther removed, and 2) whether sage grouse avoidance of tall objects in the environment is an innate or learned behavior. Predation risk can be quantified experimentally or estimated from previously published studies. Separating innate from learned behavior is more problematic but could be approached experimentally.

While it is potentially straightforward to install anti-perch devices as a mitigation measure, the more difficult question is how to mitigate the effects if there is an innate tendency to avoid tall objects? Possible mitigation in core habitat includes: burying of transmission lines in sensitive areas, building cell towers away from high quality grouse habitat, and concentrating drill rigs (e.g. through the use of horizontal drilling technology).

Despite the obvious importance of these issues, the authors gave only a brief, half-sentence to the idea that transmission line towers might facilitate predation on sage grouse.

19.8) A suggestion of merit by the authors is the concept of sage grouse population "strongholds" as conservation priorities. However, such prioritization of effort could be based on information other than this paper. Not including the analyses from this paper in prioritization would be a prudent conservation strategy because the analyses have a questionable basis. In this regard, Aldridge et al. (2008), who used different data and a logistic regression approach, would be a more useful alternative. The primary limitation of the Aldridge et al. (2008) paper was its reliance on Schroeder et al.'s (2004) subjective pre-settlement map.

19.9) The discussion is of excessive length relative to reported results. It restates the obvious (e.g. that peripheral or disjunct populations are at greater risk of extirpation), and repeats policy and management suggestions presented elsewhere in this monograph.

Review of:
FACTORS ASSOCIATED WITH EXTIRPATION OF SAGE-GROUSE

Review by: Dr. Robert M. Zink

19.20) The authors of this interesting paper do not differentiate Greater Sage-grouse (*Centrocercus urophasianus*) and Gunnison Sage-Grouse (*C. minimus*), thus in this review they are both lumped as sage-grouse.

19.21) The authors claim that *"a myriad of widely distributed birds and mammals have experienced large contractions in their historical ranges…"* According to the Encarta Dictionary, "myriad" means "too numerous to count". This sort of hyperbole does not set the paper on a solid foundation. There is no doubt that many species have experienced range contractions, and it is likely that others are experiencing range expansions.

19.22) The paper aims to identify environmental factors that might have resulted in the regional extirpation of sage-grouse. There were 4 specific objectives: 1) identify environmental factors that might explain why sage-grouse are currently not found in some plots where they were historically presumed to be present, 2) use these factors to evaluate which currently occupied plots might be more likely to experience future extirpations of sage-grouse, 3) use the results for conservation planning, and 4) suggest future research needs.

19.23) The analysis is based on Schroeder et al.'s (2004) estimate of historical and current ranges. That paper, however, can only be interpreted as a very general guide to sage-grouse occurrence. For example, just because Schroeder et al. (2004) estimated that sage-grouse might occur in some area, there is no guarantee that it is an optimal area or one that would have self-sustaining populations. Also, it is not possible to know the historical density of sage-grouse in either plot category; i.e., some might have been suboptimal. However, it is the only such effort available, and was used by Wisdom et al. But there is an element of error in the estimates of Schroeder et al. (2004), of unknown proportion, which is carried forward in this paper.

19.24) The authors do not consider the fundamental point that over time, any species' range can change for purely natural reasons. Thus, the authors assume that areas where sage grouse were once estimated to be but are not detected on modern surveys is a result of human-caused extirpation. However, there is some non-zero expectation that an area once used by a species will no longer be used owing to habitat succession or other naturally occurring events. The potential magnitude of this effect is unknown, but affects both Schroeder et al. (2004) and Wisdom et al.

19.25) The basic idea of Wisdom et al. was to identify plots where sage-grouse occurred historically, and then to identify plots that today are either still occupied (N = 239) or no longer occupied (extirpated; 136). The circular plots were ca. 1020 km^2 in area. They then attempted to identify what environmental changes occurred in the extirpated plots,

relative to the still-occupied plots, to determine why the extirpated plots no longer supported sage-grouse.

The analysis involved 22 environmental variables, which were chosen *a priori* because they *"likely differed"* between occupied and extirpated plots. Thus, there was a potential bias in the features of the environment chosen for study. Nine variables were "biological measures" and included area of sagebrush, patch size and density, two measures of edge density (sagebrush adjacent to non-sagebrush), nearest neighbor (mean distance between sagebrush patches), proximity index (among patches), core area (sagebrush within 100 m of the edge of each patch), distance to occupied-extirpated boundary. Five represented abiotic variables: mean annual precipitation, elevation, soil water capacity, soil rock depth and soil salinity. Lastly, there were 8 variables related to human-land interactions (anthropogenic): agricultural area, human density, distance to roads, road density, distance to major highways, distance to nearest electrical transmission line, distance to nearest registered cell tower, and land ownership.

19.26) Several of the variables were not clearly explained. For instance, if a road went through one of the circular plots, what was the distance to the nearest road (presumably 0)? This is especially unclear as road density is the area of roadways (roads vs highways) "within" the 18-km radius plots. It is also not clear how some of the variables differed, such as the "edge" variables.

19.27) As the authors note, a problem with this sort of analysis is geographic variation in climate. Ideally, there would be a large area of relatively homogeneous climate so that the relationship between it and the other variables could be multiply tested. However, there are likely many interactions among the 22 variables, and these interactions likely differ across the sampled space, confounding interpretation. That is, in the extreme NW there might be a certain relationship between precipitation and sage grouse occurrence, but a different relationship might occur in the extreme SE. In effect, the analysis "averages" out these relationships, meaning that the average across the whole area might not apply to any particular area.

19.28) The authors chose discriminant function analysis, which seeks to find combinations of variables that best discriminate among *a priori* groups (occupied, extirpated). They note that this is an appropriate procedure when variables are quantitative and normally distributed – it is not clear that several of their variables meet this criterion (e.g. land ownership). Personally, I think that a better choice was to uses principal components analysis for each group separately to see whether different combinations of variables explained the maximum amount of variance. I think this would provide a better indication of the nature of differences between occupied and extirpated plots. Of course in a PCA one would have to do some standardization to account for the different scales of measurement.

19.29) It is also of concern that plots are classified as occupied or extirpated, which might be overly coarse-grained. A better approach might be to consider the relationship between the environmental variables and the density of the sage-grouse (as done

elsewhere in this volume (Hanser and Knick); the authors attempt (see below) to consider strongholds gets as this issue.

19.30) The authors state that there are low correlations between the variable measured, yet in several places they note that there are correlations between different variables. For example, plots with higher percentages of sagebrush would have fewer roads (at least within them). Larger expanses of sagebrush would necessarily have fewer roads and people. Thus, I am unconvinced that the correlations are as low as suggested by the authors (all < 0.3).

19.31) The paper engages in a distracting number of multiple analyses. It is generally acknowledged that testing the same data multiple ways does not constitute independent tests. Different combinations of the same variables are interpreted as though they were independent analyses (e.g., Table 3). This makes it difficult to ascertain exactly which analyses should be interpreted. Indeed, the authors pick and choose sets of variables to discuss (a PCA would have identified important variables without this bias). Furthermore, there is no statistical test of which models are better, only a comparison of how well they perform in discrimination.

19.32) The results of the many analyses did not discover any strikingly unexpected conclusions. Occupied plots contained ca. twice as much sagebrush, mean patch size was 9 times larger and mean core area was 11 times larger, then extirpated plots. In other words, sage-grouse are aptly named. Occupied patches were also substantially closer to each other, which is expected as dispersal via corridors can maintain populations in the event of local extirpation. Three of five abiotic variables were significant, and occupied plots tended to be higher in elevation and salinity, and lower in water capacity of the soil.

19.33) Several significant anthropogenic variables differed between occupied and extirpated plots. Occupied plots had less area in agriculture and lower human density. There were also fewer roads, greater distances to transmission and cell towers, and more public ownership. These are all obviously correlated. Sage-grouse occur in sagebrush, not on roads, agricultural plots, or cities. One could argue only somewhat tongue-in-cheek that early naturalists knew this 200 years ago.

19.34) Again, it is difficult to evaluate the discriminatory models owing to overlap in which variables were used. At best, an analysis with all biotic and anthropogenic variables classified 72% of occupied and 80% of extirpated plots. However, an analysis with only sagebrush area classified 76% of occupied and 65% of extirpated plots. Thus the 17 variables in the former analysis only did slightly better than the single variable, sagebrush area in the second analysis. Confounding this is likely the geographic interaction among many of the variables.

19.35) The sections on “individual variables and biotic, abiotic and anthropogenic groups” vs “best-performing combinations of variables”, were confusing. However, there were some post hoc reasons for choosing variables in the four additional models in the latter section. The best performing model contained sagebrush area, elevation,

distance to transmission lines, distance to cell towers and land ownership, and correctly classified 85% of occupied and 83% of extirpated plots. It seems likely that at least some of these variables are highly correlated (again despite statements to the contrary) such as elevation and land ownership (a non-quantitative variable that is not normally distributed). For example later in the paper the authors point out that "Elevation was a good discriminator, probably because most sagebrush loss has occurred disproportionately at lower elevations where human activities and developments have been concentrated…." This sounds like strong correlation to me. Some of the misclassifications involved Great Plains plots, were the birds are probably not well adapted or common – thus casting some doubt on whether the results are an "average" over a large area and not necessarily applicable to a local area. That is, an analysis excluding Great Plains populations might be informative, as it would not be factored in with other ecologically disparate areas.

19.36) It is also important to recognize that a variable's occurrence in a model does not mean that it alone explains all of the variation in occupied vs extirpated plots. In many places in the paper, the authors single out specific variables without acknowledging that they do not explain all of the variation, and have likely complex interactions with other variables. In other words, sage-grouse presence is likely a complex outcome of many variables, including some potentially not measured (hunting pressure?), and it is not clear that the variables can be interpreted independently. The role of exotic grasses seems especially important, as the authors note.

19.37) Other conclusions are interesting but it is not clear how generally applicable they are. For instance, the authors say that landscapes with sage-grouse with less than 27% sagebrush have a greater than 97.5% probability of matching an extirpated site. It is not clear if this is true throughout the large area they studied or only in some places. It is also troublesome that in the discussion, they continue to discuss the results of various models (combinations of the same variables) as if they were independent analyses.

19.38) The authors point out that species including sage-grouse are likely more susceptible to extirpation at the periphery of the range. However, they did not classify the historical range they used to establish occupied vs extirpated as to which areas might be peripheral – instead, all potentially occupied areas were considered as sage-grouse habitat, or not. Thus, the perception of historical range is inflated, because in all species, there are core areas, and then reduced densities (and increased vulnerability) as one moves towards the range boundary.

19.39) It is difficult to disagree with their statement that "sage-grouse extirpation is associated with a varied combination of biotic, abiotic, and anthropogenic influences, and that holistic consideration of these many environmental factors…appears important to maintain persistent populations…". However, this statement is true for all species.

19.40) I thought that the authors attempt to identify "strongholds" was very clever. They used model 2 to identify strongholds. In short, finding areas of occupied territory that nonetheless possess the characteristics of extirpated areas, the authors could hypothesize

where existing sage–grouse populations are at greatest risk. Conversely, those occupied areas that shared the least in common with extirpated areas could be considered places where sage-grouse had the highest likelihood of continued persistence, which they termed "strongholds". Of course, ironically, it is in these areas that populations might not be declining. The authors did not state whether there are Breeding Bird Surveys (http://www.pwrc.usgs.gov/BBS/results/), or other surveys, in the areas identified as strongholds, which might indicate population trends. It would be useful to know whether populations in these strongholds are increasing, decreasing or holding steady. Nonetheless, these areas could be future preserves for the species, or areas where breeding stock might be taken for translocation. In particular their suggestion of protecting strongholds is appropriate because this is likely cheaper in the long run than restoring degraded areas elsewhere.

19.41) The paper's main conclusion boils down to the observation that areas where sage-grouse occur today have larger uninterrupted expanses of sagebrush than those areas where they do not occur. This conclusion might be inferred without much analysis (I suspect it is common knowledge among hunters). Sage-grouse are obligate sagebrush birds, and being large-bodied, need relatively large areas to be successful. The other correlates of sage grouse occurrence are potentially correlates of large patch size – a larger contiguous patch of sagebrush will have fewer roads, and longer distances to towers (transmission, cell) and be less fragmented. Thus, their conclusion that extirpated range contained 27 times the human density, 3 times more area on agriculture, was 60% close to highways and had a higher density of roads, is not surprising.

19.42) If sage-grouse were listed, the strongholds might function as areas of critical habitat. However, this paper does not identify strongholds per se, but identifies which combinations of the 22 variables are associated with them. Where this analysis might be useful is in restoration of habitat for sage-grouse, if that route were taken by managers. Wisdom et al. have identified some conservation guidelines that might guide restoration away from current strongholds. Some of these are obvious, such as large expanses of sagebrush that are relatively close together and on publically owned lands, although proximity was not identified in their best model (yet the importance of corridors is supported by many other studies). The relatively novel suggestion that distance to cell towers (and transmission towers) merits further study. If for example, cell towers provide perches for raptors preying on young sage-grouse, there are steps to discourage such behaviors.

Chapter 20 (2009), Chapter 19 (2011):
GREATER SAGE-GROUSE AS AN UMBRELLA SPECIES FOR SHRUBLAND PASSERINE BIRDS: A MULTI-SCALE ASSESSMENT

Authors: Steven E. Hanser and Steven T. Knick

Abstract from Hanser and Knick:
"Working groups and government agencies are planning and conducting land actions in sagebrush (Artemisia spp.) habitats to benefit Greater Sage-Grouse (Centrocercus urophasianus) populations. Managers have adopted an umbrella concept, by creating habitat characteristics specific to sage-grouse requirements, in the belief that other wildlife species dependent on sagebrush will benefit. We tested the efficacy of this approach by first identifying the primary environmental gradients underlying sagebrush steppe bird communities (including Greater Sage-Grouse). We integrated field sampling for birds and vegetation with geographic information system (GIS) data to characterize 305 sites sampled throughout the current range of Greater Sage-Grouse in the Intermountain West, US. The primary environmental axis defining the bird community represented a gradient from local-scale Wyoming and basin big sagebrush (Artemisia tridentata ssp. wyomingensis and A. t. ssp. tridentata), and bare ground cover to local and regional grassland cover; the second axis represented a transition from low elevation Wyoming and basin big sagebrush and bare ground to mountain big sagebrush (A. t. ssp. vaseyana) and habitat edge. We identified the relative overlap of sage-grouse with 13 species of passerine birds along the multi-scale gradients and estimated the width of the umbrella when applying management guidelines specific to sage-grouse. Passerine birds associated with sagebrush steppe habitats had high levels of overlap with Greater Sage-Grouse along the multi-scale environmental gradients. However, the overlap of the umbrella was primarily a function of the broad range of sagebrush habitats used by sage-grouse. Management that focuses on creating a narrow set of plot-scale conditions will likely be less effective than restoration efforts that recognize landscape scale heterogeneity and multi-scale organization of habitats. These multi-scale efforts may improve some sage-grouse habitats and strengthen the management umbrella for shrub-steppe passerine birds."

Review of:
GREATER SAGE-GROUSE AS AN UMBRELLA SPECIES FOR SHRUBLAND PASSERINE BIRDS: A MULTI-SCALE ASSESSMENT

Review by: Dr. Robert M. Zink

Overview
The chapter by Hanser and Knick (hereafter HK) evaluates the potential benefit of managing sagebrush habitat for Greater Sage-Grouse (GRSG; *Centrocercus urophasianus*) to 13 species of birds that use sagebrush habitat to varying degrees. The conceptual basis is that of the "umbrella species", which has been defined (Groom et al. 2006) as: "A wide-ranging species whose requirements include those of many other species". Hence, such species act as an "umbrella." Thus, if one preserves as much habitat as possible for populations of an umbrella species, such as GRSG, it will have the ancillary or indirect effect of providing suitable habitat for many other species. Earlier assessments of GRSG as an umbrella species were not supportive. HK criticized these studies as having too coarse a resolution, and suggested that their analysis of multiple habitat levels showed that management of sagebrush for GRSG will have beneficial effects on 13 species of passerine birds, ranging from obligate sagebrush species (Brewer's Sparrow [*Spizella breweri*], Sage Sparrow [*Amphispiza belli*], and Sage Thrasher [*Oreoscoptes montanus*]) to ten other species that depend to lesser extents on sagebrush communities. Most of these 13 species have been listed in one or more states or regions, thus efforts to increase their populations would be helpful.

Critique
20.1) The idea, although logical in theory, is nonetheless hypothetical in practice. No one has as yet determined if there are higher populations of the other 13 species in areas with optimally managed habitat for GRSG. That is, do these 13 species have peak population performance in microhabitats that promote maximum GRSG population viability? Perhaps more appropriately, do these 13 species do better in restored GRSG habitat than in the current sagebrush communities? This information is presumably available and could be used to test this hypothesis. At a broader scale, HK point out that over 350 species (other animals and plants) are dependent to some degree on sagebrush habitats, not just the 13 birds they considered. They did not estimate whether the GRSG would be an effective umbrella for the other 95% of the species that use sagebrush environments.

20.2) HK studied a large number of plots (305) in five states in which they gathered data on the following habitat variables: % Low-black sagebrush, % Mountain big sagebrush, % Wyoming-basin big sagebrush, % Total sagebrush, Sagebrush height, %Grass, % Forb, % Total native grass and forb, % Exotic grass, % Bare ground, % Litter. Each of these was analyzed at the plot scale (180 x 180 m), 1 km and 5 km scales. They also recorded presence of birds, and the presence of GRSG droppings (pellets). These data sets are intensive, although it is not clear how valid an index of GRSG presence can be obtained from counts of pellets. Although HK say that they can remain for up to 3 years on a site, it is not clear how long one can obtain a valid estimate of the presence of GRSG after the pellets were deposited, nor what time of the year the pellets were cast. Densities

of the 13 species were not considered from the census data because it was not possible to convert GRSG pellet counts to density; hence, all birds were compared as either present or absent from a plot. It is not apparent whether the habitat or bird census data are publically available; it appears they are not.

20.3) Various analyses were performed on the habitat data at the different spatial scales. Computations were made that quantify similarity among habitat patches Shannon's diversity index was used to estimate landscape diversity. For the core of their analysis, HK performed a multivariate analysis of the habitat variables. This analysis, a canonical correspondence analysis (CCA), yields a linear combination of habitat variables that maximize the dispersion of species in multivariate space. The analysis identifies the relative contributions (or quantitative weights) of each of the variables to a set of axes (each of which is a linear combination of the variables). A high weight means that the variable contributes highly to the spread of points on the axes. Usually, only some variables contribute significant to explaining variation on a given axis, and that is interpreted in terms of those variables (e.g., plant species x height axis).

Of the 12 habitat variables, CCA I was most highly correlated with % grass cover, % of bare ground, and % Wyoming-basin big sagebrush; the remaining 9 variables were not deemed significant to explaining habitat variation among species. The coefficients for % of bare ground, and % Wyoming-basin big sagebrush were negative, meaning that less bare ground and Wyoming-basin big sagebrush, and more grass cover, were correlated with positive species' occurrences. It is possible to estimate both the amount of the total variance each axis explains. HK found that the first axis only "explained" 27% of the species-habitat relationship. In other words, 73% is explained by some unmeasured set of variables. The second axis was similar in explaining 24.8% of the species-habitat relationship, but was interpreted at the plot scale as a function of only the % Mountain big sagebrush, with no other variable being important. Although these are by no means atypical results, it indicates a high level of uncertainty in exactly what environmental factors limit distribution of GRSG, and the other species as well.

20.4) A CCA is only as good as the choice of included variables and their measurement. Although this is a standard type of analysis, it is a heuristic view of habitat associations, owing to the difficultly of including all of the habitat and environmental variables that describe where a species lives (and it ignores the wintering areas). Furthermore, the results are not discrete, but continuous. That is, they do not identify a discrete combination of characters that explain where a species lives (in ecological/environmental space), but identify a range of habitat characteristics that are correlated with species' occurrences. In this study, the species is represented as a centroid mean and a measure of dispersion equal to two standard deviations. Thus, there is a range of combinations of variables where species tend to occur, with lower densities likely at the ends of the species' environmental distributions. Looking at the figure (2) that summarizes this analysis, one sees that a continuous distribution of species on the axes. One needs to know how much the species overlap with GRSG on the habitat characteristics most important to them. A way to assess this is to determine, for the habitat variables measured, how much the species overlap in "habitat space."

20.5) From these data, the authors calculate overlap of each species habitat characteristics with the GRSG. These overlaps are judged subjectively as low (< 0.3), moderate (0.3 – 0.7) and high (0.7). They found that many of the 13 species overlapped in the multidimensional niche-space diagram. However, even a high value of overlap doesn't insure that GRSG is a good umbrella as it might exclude an important niche dimension for a particular species (e.g., nest sites), and does not indicate the species success during the migratory period or wintering grounds. Thus, it is unknown whether an increase in GRSG populations of say 10% or 20% would have a similar impact on the other species. It is very possible that many species listed as special concern suffer more from habitat degradation or other problems in migration or during winter. Also, at least three species do not share very high similarities with GRSG, suggesting that almost 25% of the species would not benefit from managing sagebrush habitats for GRSG.
Choice of 13 passerine species.

20.6) Two concerns relate to the species chosen. First, it is not clear why only species already in some peril were chosen. By suggesting that GRSG is an umbrella for these 13 species (actually only 10), one runs the risk of managing GRSG as an umbrella for these species while at the same time putting other, currently non-threatened species, in subsequent risk. Importantly, this could be not only birds, but mammals, plants, insects, or in short, any other organism. As HK note, there are up to 350 species that are associated with sagebrush environments. That is, what if focusing attention on the species chosen by HK brings populations of currently common species down to a point where they would be considered for listing. Hence, it does not make sense to limit the analysis to this set of species.

20.7) Secondly, it is unclear how "threatened" these species are. Are they only listed in states at the periphery of the species' range? If so, it is misleading to treat them all as "at risk". In Table 1, I used the same Breeding Bird Survey (BBS) data as those used by HK to determine the range-wide status of the species they studied. For every species listed as declining in the western U.S., the species is also declining range-wide. For example, Loggerhead Shrike is declining in the western U.S., as HK noted, but it is recognized to be in serious decline throughout its range. Thus, it is not just populations in sagebrush habitat that are experiencing declines, but all habitats. Of course, it might be that declining populations in sagebrush habitats drive the overall declining trends.

This suggestion is probably not the case. When one studies Table 1, it is apparent that 6 of the 13 species (or 14 if GRSG is counted) are not declining in the area surveyed by the Breeding Bird Survey (BBS; available free online). Of the obligate sagebrush species, those most likely to be positively influenced by managing sagebrush for GRSG, only 1 of 3 is declining significantly (Brewer's Sparrow). Also, there are three species that are not significantly declining in the western U.S. (Vesper Sparrow, Lark Sparrow, Savannah Sparrow) but they are declining range-wide; hence, they are apparently "doing worse" elsewhere than in sagebrush habitats.

A different approach?

20.8) The authors did not use GARP models to predict the distribution of GRSG, and then subsequently to predict the occurrence of the other species (Peterson 2001). That is, there is a large body of literature in which one uses environmental data, and then predicts the geographic distribution of the species by testing the predicted range with museum and sight records. If this can be done with efficiency, then one could use the GRSG information to predict the occurrence of other species. If GRSG is a good predictor of the other species, one would agree that it's a potentially valuable umbrella species. The correlative analysis done by HK suggests that this would probably not be the case.

Conclusions

1. This is an interesting analysis of the degree of habitat overlap, as measured by 13 variables, between GRSG and 13 other bird species.
2. There is little doubt that managing sagebrush habitat for GRSG will increase populations of GRSG.
3. It is not clear that GRSG can be an effective umbrella species for more than the three obligate sagebrush passerine birds studied. Even then, two of these are not in decline. It might be that habitat fragmentation is of a scale that is not detrimental to these other species, whereas it might be for GRSG.
4. Over 300 species use sagebrush habitats. Clearly it is unreasonable to ask HK to analyze all of these. Still, it is possible that using GRSG as an umbrella might cause unanticipated declines in other species, such as Green-tailed Towhee, Grasshopper Sparrow and Savannah Sparrow.
5. This analysis does not justify designation of GRSG as an umbrella species.

Table 1. Species studied in HK and their population status in breeding bird surveys range wide. BBS trends for Western NA and Status (an "X" indicates that the species is listed in at least one state or province) from HK. All columns are the same as in HK except the last column is added here for comparison of western NA and the entire ranged covered by the BBS.

Common	name	Scientific name		BBS Trend in Western NA	Status	BBS Trend Range-wide
Gray	Flycatcher	*Empidonax*	*wrightii*	4.61*	X	Positive*
Loggerhead	Shrike	*Lanius*	*ludovicianus*	-2.70***	X	Negative*
Horned	Lark	*Eremophila*	*alpestris*	-2.39***		Negative*
Sage	Thrasher	*Oreoscoptes*	*montanus*	ns	X	ns
Green-tailed	Towhee	*Pipilo*	*chlorurus*	ns	X	ns
Brewer's	Sparrow	*Spizella*	*breweri*	-2.01**	X	Negative*
Vesper	Sparrow	*Pooecetes*	*gramineus*	ns	X	Negative*
Lark	Sparrow	*Chondestes*	*grammacus*	ns		Negative*
Black-throated	Sparrow	*Amphispiza*	*bilineata*	-1.73***	X	Negative*
Sage	Sparrow	*Amphispiza*	*belli*	ns	X	ns
Savannah	Sparrow	*Passerculus*	*sandwichensis*	ns		Negative*
Grasshopper	Sparrow	*Ammodramus*	*savannarum*	-5.31***	X	Negative*
Western	Meadowlark	*Sturnella*	*neglecta*	-1.23***		Negative*

Chapter 21 (2009), Chapter 20 (2011):
ENERGY DEVELOPMENT AND GREATER SAGE-GROUSE

Authors: David E. Naugle, Kevin E. Doherty, Brett L. Walker, Matthew J. Holloran, and Holly E. Copeland

Abstract from Naugle et al.
"Rapidly expanding energy development in western North America poses a major new challenge for conservation of Greater Sage-Grouse (Centrocercus urophasianus). We reviewed the scientific literature documenting biological responses of sage-grouse to development, quantified changes in landscape features detrimental to sage-grouse that result from development, examined the potential for landscape-level expansion of energy development within sage-grouse range, and outlined recommended landscape-scale conservation strategies. Shrublands developed for energy production contained twice as many roads and power lines, and where ranching, energy development, and tillage agriculture coincided, human features were so dense that every 1 km^2 could be bounded by a road and bisected by a power line. Sage-grouse respond negatively to three different types of development and conventional densities of oil and gas wells far exceed the species' threshold of tolerance. These patterns were consistent among studies regardless of whether they examined lek dynamics or demographic rates of specific cohorts within populations. Severity of current and projected impacts indicates the need to shift from local to landscape conservation. The immediate need is for planning tools that overlay the best remaining areas for sage-grouse with the extent of current and anticipated development. This will allow stakeholders to consider a hierarchy of set-aside areas, lease consolidations, and more effective best-management practices as creative solutions to reduce losses. Multiple stressors including energy development must be managed collectively to maintain sage-grouse populations over time in priority landscapes.

Review of:
ENERGY DEVELOPMENT AND GREATER SAGE-GROUSE

Review by: Dr. Rob Roy Ramey II

This paper purports to provide:
1) *"the scientific literature documenting biological responses of sage-grouse to development,"*
2) *"quantified changes in landscape features detrimental to sage-grouse that result from development,"*
3) *"examined the potential for landscape-level expansion of energy development within sage-grouse range, and"*
4) *"outlined recommended landscape-scale conservation strategies"*

21.1) This is not an impartial review of the scientific literature. The authors examined 32 published papers, reports, management plans, and theses regarding biological responses of sage-grouse to energy development. The authors dismissed all but four peer-reviewed publications, one unpublished dissertation, one unpublished M.S. thesis, and a USGS report in their summary. This *"critical review"* is not impartial because the authors of Naugle et al. are also authors on four of the seven pieces of the literature reviewed. Studies not written by the authors of Naugle et al. were reinterpreted.

21.2) Notable, is one of the studies *not* written by Naugle et al. That study, (Aldridge and Boyce 2007) used empirical data to model both "sources" and "sinks" for nesting and brood-rearing habitat in southern Alberta, including an area of energy development. Aldridge and Boyce (2007) then produced a high-resolution map (with a 30 m pixel resolution and a 1km shifting frame) to identify non-habitat, nesting and brood-rearing habitats (priorities for protection), and key sink habitats *"which provide managers with the ideal opportunity to evaluate management alternatives aimed at increasing productivity through habitat management following an adaptive management framework."* Aldridge and Boyce (2007) also provided several key departures from the standard paradigm on sage grouse conservation: 1) the traditional focus on habitat protection around lek sites *"may not be suitable to ensure the viability of Sage-Grouse populations"*, 2) nest success was independent of anthropogenic features, and although birds tended to avoided human development, chick mortalities tended to occur in proximity to oil and gas developments and along riparian habitats, and 3) approximately 60% of the study area was low occurrence/noncritical habitat. All three are in contrast to the coarse mapping efforts of other authors used to recommend conservation priorities and policy: Doherty et al. (this issue), Copeland et al. (2009), and Naugle et al. (this issue).

Naugle et al.'s summary of Aldridge and Boyce (2007) did not mention these aspects but focused instead on sage grouse mortalities and avoidance/abandonment of habitat near oil and gas fields:

"In an endangered population in Alberta, Canada, where low chick survival (12%

to 56 days) limits population growth, risk of chick mortality in the Manyberries Oil Field was 1.5 times higher for each additional well site visible within 1 km of a brood location (Aldridge and Boyce 2007).

Negative responses of sage-grouse to energy development were consistent among studies regardless of whether they examined lek dynamics or demographic rates of specific cohorts within populations. Recent research demonstrated that sage-grouse populations declined when birds behaviorally avoid infrastructure in one or more seasons (Doherty et al. 2008), when cumulative impacts of development negatively affect reproduction or survival (Aldridge and Boyce 2007)

Avoidance of energy development reduces the distribution of sage-grouse and may result in population declines if density dependence, competition or displacement into poor-quality habitats lowers survival or reproduction among displaced birds (Holloran and Anderson 2005, Aldridge and Boyce 2007)."

21.3) Naugle et al. inaccurately represented results of the other peer review paper that they were not authors on (Lyon and Anderson 2003), in support of a statement that males and female grouse abandon leks due to *"noise and human activity associated with energy development."* While this seems like a logical finding of such a study, in fact Lyon and Anderson (2003) never mentioned abandonment. Instead, Lyon and Anderson (2003) reported that: *"Hens we captured on disturbed leks demonstrated greater movements from capture lek to nest than hens from undisturbed leks. Hens from disturbed leks nested approximately twice as far from capture leks as did hens from undisturbed leks."* Lyon and Anderson (2003) also reported that females tended to nest farther from roads *"due to light road traffic (1-12 vehicles per day) during breeding."* The primary impact of energy development was thought to be related to traffic and that additional traffic restrictions might be considered.

21.4) Four of the seven studies reviewed by Naugle et al. focused on impacts to sage grouse in Pinedale/Jonah Field area and two in Powder River Basin. Thus, these represent studies of *intensive* energy development and are not necessarily representative of less intensive energy development, development based on newer environmental regulations, or newer technologies.

21.5) The authors briefly mention mechanisms that *may* result in some of the avoidance behavior by sage grouse, however the primary focus is on "impacts". The paper is therefore lacking in analysis of understanding *why* grouse may avoid energy development or have lower survivorship adjacent to it, which is key to mitigating its effects.

21.6) Naugle et al. repeatedly refer to the need for "landscape level" or "landscape-scale" effects to sage grouse. However, the authors do not provide a definition of "landscape level" impacts and the need for "landscape-scale" conservation strategies. Although they use the term "landscape" 21 times in this paper, Naugle et al. never provide a definition for "landscape" in this context.

Examples are provided below:
"Severity of current and projected impacts [of energy development] *indicates the need to shift from local to landscape conservation."*

"Finally, we recommend a paradigm shift from local to landscape conservation and discuss the implications of this change."

"We quantified changes in landscape features detrimental to sage grouse that result from energy development."

21.7) Naugle et al.'s assertion that sage grouse are "landscape specialists," is both unsupported and left undefined, and appears to be a unique invention of the term by these authors. It could be argued that most species have a "landscape" requirement and thus, the term is meaningless.

Naugle et al. used the term as follows: *"sage-grouse are landscape specialists that require large and intact sagebrush habitats to maintain populations."*

Webster's dictionary defines "landscape specialist" quite differently from the author's apparent intended use: *a human grounds keeper*.

21.8) Much of Naugle et al.'s lengthy discussion is devoted to quantifying impacts or potential impacts of development on sage grouse based on correlative studies, and recommending policies based on those. What is lacking are testable hypotheses regarding *why* sage grouse may be impacted by various types and intensities of development. By focusing only on the pattern and not the process, Naugle et al. emphasize a research and regulatory approach that only focuses on large scale or "landscape level" conservation strategies. This ignores mitigation and enhancement opportunities at a local level that can be based on an understanding of *why* sage grouse avoid or do poorly in response to particular situations. In this regard, the paradigm offered by Aldridge and Boyce (2003), provides an attractive alternative.

Review of:
ENERGY DEVELOPMENT AND GREATER SAGE-GROUSE

Review by: Dr. Matthew A. Cronin

In this chapter the potential for energy (oil and gas) development to affect sage grouse is assessed. Relationships of sage grouse to energy development, tillage agriculture, and livestock ranching were also assessed. It is stressed that energy development in sage grouse range in the western states is increasing rapidly and this could impact sage grouse. It is noted that sage grouse have been extirpated from almost half of the original range. The authors used coal bed methane development in the Powder River Basin in northeast Wyoming as a case study to assess habitat changes detrimental to sage grouse. A case is made for a paradigm shift from local management to implementing "landscape conservation".

The primary impact of oil and gas development as described is disturbance of leks (i.e., breeding grounds). Disturbance can be from raptors perching on power lines, vehicle traffic, and noise and human activity. There also can be mortality from collisions with power lines and vehicles and predation by raptors. Man-made ponds may also support mosquitoes with West Nile virus.

First the authors assessed development in the Powder River basin. The authors used satellite imagery from 2003 to classify land cover for a 9,081 km^2area. Land uses were:

1. Ranch lands,
2. ranch lands with energy development,
3. ranch lands with tillage agriculture, and
4. ranch lands with energy development and tillage agriculture.

Gas wells, power lines, roads, ponds, and tilled agricultural land were identified on grids, and the density of these was considered as potentially affecting sage grouse. They found ranching was the most environmentally benign land use with fewer human features than the other land uses. The highest density of human features was where ranching, tillage agriculture and energy development co-occur and in these areas 70% of the land was within 100 meters of human features.

Next, the authors assessed the response of sage grouse to energy development by reviewing the literature, primarily seven scientific studies.
- These studies reported negative impacts of energy development on sage grouse.
- There were no reports of positive impacts.
- Development in excess of one pad/2.6 km^2 impacted breeding populations.
- Conventional pad densities of eight pads/2.6 km^2 exceeded sage grouse threshold of tolerance (presumably meaning abandonment of the area).
- Numbers of grouse in leks in gas fields declined by 82% from 2001-2005 and numbers outside the gas fields declined by 12%.

- By 2004-2005, 38% of the leks inside gas fields remained active and 84% of the leks outside gas fields remained active.

The BLM stipulation of no surface infrastructure within 0.4 km of a lek was assessed. Impacts to leks were discernable out to > 6 km and have led to "extirpation of leks within gas fields". This probably means abandonment of an area for breeding, that is, the birds probably moved to another location so extirpation may not be the appropriate term.
- Development "influenced" numbers of displaying males to 4.7 to 6.2 km from infrastructure.
- Models indicated a strong negative effect of energy development on lek persistence within 0.8 km or 3.2 km of a lek and showed negative impacts out to 6.4 km.
- These results were used to show that the BLM stipulation of 0.4 km distance of development from leks was insufficient to conserve breeding sage grouse populations in fully developed gas fields. A 0.4 km stipulation results in 98% of the landscape with infrastructure within 3.2 km of leks and would reduce the probability of lek persistence from 87% to 5%.

21.9) Negative impacts were found for leks and demographics of populations. Populations declined with avoidance of infrastructure or when cumulative impacts affect reproduction or survival. However, the authors note that avoidance of energy development reduces the distribution of sage grouse and **may** result in population declines if density, competition, or displacement to poor habitats reduces survival or reproduction.

It is not clear if the cited research documented population declines or simply speculated it could occur. It appears that displacement from leks has been shown, but its not clear if there are good data on population numbers (including simple relocation to other areas, as opposed to mortality and total failure to reproduce).

21.10) Areas other than leks were briefly discussed.
- Avoidance of winter habitats with energy development was cited.
- Nest sites were further from disturbed than undisturbed leks and nest initiation was lower for birds breeding on disturbed leks. In contrast, and perhaps in contradiction to these points, adult females remained in traditional nesting areas with development, but yearling females nested farther from haul roads and avoided infrastructure.

21.11) The authors note that sage grouse declines are partly explained by lower annual survival of females and this resulted in a population level decline. It is not clear if this is documented or suspected.

There is high site fidelity but low survival of adult sage grouse, and lek avoidance of young birds resulted in a lag of 3-4 years between the onset of development and lek loss.

21.12) The literature cited in this chapter needs thorough review and reanalysis. The authors contend that the scientific evidence shows that energy development is impacting sage grouse populations, though the exact mechanisms are not certain. However, the

disturbance from leks is presented as definitive so the uncertainty needs to be better explained.

21.13) The authors note that the efficacy of mitigation methods need testing. Mitigation methods such as burying power lines, minimizing roads and pads, minimizing vehicle traffic and noise, and managing produced water were mentioned. It was stated that rigorous testing is needed to know if these or other methods will allow sage grouse to persist in developed areas. This seems like the critical point, and other issues are secondary to simple development of low impact development scenarios.

21.14) It is noted that translocations and reintroductions of sage grouse are rarely successful so population level impacts are a major concern. This seems to be a problem for the wildlife biologists to solve, not accept.

21.15) The authors then describe current and future energy development in sage grouse ranges and state there is increased risk of further decline of sage grouse distribution and abundance and call for a "fundamental shift from local to landscape conservation". In doing so they ignore their call for research on mitigation measures which is the obvious need for effective multiple use management. There is no definition of "landscape" but it seems to indicate range-wide planning.

21.16) The authors discuss "Conservation Implications. This actually refers to management implications. Conservation (of sage grouse in this case) is but one management objective.

21.17) There was considerable uncertainty of the overall impacts of energy development on sage grouse, particularly with regard to the potential for enhanced mitigation measures to minimize displacement from leks and impacts on nesting and survival. However, the authors state: *"Severity of impacts and continued leasing… dictate the need to shift from local to landscape conservation."* This is not particularly meaningful. Local management is clearly needed to ensure effective mitigation and can allow local populations to be maintained. "Landscape" seems to refer to the status of the species over large ranges. This should be part of the basic state management and cooperation among states for common species.

21.18) It is stated that federal and state government and industries need to implement solutions at a large scale. They suggest that one approach is to forego development in priority landscapes until new best management practices are implemented. This is reasonable. However, Connelly et al. (2000) note that mining and oil and gas development can have negative impacts on sage grouse but that populations can recover after the development ceased. This critical point is that both temporal and spatial management are needed. Development with subsequent restoration of areas with oil and gas resources can occur over time to maintain populations over the range of the species. Coupled with development of effective mitigation to minimize impacts close to development, this approach would allow achieving multiple objectives without excluding development from large areas.

21.19) Additional factors (climate change, habitat loss, range management, disease) are mentioned that may affect sage grouse. The potential impact of sport hunting and predation are not discussed in this chapter. Hunting will incur mortality and disturbance and fear of humans. This may contribute significantly to disturbance from energy developments if the birds are afraid of humans with whom they associate danger.

21.20) The treatment of "conservation implications" implies the entire species is somehow at risk, when the review so far has only documented some (potentially manageable) local impacts. It is an effort to coordinate management of sage grouse across the western U.S., which in itself is not a bad idea. The problem is this has created the impression the species is at risk of large scale declines and endangerment. There is no supporting evidence of this in this chapter. The numbers of sage grouse in various states and local areas needs tabulation before this landscape perspective is practicable. I would assume each state already has knowledge of their sage grouse populations.

21.21) The authors note that restoration programs in areas already developed could re-establish populations. This is a good idea and related to the development of effective mitigation methods.

21.22) It's not clear how much of the sage grouse habitat is likely to be anywhere near development. The maps are very large scale, with dots representing gas wells. Dots are not to scale so it's not clear what the actual distribution of wells and sage grouse habitat looks like. A detailed atlas of sage grouse populations and oil and gas development is needed to properly assess these issues.

21.23) The issue is very similar to that with caribou in the North Slope Alaska oilfields. Some local disturbance/displacement impacts were suspected. Then they are assumed to be definitive, and speculated to have population level impacts. These perceptions persist despite evidence the local disturbances on caribou can be effectively mitigated and the population has grown dramatically (from 5000 to 67000) since the oil fields were developed. It is important to note that there is no hunting in the oil fields, which may contribute to their habituation to human activity. Comparative assessments with other species and other energy developments can provide insights to help plan sage grouse management.

Chapter 22 (2009), Chapter 21 (2011):
ENERGY DEVELOPMENT AND CONSERVATION TRADEOFFS: SYSTEMATIC PLANNING FOR GREATER SAGE-GROUSE IN THEIR EASTERN RANGE

Authors: Kevin E. Doherty, David E. Naugle, Holly Copeland, Amy Pocewicz, and Joseph Kiesecker

Abstract from Doherty et al.
" We developed a framework for conservation planning to evaluate options for reducing development impacts on Greater Sage-Grouse (Centrocercus urophasianus) in Wyoming, Montana, Colorado, Utah, and North and South Dakota that contained some of the largest populations and highest risk of energy development. We used lek-count data (N = 2,336 leks) to delineate high abundance population centers which we termed core regions, that contained 25, 50, 75, and 100% of the known breeding population. We assessed vulnerability of these areas by examining risk of future land transforming uses from energy development. Sagegrouse abundance varies by state, core regions contain a disproportionately large segment of the breeding population, and cores regions vary dramatically by risk of future energy development. Wyoming contains 64% of the known sage-grouse population and more active leks than all the other states combined within our study area. Conservation success in Wyoming will depend on leasing and permitting policy decisions because this state has the highest risk of development. Montana contains fewer sage-grouse (24%) than Wyoming, but actions that that reduce sagebrush (Artemisia spp.) tillage by providing private landowners incentives to maintain sagebrush-dominated landscapes would provide lasting benefits because core regions in Montana are at comparatively low development risk. Habitat restoration in areas with low risk of development but containing fewer sage-grouse fit into the overall conservation strategy by targeting populations that promote connectivity of core regions. This vulnerability assessment illustrates the tradeoffs between conservation and energy development, and provides a framework for maintaining populations across the species' eastern range."

Review of:
ENERGY DEVELOPMENT AND CONSERVATION TRADEOFFS: SYSTEMATIC PLANNING FOR GREATER SAGE-GROUSE IN THEIR EASTERN RANGE

Review by: Dr. Rob Roy Ramey II

22.1) Doherty et al. combined data on projected oil and gas development with potential wind development to produce a new category, "energy development," for sage grouse vulnerability assessment and conservation planning. While projected oil and gas development were based on *actual* well data or lease sales that overlap sage grouse core areas, wind development was based on undeveloped and unleased commercial wind *potential*. While the basic approach of mapping areas of key conservation importance and development to avoid conflict was first introduced in the 1960's by McHarg (1969) and widely applied since, there are serious issues with this methodological approach presented here.

> **First, by combining two very different types of data into a single category of "energy development", the authors confound the effects of the two different types of development on sage grouse. This ignores differences in their level and type(s) of impact, and negates the authors' intention of using this information for planning purposes.**
>
> **Second, the effect of combining two different types of data, one that is based on current and known future development (oil and gas well and leases) with speculative information based only on wind potential, introduces unnecessary and unmeasurable error into their analysis. Thus, their results, based in large measure on speculation, are unreliable, especially as a basis for informed policy decisions.**
>
> **Third, the 1km^2 grid size used is very crude and over estimates the scale of impacts. Data at much finer resolution are readily available and are the industry standard for habitat analyses.**

22.2) An unbiased approach would involve analysis the two types of development separately, then overlay their projected impacts to sage grouse in a common unit that reflects each development's impact(s) to sage grouse. If the common unit was expressed in terms of habitat quality or probability of sage grouse use, then mapping would show degree of effect: from 100% habitat loss, to zones where avoidance is expected, and where restoration efforts have *increased* useable habitat. Instead, the authors equate "risk" with permanent loss of areas surrounding potential development.

22.3) The authors present an approach that assumes all impacts are created equal, regardless of whether they are from oil and gas, or wind development. This one-size-

fits-all approach grossly overestimates loss of sage grouse within areas of potential development. Furthermore, while some impacts from oil and gas to sage grouse have been quantified, there are no studies documenting impact of wind energy development to sage grouse.

22.4) The authors ranked all 1km grid cells within 6.4km of leks to delineate their importance without consideration of whether the entire area was of equal importance. These delineations were made without regard for the fact that the errors (standard deviations) associated with estimates of average distance to nearest lek and the lek counts were nearly as large as the estimates themselves (Table 1). (Under this approach, the area around each lek encompasses approximately156km^2 (depending upon where a lek was in a cell) because all cells within 6.4km of a lek receive the same ranking. By comparison, the area of a circle drawn around a lek would be128.7km^2 or ~28km^2 less). The approach used in this paper has the effect of overestimating impacts in some areas while underestimating impacts in other areas.

22.5) A measured approach that makes use of best available information would incorporate current development, physiographic features, and vegetation into an analysis to more accurately portray landscape potential for sage grouse. Failure to incorporate readily available information on essential sage grouse habitat elements as well as areas that are permanently unavailable, limits its accuracy and utility of this "risk assessment" and its use for policy decisions.

22.6) A 1km^2 grid size for land use does not constitute best available information. A 1km^2 grid is a crude spatial resolution for planning assessments and overestimates areas of potential conflict, a shortcoming not acknowledged by the authors. A 1km^2 resolution does not constitute best available information and is over 33 times larger than the 30m grid size used by the 2001 National Land Cover Data (NLCD 2001, http://www.epa.gov/mrlc/nlcd-2001.html). The 30m grid size has long been in use to develop qualitative models for endangered species critical habitat because it is the resolution of many digital elevation models (e.g. Turner et al. 2004). Some conservation GAP analyses use data with a resolution of 10m. While data resolution may limit analyses in some regions, a more focused evaluation of sage grouse core areas that utilizes a more informative grid size (e.g. industry standards of 90, 30, or 10 m) would be a more appropriate basis for policy decisions and conservation measures than that offered in this paper.

22.7) There is a need for a measured approach to mapping the different types of energy development along with sage grouse, a key issue in sage-grouse conservation. However, the author's implicit assumption that oil and gas development always results in sage grouse population declines appears to be based on an evaluation of the effects of past practices and does not reflect current realities (e.g. new BLM requirements, a slow down of leasing) or future technologies (e.g. lower impact extraction methods).

22.8) As with other papers in this monograph, the authors did not consider hunting

to be a factor controlling sage grouse populations.

22.9) The discussion section of this paper includes an extensive set of policy recommendations for decision makers that are based less on the results of the study than on a political point of view and self-importance. This detracts from the results of the study and gives the appearance of advocacy dressed up as science. Examples are presented below:

> *"The unprecedented leasing of the public mineral estate dictate the need for a shift from piecemeal to landscape-scale conservation."*
>
> *"Successful planning must embrace the social and political realities of the region. Our analysis is both sufficiently broad in scale to allow a relevant examination of the necessary tradeoffs and, by assessing the potential impacts of energy development, we bring recognition of the political reality of energy development in the West."*
>
> *"Analyses reported here provide a framework for planning across political boundaries and a currency for measuring the success of its implementation."*
>
> *"Our analyses will enable policy makers to consider a portfolio of set-aside areas, priority conservation areas, lease consolidations, and more stringent spatially-based best management practices as creative solutions to balance energy development with sage-grouse conservation."*

22.10) Audubon's retiring CEO recently declared this pre-publication paper as the basis of new policy and successfully headed off an ESA listing. The lead author of this article is employed by Audubon.
http://www.audubonmagazine.org/audubonview/audubonview1001.html

22.11) The term "mitigation" does not appear anywhere in this paper although it would seem to be of some importance to sage grouse conservation.

Review of:
ENERGY DEVELOPMENT AND CONSERVATION TRADEOFFS: SYSTEMATIC PLANNING FOR GREATER SAGE-GROUSE IN THEIR EASTERN RANGE

Review by: Dr. Matthew A. Cronin

In this chapter the potential for energy (oil, gas, and wind power) development to affect sage grouse is assessed. Relationships of sage grouse and energy development were assessed by mapping sage grouse lek distributions and potential energy development across the eastern part of the sage grouse range (Wyoming, Montana, Colorado, Utah, North Dakota, South Dakota, and Idaho). The goal was to address three questions:

1. Where are landscapes with the highest biological value for sage grouse?
2. How do these landscapes differ with respect to risk from future energy development?
3. How does variation and juxtaposition in risk and biological values of areas affect the potential to develop a successful conservation strategy for sage grouse?

The types of energy development considered included oil, gas, and wind power. Areas with sage grouse populations (some were designated "core areas") and areas of potential energy development were mapped, and risk of impacts assumed where there was overlap. These analyses addressed the first two questions, although it was not a thorough analysis and at such a large scale it was of limited utility. The third question was not clearly addressed other than the apparent assumption that development would lead to loss of populations and the need to plan energy development considering this.

The results were described by state. Wyoming has the most sage grouse and most energy development, followed by Montana, and Colorado. It was stated that "Risk of energy development to sage grouse core regions" increase with biological value across the entire species' eastern range. This is especially the case for oil and gas, and less for wind power. Oil and gas pose the greatest risk in Colorado and Utah, and wind power poses the greatest risk in Montana and the Dakotas. Both oil and gas and wind power pose the highest risk in Wyoming.

22.12) Three strategies are suggested to ensure persistence of sage grouse.

1. Policy changes in areas of high biological value and high risk of energy development to manage leasing and permitting of oil and gas development on federal lands and to proactively site wind developments.
2. Rapid implementation of conservation to enhance populations in high value biological areas that don't have energy potential.
3. Restoration of fringe habitats and low density areas with limited risk from energy to promote connectivity.

The second and third strategies make sense and are general, but self evident. Regarding the first strategy (high value areas with high risk of development), several ideas are presented. It is claimed that:

"The future of sage grouse conservation is in question in the eastern range in part because 44% of the lands that the federal government has authority to control for oil and gas development (7 million of 16 million ha) has been authorized for exploration and development."

It's not clear how this questions the future of sage grouse (the proposed development is less than half of the range and it does not seem reasonable to assume that development will cause complete loss of local sage grouse populations where it occurs).

22.13) The authors also note that lease sales are continuing despite concerns (without citation) that no policy is in place for risk assessments at the scale at which impacts occur. However, they don't describe current policies or what scale they are talking about. Impacts can occur from the individual bird to subpopulations of a larger population.

22.14) Doherty et al. also say that the severity of impacts and unprecedented leasing dictate the need for a *"shift from piecemeal to landscape scale conservation"*. This is questionable and vague. All practical management will be at the local level, and can be applied over geography to achieve a large scale objective. Instead of shifting from local to landscape scales, they really want to add a large geographic scale to complement and integrate with local management. This is ok, but one scale won't replace the other.

22.15) With regard to wind energy, the authors suggest there is an urgent need for policies that promote landscape scale considerations. They note that much of the wind energy will be developed on private lands, particularly in Montana and the Dakotas. They suggest that private lands with "high value sage grouse habitat" could be considered for conservation easements to limit surface development. They note the high cost of easements and profitability of wind energy (without citations) require broader strategies to minimize wind development footprints. They do not describe what such strategies would be.

22.16) The authors note that 17% of the eastern sage grouse range has high biological value and low risk of energy development and these should be maintained, especially where they are next to areas of development. They say this is critical to ensure genetic connectivity and re-colonization after development is completed (apparently assuming development results in complete loss of sage grouse).

22.17) They note other potential "stressors" in these habitats such as tillage (farming), residential development, and invasive plants. They note the large amounts of private property in Montana and Utah as potential areas of ranching and rural residences (as with wind development) and suggest they are good places for incentives such as the Conservation Reserve Program (CRP). It is encouraging to see recognition of incentives

for private property owners as a good way to proceed. The same rationale should be extended to private energy lease holders on public lands.

22.18) Areas of low biological value and low energy potential (19% of the eastern range) are identified as important for connectivity of populations to core areas in Montana and fringe areas in the Dakotas, Montana, and Canada are in need of aggressive habitat restoration programs. They suggest restoring currently farmed lands to sagebrush dominated grasslands. Assuming farmed land is private, this is a questionable assumption without consideration of land owners' preferences and economic impacts from such changes.

22.19) The authors conclude by saying that *"Conservation concerns related to sage grouse will remain at the forefront until collaborative landscape planning and conservation are demonstrated."* This seems to ignore existing cooperative management groups (e.g. the Montana sage grouse work group).

22.20) They also state that their analyses provide a framework for planning and a currency for measuring success. It's not clear what their "currency" is, other than identifying good habitat and trying to protect it.

22.21) The basic premise of this chapter is that management planning is needed across Wyoming, Montana, Colorado, Utah, North Dakota, and South Dakota. It's not clear that it is needed, but seems to be the authors' personal preference. I think there's an unwritten assumption that the species is endangered with extinction (it is being considered for Endangered Species Act listing) so planning at the species level is needed. This may be so (although with more than 50% of the native habitat intact it seems unlikely), but it is not adequately justified or documented.

22.22) The title and text suggest the chapter is going to assess "tradeoffs" between conservation and energy development. However, there is no real description of tradeoffs. To deal with this issue one would assess economics, property rights, employment, and the states' and country's energy needs in light of managing sage grouse. The chapter simply claims unavoidable impacts of energy development and that a "landscape" approach is needed. An effective assessment of tradeoffs would include co-authorship by representatives of the energy industries and landowners.

22.23) The chapter does not acknowledge that mitigation and local management is the key to managing sage grouse and energy development. Instead it calls for landscape policy for sage grouse, coupled with the tacit assumption of loss of populations with development. In my opinion, this probably indicates the authors' (unstated) goals of excluding development from large areas.

22.24) The paper also doesn't acknowledge that the States have management authority for wildlife. The call for regional "landscape" policies need to be tempered with this foundational Constitutional tenet.

22.25) With regard to the mapping analysis of the chapter, it's not clear how much of the sage grouse habitat is likely to be near development. The maps are very large scale, covering several states, with dots representing gas wells. Dots are not to scale so it's not clear what the actual distribution of wells and sage grouse habitat looks like. A detailed atlas of sage grouse populations and oil and gas development is needed to properly assess these issues.

22.26) A very important issue is the current state of sage grouse populations and energy development. The authors of this chapter made no effort to determine if current development coincided with populations and if they were compatible. The issue of hunting is also important in this regard. It is possible that un-hunted sage grouse populations would habituate to human activity and noise better than hunted populations.

22.27) The use of the term "conservation" in this chapter and others indicates the mentality that the sage grouse needs to be conserved. It actually needs to be managed, and in my opinion "management" should replace the term "conservation".

22.28) This chapter lacks acknowledgment that sage grouse and energy development could be compatible with proper mitigation and restoration. Connelly et al. (2000) note that mining and oil and gas development can have negative impacts on sage grouse but that populations can recover after the development ceased. This critical point suggests that the authors are calling for only half of the proper management equation. Doherty et al. are calling for *spatial* "landscape management". Both *temporal* and *spatial* management are needed. Development with subsequent restoration of areas with oil and gas resources can occur over time to maintain populations over the range of the species. Coupled with development of effective mitigation to minimize impacts close to development, this approach would allow achieving multiple objectives without excluding development from large areas.

Chapter 23 (2009), Chapter 22 (2011):
RESPONSE OF GREATER SAGE-GROUSE TO THE CONSERVATION RESERVE PROGRAM IN WASHINGTON STATE

Authors: Michael A. Schroeder and W. Matthew Vander Haegen

Abstract from Schroeder and Vander-Haegen:
"We examined the relationship between the Conservation Reserve Program (CRP) lands and Greater Sage-Grouse (Centrocercus urophasianus) in Washington state including an assessment of population change, nest-site selection, and general habitat use. We monitored nest site selection of 89 female sage-grouse between 1992 and 1997 with the aid of radiotelemetry. The proportion of nests in CRP lands significantly increased from 31% in 1992–1994 to 50% in 1995–1997, although more nests were detected in shrubsteppe (59 vs. 41% of 202 nests). The increase appeared to be associated with maturation of CRP fields, which were characterized by increased cover of perennial grass and big sagebrush (Artemisia tridentata). Nest success was similar (P = 0.38) for nests placed in the two cover types (45% in CRP and 39% in shrubsteppe). Counts of fecal pellets indicated that sage-grouse selected areas with greater sagebrush cover, especially in relatively new CRP in a shrubsteppe landscape. Analysis of male lek attendance, prior to implementation of CRP (1970-1988) illustrated similar rates of declines in two separate populations of sage-grouse in north-central and south-central Washington. Data from 1992–2007 following establishment of the CRP revealed a slight reversal of the population decline in northcentral Washington while the south-central population continued a long-term decline (~ 17% vs. 2% of the occupied areas were in the CRP, respectively). These results indicate that lands enrolled in the CRP can have a positive impact on Greater Sage-Grouse, especially if they include big sagebrush and are focused in landscapes with substantial extant shrubsteppe. The CRP for sage-grouse and other sage-dependent species should be considered a long-term investment because of the time required for sagebrush plants to develop."

Review of:
RESPONSE OF GREATER SAGE-GROUSE TO THE CONSERVATION RESERVE PROGRAM IN WASHINGTON STATE

Review by: Dr. Rob Roy Ramey II

This paper analyzes sage grouse populations prior to converting cropland to sagebrush, and after conversion of cropland to sagebrush, through the Conservation Reserve Program in Washington State. There was not an obvious direct increase in grouse populations following conversion - one population increased 19% while the other population decreased 56% - however the gist is that if cropland near existing sage steppe is converted back to sagebrush, sage grouse will nest there. Former cropland that is not near existing sage steppe may not attract sage grouse because there is not enough existing natural habitat nearby.

Logistic regression was used to quantify differences in occupied vs. unoccupied land used by sage grouse in the Conservation Reserve Program. The maturation of shrubs on CRP lands was important factor in sage grouse utilization, which indicates the need for a long-tern investment in this conservation strategy.

Chapter 24 (2009), Chapter 23 (2011):
RESTORING AND REHABILITATING SAGEBRUSH HABITATS

Author: David A. Pyke

Abstract by Pyke:
"Less than half of the original habitat of the Greater Sage-Grouse (Centrocercus urophasianus) currently exists. Some has been permanently lost to farms and urban areas, but the remaining varies in condition from high quality to no longer adequate. Restoration of sagebrush (Artemisia spp.) grassland ecosystems may be possible for resilient lands. However, Greater Sage-Grouse require a wide variety of habitats over large areas to complete their life cycle. Effective restoration will require a regional approach for prioritizing and identifying appropriate options across the landscape. A landscape triage method is recommended for prioritizing lands for restoration. Spatial models can indicate where to protect and connect intact quality habitat with other similar habitat via restoration. The ecological site concept of land classification is recommended for characterizing potential habitat across the region along with their accompanying state and transition models of plant community dynamics. These models assist in identifying if passive, management-based or active, vegetation manipulation-based restoration might accomplish the goals of improved Greater Sage-Grouse habitat. A series of guidelines help formulate questions that managers might consider when developing restoration plans: (1) site prioritization through a landscape triage, (2) soil verification and the implications of soil features on plant establishment success, (3) a comparison of the existing plant community to the potential for the site using ecological site descriptions, (4) a determination of the current successional status of the site using state and transition models to aid in predicting if passive or active restoration is necessary, and (5) implementation of a post-treatment monitoring to evaluate restoration effectiveness and post-treatment management implications to restoration success."

Review by: Dr. Rob Roy Ramey II

This paper provides a useful review of information on sagebrush restoration and is a practical "how to" guide for restoration strategies. It is the most immediately useful paper in the monograph to sage grouse conservation and mitigation.

Chapter 25 (2009), Chapter 24 (2011):
CONSERVATION OF GREATER SAGE-GROUSE: A SYNTHESIS OF CURRENT TRENDS AND FUTURE MANAGEMENT

Authors: J. W. Connelly, S. T. Knick, C. E. Braun, W. L. Baker, E. A. Beever, T. Christiansen, K. E. Doherty, E. O. Garton, C. A. Hagen, S. E. Hanser, D. H. Johnson, M. Leu, R. F. Miller, D. E. Naugle, S. J. Oyler-McCance, D. A. Pyke, K. P. Reese, M. A. Schroeder, S. J. Stiver, B. L. Walker, and M. J. Wisdom

Abstract from Connelly et al.:
"Recent analyses of Greater Sage-Grouse (Centrocercus urophasianus) populations indicate substantial declines in many areas but with relatively stable populations in other portions of the species' range. Sagebrush (Artemisia spp.) habitats necessary to support sage-grouse are being burned by large wildfires, invaded by nonnative plants, and developed for energy resources (gas, oil, and wind). Management on public lands, which contain 70% of sagebrush habitats, has changed over the last 30 yr from large sagebrush control projects directed at enhancing livestock grazing to a greater emphasis on projects that often attempt to improve or restore ecological integrity. Nevertheless, the mandate to manage public lands to provide traditional consumptive uses as well as recreation and wilderness values is not likely to change in the near future. Consequently, demand and use of resources contained in sagebrush landscapes plus the associated infrastructure to support increasing human populations in the western United States will continue to challenge efforts to conserve Greater Sage-Grouse. The continued widespread distribution of sage-grouse, albeit at very low densities in some areas, coupled with large areas of important sagebrush habitat that are relatively unaffected by the human footprint, suggest that Greater Sage-Grouse populations may be able to persist into the future. We summarize the status of sage-grouse populations and habitats, provide a synthesis of major threats and challenges to conservation of sage-grouse, and suggest a roadmap to attaining conservation goals."

Review of:
CONSERVATION OF GREATER SAGE-GROUSE: A SYNTHESIS OF CURRENT TRENDS AND FUTURE MANAGEMENT

Review by: Dr. Rob Roy Ramey II

This chapter provides a convenient summary of the results, discussion, and recommendations of previous chapters, as well as other cited studies. As a result, it is one of the most important chapters to read in its entirety. The authors also lay out a *"Roadmap To Conservation"* that is likely to become the basis of a recovery plan, critical habitat designation, and biological opinions.

Fifteen major threats to sage grouse identified by the authors and other studies are identified in Table 1. There are obvious differences in opinion regarding the primary threats, although energy development, drought, and wildfire were most frequently cited. The problem with the categories used is that they are too coarse and combine effects from multiple factors (i.e. energy includes oil and gas as well as wind; urbanization includes roads, powerlines, and traditional development).

25.1) The authors make a number of strong statements that are clearly aimed at influencing policy:

"Severity of impacts (Holloran 2005, Aldridge and Boyce 2007, Walker et al. 2007) and extensive leasing of the public mineral estate suggest a need for landscape-scale conservation. Lease sales continue despite concerns because no policy is in place that would permit an environmental assessment of risk at the scale at which impacts occur."

"Aggressive habitat protection and restoration programs may be necessary to maintain the biological integrity of fringe populations in North Dakota, South Dakota, northern Montana, and Canada."

The following suggestion appears to make sense until one realizes that *"immediately implemented"* can mean severe restrictions on other productive land uses (e.g. agriculture) *"Areas of high biological value combined with low energy potential represent regions where conservation actions can be immediately implemented (Doherty et al., this volume)."*

25.2) Lacking from this and other chapters in this monograph is a comprehensive treatment of how individual states or the private sector have contributed to sage grouse conservation. The only mention is the study of sage grouse response to the Conservation Reserve Program in Washington State in Chapter 23.

25.3) The authors believe that direct predation management is ineffective and recommend that habitat manipulations be used instead. Additional research on predator dynamics is also proposed. There is no discussion of research into how to

make predator management (lethal and non-lethal) *more effective* in populations of sage grouse that are in decline or at risk of extirpation. For example, testing and refinement of mitigation measures designed to actually reduce predation (e.g. installing anti-perch devices on towers, power poles, and fence posts) would be relatively straightforward and easy to achieve as compared to large-scale habitat manipulations.

The authors' stated views on predator management:

> *"Because of these considerations, predator management for sage-grouse has generally been accomplished most efficiently by manipulating habitat rather than by predator removal to enhance populations (Schroeder and Baydack 2001). For future sage grouse conservation efforts we recommend quantifying predator communities as they relate to demographic rates and habitat variables so the predator-cover complex as it pertains to sage-grouse life history can be better understood on how species that prey on sage-grouse respond to anthropogenic changes."*

25.4) The authors propose using a "before-after control-impact design" for proposed projects in sage grouse range. This will spawn a cash cow for researchers and consultants.

> *"For proposed projects that occupy spatially discrete as opposed to dispersed areas, a before-after control-impact design (BACI) may provide the most powerful statistical approach. To assess population effects, we recommend that BACI include marking sage grouse at each impact and control site(s). Required sample sizes of marked birds will vary depending on size and extent of the grouse population being considered questions being asked, and the marking technology employed. We recommend capturing and marking birds in a manner that allows sampling of the entire project area, focusing on leks most proximate to the proposed impact site(s). We also recommend marking additional female grouse in an 18-km buffer zone to characterize the migratory status of the population, but this sample will not allow evaluation of avoidance behavior. Because of the effect of lag periods on population response, at least 3 yr pre- and 4 yr postconstruction may be required at a minimum, as well as the year of construction to fully assess project effects on grouse populations. Given the lifespan of sage-grouse, strong fidelity to breeding areas, and lag-effects in population dynamics some longer term (8–12 yr) less intensive monitoring will be necessary to fully assess impacts."*

It is important to realize the costs associated with such a monitoring effort. The cost of such a monitoring study for a monitoring project could easily exceed $150,000 per year, per project. This makes the proposed monitoring a cash cow for researchers and consultants (especially those who have contributed to this monograph) with little in return for sage grouse conservation or the project proponent.

25.5) The authors explain away hunting as a cause for concern and do not suggest any additional study, regardless of the fact that the level of hunting in 2007 removed approximately 9% of the adult population, and this level of harvest occurs annually:

> *"Hunting has also been identified as a management concern for sage-grouse populations (Connelly et al. 2003; Reese and Connelly, this volume). Nine of 11 states with sage grouse presently have hunting seasons for this species. Sage grouse normally experience high survival over winter (Wik 2002, Hausleitner 2003, Beck et al. 2006, Battazo 2007), thus mortality from hunter harvest in September and October may not be totally compensatory. Nevertheless, harvest mortality is low on most populations of sage grouse, and no studies have demonstrated that hunting is a primary cause reducing populations (Reese and Connelly, this volume)."*

As noted in the review of Chapter 16:
Garton et al. estimated 88,816* male grouse in 2007 or a total population size of 310,856 (using their assumption of 2.5 adult females per male to obtain total population). That was the count in the spring while leks were active. However, in the fall of 2007, a total of 28,180 sage grouse were harvested, or 9 percent of the estimated population number of this species. And in four of the six pervious years, the take was even higher (up to 37,607 in 2006). To date, there has not been a field-verified study of maximum sustainable yield applied to this species and this intensity of harvest. The state of the science does not provide an empirical basis that is solid enough to forecast the future of sage grouse with any degree of accuracy, especially when known sources of mortality are not included.

25.6) The authors' proposed monitoring and mitigation strategy does not explicitly provide for thresholds to be set in advance, and therefore cannot provide an objective assessment of results. Simply put, the approach advocated by the authors leaves results open to subjective interpretation and bias. The authors suggest here, and elsewhere in this monograph, that monitoring and planning be "*carefully*" conducted and implemented. However, the authors never describe what *"careful"* means in this context. Nor do they describe how monitoring or management prescriptions would be deemed successful or not:

> *"Well planned and carefully implemented monitoring and assessment will allow an objective evaluation of conservation measures over varying temporal and spatial frames. It will also provide an unbiased assessment of impacts that can be used to guide appropriate mitigation efforts."*

> *"Energy development and other anthropogenic change represent substantial challenges to protecting existing habitat, and will require development and implementation of broad-scale and long-term conservation plans (Stiver et al. 2006; Stiver, this volume) that are carefully developed using the best available data."*

Similarly, the authors recommend, *"statistically sound sampling designs"* but say nothing about how those will be used in any sort of objective problem analysis or hypothesis testing framework. Because other papers in this monograph had relied on interpretation of results and inductive reasoning, it is reasonable to conclude that the same can be

expected from these authors in the future. The absence of hypothesis testing in this chapter, as well as other chapters in this monograph, indicates a lack of basic epistemology.

25.7) In contrast to the approach recommended by the authors of this monograph, the most effective means to ensure an unbiased analysis of results is to utilize an adaptive management approach. A scientific approach to adaptive management requires that threats and management actions be treated as potentially falsifiable hypotheses (Popper 1963), rather than certain knowledge. If the presumed threats to a population are ranked in order of importance (based on plausible cause and effect mechanisms, or available data on mortalities and recruitment), then even hypothetical threats can be prioritized and subsequently investigated in a scientific manner. Priorities may be revised as some hypotheses are rejected when new information becomes available.
Alternative management actions that have been designed to address a specific threat may be treated as alternative hypotheses and their effectiveness tested against quantitative thresholds. These can be laid out in a series of "if - then" statements in the adaptive management plan. This same strategy can be used to set "triggers" for additional or alternative management actions.

In all cases, if the thresholds for rejecting hypotheses or triggering management actions are set in advance of data collection, then an objective and scientifically defensible evaluation of the evidence is possible. Such a scientific approach to adaptive management increases the likelihood that the allocation of conservation effort will go towards providing the greatest benefit; in this case, contributing to the recovery of the sage grouse populations at multiple locations. A list of adaptive management actions, their priority, and the value to the sage grouse populations could be quickly developed based on available information.

In order to prioritize Adaptive Management actions and the areas where they will be conducted, the evaluation of sage grouse habitat is needed. While the studies presented in this monograph could be used, they could also be substantially improved upon to develop a composite ranking of sage grouse habitat, including identification of areas of non-habitat. It could be used to identify specific areas where habitat enhancements could have the greatest benefit to sage grouse. The habitat evaluation would also identify areas where physical and/or biological constraints impose limitations on the potential for sage grouse occupancy.

Other key components of an adaptive management approach include expected outcomes from implementation, measures of success, and an operational management plan.
It is more than an oversight that adaptive management is not mentioned anywhere in the 25 chapters of the sage grouse monograph.

Review of:
CONSERVATION OF GREATER SAGE-GROUSE: A SYNTHESIS OF CURRENT TRENDS AND FUTURE MANAGEMENT

Review by: (This reviewer wishes to remain anonymous.)

25.8) This chapter summarizes data indicating a continuing decline in the sage grouse over much of its range. It also lists a great variety of known and suspected threats causing this decline. However, the analyses of which threats are most important in the sage grouse declines are mostly unquantitative, and leave something to be desired. In effect the authors attempt to come to conclusions about the relative importance of various threats by taking a vote of the opinions of people who have been on various panels (dominated by themselves) who have been asked to evaluate the decline of the species. This may be good politics, but it is not necessarily good science. There appears to be a general absence of data allowing rigorous evaluation of the relative importance of various threats for any particular area. Especially questionable is the fact that the authors present data on a number of panels of opinions (Table 1) as if they might be independent judgements, when in fact the authors themselves were involved in the operation of all four panels, with the senior author a conspicuous co-author on three of the panels. Membership on three additional panels (Table 2) is not specified, but one wonders whether the authors of this chapter may have been involved with these panels as well. My own experience with bureaucratic panels on other species is that they are often misguided concerning the reality of limiting factors, depending on the field experience and biases of personnel involved and the quality of data available on the species in question.

25.9) The authors seem to have accepted an underlying assumption that the primary stresses on the species must be habitat related, hence the emphasis on maintenance and improvement of sagebrush habitats. However, while correlation of population declines with habitat degradation may well exist, this does not prove causality between these factors as many other stresses tend to go along with habitat degradation and may be more important, but relatively unacknowleged, causes of difficulty. Rigorous experimental tests of the importance of various factors appear to be largely missing.

25.10) I am especially puzzled by the way the authors treat the potential stresses of hunting and grazing – two threats that might reasonably be very important but often pose political problems in correcting. With both of these threats, the authors claim that data do not exist to show the clear detrimental effects of these practices. Yet with respect to hunting, they acknowledge that where populations are continuing to decline, there could be a need to adjust levels of hunting, implying a recognition of detrimental effects. They do not say there could be a need to cease hunting of such populations. However, a failure to cease hunting of declining populations places the burden of reversing declines on improving other potential stress factors which may be of lesser importance and may be much more difficult to identify and correct.

I would argue that for continuously declining populations there is no persuasive justification for hunting activities, as any additional mortality over natural mortality should be conservatively assumed to exacerbate difficulties. Hunting activities should cease in order to determine whether elimination of such pressure might be enough to reverse the declines. If a cessation of hunting results in populations increasing, then limited low levels of hunting could be reinstituted, depending on how close populations might be to a healthy level. If not, hunting should remain in abeyance until population recoveries are achieved by whatever means may be appropriate.

25.11) It is also surprising that, with the exception of one study on the impacts of horses on sage grouse habitat, no studies are presented that compare various sage grouse populations under grazing pressure from domestic livestock with those that are not. It would be especially interesting to compare sage grouse populations that are associated with native herbivores, such as bison and pronghorn, with those associated with cattle or sheep. No discussion of such comparisons or the values of such comparisons is presented, yet such studies could have profound importance in achieving optimal management of the species.

25.12) The authors' use of the term “carrying capacity” seems unclear on page 15 of the paper. Quite reasonably, many sage grouse populations may be far below carrying capacity because of stress factors unrelated to resources such as food, cover, nest sites, etc. How carrying capacity is defined and established is not explained in this summary although it may be clearer in Garton et al., which I have not seen. Carrying capacity is usually very difficult to pin down in any rigorous way, because of a lack of critical information on limiting factors.

25.13) As a general review, this summary does not take us much beyond a catalogue of known and potential threats to sage grouse populations, and a presentation of a number of actions that might help declining populations to some largely unspecified extent. There is no urgent call for the sort of research that could establish the true impacts of stresses such as grazing, or studies that could establish clearly whether the species is a true specialist on sagebrush incapable of persisting in any other habitat types. The distribution of early records of the species in many regions east of the sagebrush dominated zone in early times (see Schroeder et al. paper) leaves this question unresolved.

Mapping Oil and Gas Development Potential in the US Intermountain West and Estimating Impacts to Species

Citation:
Copeland, H.E., K.E. Doherty, D.E. Naugle, A. Pocewicz, J.M. Kiesecker (2009) Mapping Oil and Gas Development Potential in the US Intermountain West and Estimating Impacts to Species. *PLoS ONE* 4(10).

Abstract from Copeland et al.:
"Background: Many studies have quantified the indirect effect of hydrocarbon-based economies on climate change and biodiversity, concluding that a significant proportion of species will be threatened with extinction. However, few studies have measured the direct effect of new energy production infrastructure on species persistence."

"Methodology/Principal Findings: We propose a systematic way to forecast patterns of future energy development and calculate impacts to species using spatially-explicit predictive modeling techniques to estimate oil and gas potential and create development build-out scenarios by seeding the landscape with oil and gas wells based on underlying potential. We illustrate our approach for the greater sage-grouse (Centrocercus urophasianus) in the western US and translate the buildout scenarios into estimated impacts on sage-grouse. We project that future oil and gas development will cause a 7–19 percent decline from 2007 sage-grouse lek population counts and impact 3.7 million ha of sagebrush shrublands and 1.1 million ha of grasslands in the study area."

"Conclusions/Significance: Maps of where oil and gas development is anticipated in the US Intermountain West can be used by decision-makers intent on minimizing impacts to sage-grouse. This analysis also provides a general framework for using predictive models and build-out scenarios to anticipate impacts to species. These predictive models and build-out scenarios allow tradeoffs to be considered between species conservation and energy development prior to implementation."

Review of Copeland et al. (2009):
Mapping Oil and Gas Development Potential in the US Intermountain West and Estimating Impacts to Species

Review by: Dr. Rob Roy Ramey II

This paper used predictive modeling to map potential impact of oil and gas development on existing sage grouse core areas. When magnified, a perusal of Figure 1 suggests that there is far less certainty with its predictive powers that the authors acknowledge. The reasons are identified below.

Methods

C09.1) "Random forests" is a classification method that uses decision tree algorithms for sorting observations into categories, as well as predictive modeling and data mining. Widespread application of this approach is relatively new, although some of the algorithms have been around for decades. It differs from classic vector-based statistical methods such as discriminant analysis and (nonparametric) multivariate adaptive regression splines, and reportedly performs better in many cases. The "random forests" method has seen use in medical and ecological fields although several authors have recently pointed out limitations. The most serious involve cases when the predictor variables differ widely in scale or classification, and when there are an excessive number of variables being used (Strobl et al. 2007; Siroky 2009). As a relatively new technique, the potential and limitations of "random forests" are still being explored, as well as its implementation by various software packages. The authors do not present any examples of "random forests" having been previously applied to predicting subsurface geology or oil and gas deposits. It is a method who's performance is unknown and untested for this application.

C09.2) The authors claim that: *"We measured the impacts of the build-out scenarios on populations of greater sage-grouse (Centrocercus urophasianus)"* however, what they actually did was estimate *potential* impact to sage grouse habitat.

C09.3) The authors make the following assumption without supporting data: *"Where reasonable foreseeable development projections were unavailable, we calculated resource area estimates by doubling the number of wells permitted from 1996–2007 within a resource area."*

C09.4) It is questionable whether the analytical method of *"seeding the landscape with oil and gas wells according to the underlying development potential"* is an accurate approximation of how oil and gas fields have been developed. No supporting information are provided by the authors. Under a random placement scenario, estimated impacts to the landscape using this method would be greater because development would not build on existing infrastructure (e.g. roads) and leasing tracts, resulting in a higher level of habitat fragmentation.

C09.5) While the analysis methods appear novel, there is a lack of detail in describing how the parameters for each variable were quantified or categorized (e.g. the variable "geology"). It is also unexplained what amount of error was introduced into the analysis by using 1:5,000,000 scale bedrock geology maps to define the author's 1km^2 cell surface model. Application of such a crude map to such a small pixel size could be expected to result in overestimates of oil and gas potential, and therefore impacts.

C09.6) The authors predict *"a 7 percent population decline in the anticipated scenario and 19 percent population decline in the unrestrained scenario compared to 2007 lek population counts."* It is important to remember that this is a hypothetical build-out scenario based on the "random forests" method to predict underlying oil and gas potential (rather than actual oil and gas deposits). The model could be improved is actual data on oil and gas deposits were incorporated. This may be difficult because of the propriety nature of some data on oil and gas deposits.

It is not stated how the authors arrived at the population decline numbers or what the range of uncertainty is with these estimates. Readers are referred to Tables 1 and 2 in Doherty's unpublished dissertation (Doherty 2008). Greater detail on how these population level impacts were calculated is needed, as well as a review of Doherty's (2008) methods.

Doherty (2008) reported, *"Potential impacts were indiscernible at 1-12 wells within 32.2 km2 of a lek (~1 well / 1 mi2), a threshold of development compatible with conservation. Above this threshold land managers can expect to see rate of lek inactivity double at 13-39 wells and jump to > 5 times (40-100 wells) that outside of widespread development in northeast Wyoming."* Clearly, intense development has a measurable impact on sage grouse but the question remains: *Do the yearling grouse that are displaced from developed areas move into unaffected habitat and successfully reproduce?* If so, then the population-level impact would be less than predicted by Doherty (2008). If not and if grouse from unaffected areas disperse into developed areas, the population impact could be greater (a population sink).

Another unanswered question is the extent to which on-site or off site mitigation could benefit sage grouse populations. For example, Doherty (2008) made the following suggestion: *"Post-hoc analyses of 17 leks showed that clustering wells to provide open areas for nesting may increase opportunities for restoration by keeping a few small but active leks inside intensely developed landscapes."* The effects of this and other mitigation needs to be accounted for in a measured analysis of predicted impacts.

C09.7) The authors assert: *"These declines* [of sage grouse] *are in addition to the estimated range-wide population declines of 45–80 percent that have already occurred."* While it is well known that sagebrush was persecuted by a wide variety of methods up until the mid-1980s , there has always been a great deal of uncertainty regarding population number and extent of decline. All of the papers dealing with that topic have relied on the same flawed methodology: male lek count data. Consequently, there is greater uncertainty with the estimates of decline than acknowledged by the authors. A

more comprehensive treatment of the limitations of this technique and issues surrounding data collection and analysis are included in the review of Chapter 16.

C09.8) Despite assurances that the methods and results are sound, patterns emerge that suggest otherwise. Most notably, the locations of oil and gas wells in Figure 1 do not correspond well with the predicted oil and gas potential.

A) The supposed accuracy of the model is misrepresented by the following statement of the authors because it only describes the percentage of producing wells within the total predicted oil and gas deposits, rather than the area of their predicted oil and gas deposits that has producing wells or leases. *"We found that 81 percent of wells producing during 1986–2007 were in areas the validation model predicted for development, which suggests that our model accurately predicts where new wells would be placed up to 20 years into the future (Fig. 1C)."* The problem with the prediction accuracy of the model can clearly be seen by zooming on in Figure 1. First, there are large areas that the authors' model classified as having medium to high potential for oil and gas but those areas do not have any wells currently. And second, there are numerous areas with dense collections of wells that appear to be classified as having low (zero) potential. A closer approximation would be about 50% accurate. (One would assume that the oil and gas companies would have already identified and placed wells on most areas of high potential.)

B) Figure 2 shows the overlap in the authors' predicted oil and gas reserves to sage grouse leks. The locations of existing wells are not included on this map. In order to quantify the correspondence between the model and the actual oil and gas resources that have been identified to date, it would have made sense to have included existing wells on the map in Figure 2.

C09.9) There are numerous simplifying assumptions used to develop the model and these are not specified.
The analysis presented in the paper falls short in that it does not fully explore the benefits of advancements in extraction technology. While the authors acknowledge that predictions *"could be inaccurate if there are significant new advancements in extraction technology,"* the authors make no allowances for it except to say: *"Forecasted impacts to sage-grouse populations could be revised lower if directional drilling to reduce well pad density at the surface became more commonplace."*

New technologies already include horizontal drilling and methods to increase production from existing wells. The constantly evolving nature of this technology can be expected to reduce the scale of landscape level impacts in the future. Therefore, both of the scenarios presented by the authors represent over estimates with respect to the density of wells and hence, the total area impacted. This oversight would be expected to have an effect on the market value of future proposed lease swaps and buy backs. It could also foreclose options for energy extraction.

C09.10) Recently released policies by the BLM on energy development in sage grouse core areas are intended to consolidate disturbances to sage grouse (BLM Memorandum No. WY-2010-012 and WY-2010-013 dated December 29, 2009). For example, one of these policies includes the proposal: *"to not exceed one energy production location and/or transmission structure per 640 acres* [one square mile]. *The one location and cumulative value of existing disturbances in the area will not exceed 5 percent of sagebrush habitat within those same 640 acres."* Such a spatial restriction would have the effect of reducing the overall acreage of potential impact, although the actual impact would depend upon how far potential disturbance to grouse extend beyond the developed area (due to avoidance, low survivorship, or increased predation). To further avoid disturbance, the BLM has also proposed seasonal restrictions intended to reduce impacts to sage grouse during the breeding season.

C09.11) The authors present the analysis with a build-out scenario of 20 years and an unrestrained buildout scenario. From a practical point of view, the latter scenario could take over one hundred years. A prolonged build-out might reduce the extent of impacts because some wells would have gone out of production in that time period and opening up habitat to sage grouse occupation. It is unrealistic to expect that a build-out scenario would be achieved in a short time frame such as that proposed by the authors.

C09.12) The analyses used in this paper implicitly assume a permanent adverse impact, as if each 1km block would be withdrawn from sage grouse range in perpetuity. Any realistic build out scenario must account for the fact that production wells are not permanent. They all have a finite life span determined by the hydrocarbon resources they tap. The mean longevity of a typical producing individual oil and gas well in this region is likely in the 30-year range.

C09.13) The authors propose that implementation of their approach is a cost effective alternative to an Endangered Species Act listing of the sage grouse. Based on my 29 years of experience working with the Endangered Species Act, that assessment is not disputable. However, the methods proposed and results obtained should only be considered preliminary. The assumptions and models used in this paper could be refined to provide a more reliable prediction and could them be of greater utility to inform decision makers.

C09.14) Competing interests were not acknowledged by the authors.
The paper provides a rationale for promoting land conservation lease swaps and buy backs to benefit sage grouse populations in areas of overlap with potential oil and gas development. Three of the five authors list affiliations with The Nature Conservancy (TNC), including the lead author. The hallmark conservation strategies of TNC are land purchases and land swaps, so this is a natural extension of those strategies, as applied to lands of conservation interest with oil and gas resources. As part of its conservation program, TNC has recently implemented *"Energy by Design"* which is promoted on its website.* The webpage features the TNC authors of this paper, their "*complex modeling program",* its application to mitigation of a large gas field, and subsequent purchase a 1,000+ acre conservation offset. While this effort is certainly laudable, the authors should

have declared a *competing interest* as per the journal's competing interest policy. The author's declaration of *no competing interest* appears to be in conflict with the following facts: 1) TNC partnered with the fastest growing energy company in the region to implement *Energy by Design*, 2) one of the TNC Board of Directors is also a member of an executive committee for the partnering energy company, 3) the TNC listed that energy company as a donor in 2009, 4) as shown on the TNC website, there are abundant financial opportunities (employment and grants) using this model for application to sage grouse, and 5) a $100,000 contract was recently awarded by the BLM to the Audubon (Doherty's affiliation) to do additional mapping of sage grouse and energy development. It is unknown whether partnerships with TNC provide a competitive advantage to companies seeking regulatory approvals.
*http://www.nature.org/wherewework/northamerica/states/wyoming/misc/art26566.html

C09.15) The authors promote application of their analytical methods to other conservation issues, however, this would be premature given that this method is simply an inadequately tested prototype. For reasons detailed previously, this approach would need substantially more refinement, quantification of classification error, and field validation prior to its intended application. For the same reasons, extension of this method to other resources, and especially to 'cumulative effects analysis', would not be prudent at this time, despite the author's enthusiastic promotion: *"the framework we present could be modified to consider not just one type of energy development, in this case oil and gas, but also wind, solar, coal, oil shale and uranium, along with other stressors such as residential development, invasive species, and pathogens. Because many of these stressors do not correlate spatially, this approach would account for cumulative impacts. Models and maps of multiple future threats are needed to fully quantify the future risk to biodiversity."*

Despite the methodological shortcomings of this paper, the U.S. Fish and Wildlife Service has gone on record stating that *"It's a good report, and the information in it will definitely be considered in any final decision we make on the bird's status."* Please visit following links for more information:
http://www.eenews.net/public/Landletter/2009/10/22/1
http://www.audubon.org/newswire/Newswire_V7N11.html

Review of Copeland et al. (2009):
Mapping Oil and Gas Development Potential in the US Intermountain West and Estimating Impacts to Species

Review by: Dr. Vernon C. Bleich

Predictive modeling techniques have been used for many years to project changes in landcover resulting from urban development, predict habitat suitability for particular species, or examine effects of habitat alteration on habitat quality for various species. Similar techniques have not yet been used to make inferences about the impacts of anticipated energy development on wildlife. In this article, the authors took the approach of estimating the affects of future energy production infrastructure on species persistence, with an emphasis on impacts to Greater Sage-Grouse (*Centrocercus urophasianus*). They attempted to model future oil and gas build-out scenarios at two levels: (1) that which is reasonably anticipated to occur and based on Bureau of Land Management (BLM) projections for the next 20 years, and (2) an unconstrained scenario, in which the highest quintile of oil and gas potential would be expected to occur, in part because past BLM projections have been conservative. As appropriate, the authors excluded from those two scenarios those areas from which oil and gas development currently is prohibited (e.g., national parks, wilderness areas).

The authors used Random Forests (a non-parametric method with which I am not familiar) to develop a model that was used to predict potential oil and gas resources, and that was based on a series of six largely geological parameters known to be associated with those resources. Accuracy of the overall model was approximately 83%; the authors further tested the model's ability to predict future development using data from 1900-1986, and then tested the resulting model with well data from 1986-2007, reporting that 81% of wells producing during 1986-2007 were in areas their validation model predicted for development. The authors then concluded that the model would be useful in predicting locations of new wells over the next 20 years, given current extraction technologies.

Anticipated impacts to Greater Sage-Grouse were based on losses in abundance and occurrence of populations only in Wyoming, but extrapolated to other areas inhabited by sage-grouse. Timing of responses of leks to development were based on data collected over 11 years and compared to control leks not exposed to oil or gas development. Using these data, the authors predicted a decline of 7% in 2007 lek population counts in the anticipated build-out scenario, and up to 19% with unrestrained build-out. These declines are in addition to declines that already have occurred. Habitat impacts will be incurred over 2.3 million ha in the anticipated buildout, and 5.5 million ha in the unrestrained buildout, the majority of which will be sagebrush habitat in both scenarios.

C09.16) In predicting anticipated declines in sage grouse, a period of only 11 years was used to examine responses in altered versus control locations. While it is possible that other impacts to populations (e.g., harvest, predation, and grazing rates) remained constant during that short period, the potential impacts of weather did not appear to be

accounted for (and that short period likely lacked the variance in weather conditions necessary to examine the role of that variable in population persistence). Additionally, the assumptions of consistency in harvest, predation, and grazing did not appear to be substantiated. Further, proximity of impacted areas to non-impacted areas inhabited by sage-grouse could have been a meaningful consideration in assessing impacts, but there was no way of knowing if that variable played a role in population persistence. Moreover, range-wide impacts were extrapolated across a huge geographic area from responses developed only in Wyoming. Despite these shortcomings, the authors have produced a potentially useful predictive model that has implications for decreasing the severity of future impacts to sage-grouse and other sagebrush obligates.

C09.17) Application of the model (which appears to perform well) could be useful in avoiding the listing of Greater Sage-Grouse as an endangered taxon by the U.S. Fish and Wildlife Service. For example, a substantial portion (~15%) of the area included in this modeling exercise has high potential for oil or gas development, but development rights have not yet been sold. BLM could withdraw those areas from sale, or establish special mitigation measures within such areas. Another potential solution would be to establish constraints on land uses within such areas. Additionally, the model could be useful in creating a system analogous to the Natural Community Conservation Program, in which project proponents agree to set aside development rights on certain properties in order to ensure future access to others. Options for lease holders to sell back development rights could also be pursued, but I suspect the likelihood of such occurring is slight; there will be a continuing, and intensifying demand for oil and gas extraction well into the future. Application of the model could be useful in encouraging the energy extraction industry to develop or apply more advanced extraction techniques that result in less surface disturbance in certain areas. Finally, the model might be used to determine which areas inhabited by sage-grouse are most apt to be impacted and, if appropriate, grouse in some geographic areas might be listed as threatened or endangered population segments, rather than listing the taxon as a whole.

C09.18) In summary, the technique proposed by Copeland et al. (2009) appears useful in predicting anticipated oil and gas development under two scenarios, but could be further improved if additional predictive variables are incorporated to assess impacts to sage-grouse. Weaknesses of assessing impacts to sage grouse include the application of localized responses (i.e., Wyoming only), the assumption that other factors (e.g., hunting, grazing, and predation rates are constant across the range of the species), and the apparent absence of the potential of climatological effects to impact localized grouse populations. Additionally, caution should be used when extrapolating results obtained from one area to a vastly broader area. Nevertheless, the potential for decreasing some impacts to sage-grouse (and other sagebrush obligates) is enhanced if agency personnel, oil and gas companies, and other involved parties make use of this information when planning or implementing resource extraction activities. Further, similar models would appear to have application to other types of energy development, including solar and wind, that have the potential for landscape-level habitat alteration, but are less far along in planning and development on the public lands than are oil and gas extraction. Finally, application of this model, or similar models, could be useful in identifying localized geographic areas

in which sage-grouse or habitat occupied by sage-grouse may benefit from the constraints and additional project scrutiny associated with listing, while minimizing benefits (and constraints) that would be associated with listing the taxon in its entirety.

Schroeder et al. (2004):
DISTRIBUTION OF SAGE-GROUSE IN NORTH AMERICA

Citation:
Schroeder, M.A., Aldridge, C.L., Apa, A.D., Bohne, J.R., Braun, C.E., Bunnell, S.D., Connelly, J.W., Deibert, P.A., Gardner, S.C., Hilliard, M.A., Kobriger, G.D., McCarthy, C.W., McCarthy, J.J., Mitchell, D.L., Rickerson, E.V., Stiver, S.J. (2004) Distribution of sage-grouse in North America. *Condor* 106:363–373.

Abstract from Schroeder et al (2004):
"We revised distribution maps of potential presettlement habitat and current populations for Greater Sage-Grouse (Centrocercus urophasianus) and Gunnison Sage- Grouse (C. minimus) in North America. The revised map of potential presettlement habitat included some areas omitted from previously published maps such as the San Luis Valley of Colorado and Jackson area of Wyoming. Areas excluded from the revised maps were those dominated by barren, alpine, and forest habitats. The resulting presettlement distribution of potential habitat for Greater Sage-Grouse encompassed 1 200 483 km^2. with the species' current range 668 412 km^2. The distribution of potential Gunnison Sage-Grouse habitat encompassed 46 521 km^2, with the current range 4787 km^2. The dramatic differences between the potential presettlement and current distributions appear related to habitat alter- ation and degradation, including the adverse effects of cultivation, fragmentation, reduction of sagebrush and native herbaceous cover, development, introduction and expansion."

Review of Schroeder et al. (2004):
DISTRIBUTION OF SAGE-GROUSE IN NORTH AMERICA

Review by: Dr. Rob Roy Ramey II

This paper estimates the potential habitat of sage grouse from the time of pre-European settlement in North America to the present. The authors define their pre-settlement baseline as *"the period prior to1800"*. The authors utilize historic distribution maps, museum records, published accounts, Kuchler's potential natural vegetation maps, and other information to define potential pre-settlement habitat. The criteria by which this information was used in developing the pre-settlement potential habitat map was not well explained and was subjective. The definition of historic habitat is based on circular reasoning as locations outside of sagebrush habitat were excluded from consideration. Recent information (e.g. location data from states) and edits by state biologists were used to produce the map of current distribution. The authors report that the pre-settlement (pre-1800) potential habitat for greater sage-grouse encompassed 1,200,483 km^2, and that the current range is 668,412 km^2, a 56% reduction. The distribution of potential Gunnison sage grouse habitat was estimated to be 46,521 km^2, with a current range of 4,787 km^2, a 90% reduction. This paper is widely cited. While there are few issues with the current distribution map, the basis of the pre-settlement potential habitat map (and habitat loss estimates derived from it) is questionable.

S04.1) The authors use pre-1800 as their "pre-settlement potential habitat" baseline. However, there are several invalid assumptions not acknowledged by the authors that accompany use of this as a historic baseline.

First, the author's pre-settlement potential habitat (pre-1800) falls within a period extending from approximately 1400 to 1850 that was known as the *Little Ice Age*, a period of cold, arid climate (0.5-0.9°C lower then present). Thus, climatic conditions and vegetation at that time differed substantially from what would occur today even without human influences (i.e. Kuchler's potential vegetation). This problem is further exacerbated by the fact that the years surrounding 1800 correspond to one of the three temperature minimums during the Little Ice Age (Tausch et al. 1994; Peterson 2002; Yu et al. 2002). The pollen record, published in numerous studies, clearly shows that the distribution of sagebrush and other vegetation fluctuated with climate during the Holocene, and that rising temperatures and precipitation *since* the Little Ice Age (past 150 years) have resulted in changing conditions for sagebrush (e.g. upslope range expansion in some areas and invasion by juniper in others) (Miller and Wigland 1994). Therefore, a comparison between pre-1800 potential habitat and presently occupied habitat confounds the effects of natural climate change with changes that occurred as a result of post-1800 settlement.

Second, the author's simplistic application of Kuchler's potential natural vegetation (Kuchler 1966, 1985) to predict sagebrush habitat during a period of colder climate (the Little Ice Age) produces erroneous results. PNV classifications are based on

hypothetical 'climax' vegetation that could potentially occupy a site without disturbance or climatic change based on current conditions (Zerbe, 1998). According the Kuchler (1964) PNV is *"the vegetation that would exist today if human beings were removed from the scene and if the resulting plant succession were telescoped into a single moment."* Thus, Kuchler's PNV was never intended to extend to historic climate.

Third, Kuchler's (1964, 1985) PNV classifications are qualitative, generalized descriptions of vegetation communities. These do not take into account the mosaic nature of natural landscapes, including successional stages, nor do they accurately characterize physiographic features or soil type (Aminian et al. 1998). Any of these would alter predictions if incorporated into PNV models. The limitations with Kuchler's PNV have been known for decades (Harris 1965): *"From the geographer's point of view, however, the major weakness of this new map* [Kuchler 1964] *is that the vegetation units it portrays are theoretical entities of unequal validity: some established with near certainty, others dependent on informed guesswork. It is not a map of the vegetation of the United States as it existed in 1964, nor of the vegetation in pre-European times (as was Shantz and Zon's map by implication), nor yet of the pre-aboriginal 'natural' vegetation. Instead it is a summary of prevalent opinion as to the likely ecological status of many different types of American vegetation."* More recently, correspondence problems between PNV and actual vegetation have led some authors to rethink its application to real world problems (Wright et al. 1998; Zerbe 1998). Even when comparing actual vegetation with PNV over a limited area, the correspondence between the two is poor (Chen et al. 2008). Although Kuchler's PNV may be a useful heuristic tool for some applications, it is neither realistic in its assumptions nor a reliable predictive tool.

S04.2) The effect of Native Americans (prior to 1800) on sage grouse and their habitat is not acknowledged by the authors. New evidence reveals that North and South America were inhabited by an estimated 40 and 112 million people prior to European contact (Mann 2005). While the population density of Native Americans was concentrated in temperate regions, and had clearly declined as a result of introduced diseases prior to 1800, archeological evidence shows that these aboriginal people affected sage grouse in two ways. First, as noted by Schroeder et al. (2004) and others, sage grouse were hunted by Native Americans. And second, range fires were set by Native Americans to improve edible forage and game (Agee 1993), which also affected plant succession (Miller and Wigland 1994). Thus, the pre-1800 pre-settlement period selected by Schroeder et al. (2004) was not without human impacts to sage grouse.

S04.3) The are discrepancies between the published paper and the metadata description as to what "pre-settlement" means. For example, Schroeder et al. (2004) state: *"Consequently, we used the term "pre-settlement" to define the period prior to 1800, before rapid settlement by people of European descent."* However, the metadata associated with the GIS overlays for this paper defined pre-settlement as later than 1800**:** *"The 'pre-settlement' reference time generally refers to the early 1800's, when Europeans generally settled in western North America, even though settlement may have occurred*

much earlier in some locations." Metadata may be found here: *http://metadata.nbii.gov/clearinghouse/send/xsltText2;jsessionid=8337ED5924570D61F5840454B0C3FF79?fileURL=http%3A%2F%2Fmercury.ornl.gov%2Fmetadata%2Fnbii%2Fhtml%2Fwsgc%2Fwa-node.gis.washington.edu_waf_uw_dfw_na_sage_historic.html&full_datasource=Washington+State+Geospatial+Clearinghouse+Node&full_queryString=+(+title+%3A+Pre-settlement+distribution+of+potential+habitat+Greater+of+Sage-Grouse+and+Gunnison+Sage-Grouse+in+North+America+-+NA_Sage_Historic+)&ds_id=*

S04.4) The methods used by Schroeder et al. (2004) to define the pre-settlement distribution of potential habitat were poorly defined. It is unclear as to how historical accounts were quantified and incorporated into the mapping data, and use of some data appears to be subjective. Such issues raise the question of reproducibility. For example, the author's note that they *"considered 1,167 records of museum specimens"* and were *"cautious in our interpretations because of potential inaccuracies in recorded locations and the ability of individual sage grouse to travel long distances."* This led the authors to 166 records being rejected. The authors also considered *"138 published observations"* but do not explain how those were specifically used.

Additionally, observations outside the current sage grouse distribution were treated differently, from those within the current range: *"Because many published observations and museum specimens were poorly documented, we primarily considered these data in terms of their generalities."* The meaning of "generalities" is not explained by the authors.

The following excerpts are from the meta-data description accompanying data files associated with this paper. I have underlined relevant passages:

> *"The pre-settlement distribution is extremely difficult to document due to a paucity of information. Consequently sources like the journal of Meriwether Lewis and William Clark were used, which described the 1803-1806 expeditions from the Saint Louis, Missouri area to the west coast of Oregon and back (transcribed by Moulton (1986-1997; Volumes 2-11)). Swainson and Richardson (1831) was one of the few contempory publications that provided information about the early distribution of sage-grouse. Additional summaries on the historic distribution were also considered including McClanahan (1940) and Aldrich and Duvall (1955). The extent of the presettement ranges were also modified with the aid of maps of original habitats (Kuchler 1964). References on habitat distribution were evaluated relative to specific information on habitat use (Schroeder et al. 1999, Connelly et al. 2000). <u>The source information gathered on the historical range was reviewed, interpreted, synthesized, and then transcribed by Dr. Schroeder onto 1:2,000,000 scale U.S. Geological Survey Maps</u>. The polygonal data on the U.S. Geological Survey maps was then digitized and attributed, using ARCINFO software, to create a working draft coverage of*

sage-grouse distribution for North America.

"Use_Constraints:
This data was compiled at a scale of 1:2,000,000. Data should only be used for general display, mapping, and planning purposes at scales of 1:2,000,000 or smaller. Extreme care was taken during compilation of these boundaries to ensure accuracy. However, Washington State Department of Fish and Wildlife (WDFW) had to rely on outside sources of information when compiling these data, and therefore cannot accept any responsibility or liability for errors and/or omissions in the use of these data. WDFW provides no warranties to accompany this data".

"Completeness_Report:
Given the scale (1:2,000,000) the data is obviously highly generalized. For more specific information about an individual state or province it would be best to contact the natural resource managment agency responsible for managing the Sage-grouse in that jurisdiction. The accuracy varies from state to state depending on the knowledge of the biologists involved."

Distribution_Liability:
The Washington Department of Fish and Wildlife (WDFW) makes no guarantee concerning the data's content, accuracy, completeness, or the results obtained from the queries or use of these data. WDFW makes no warranty of fitness for a particular purpose, no representation as to the quality of any data, and assumes no liability for the data represented here. These data do not represent exhaustive inventories, but are compilations of existing knowledge from WDFW staff that are updated periodically as knowledge improves. These data should be used cautiously because they are not exhaustive, and are subject to change. When conducting projects or planning for fish or wildlife, please consider using additional information gathered from other spatial sources and consultations with WDFW staff."

Reproducibility is essential to scientific inquiry. However, the pre-settlement distribution map of Schroeder et al. is not reproducible because of poorly defined methodology and subjective use of information. If potential habitat maps are to be used as the basis of policy, it needs to be based on a repeatable methodology.

S04.5) The authors considered 1,167 museum specimen records and 138 published observations in compiling their pre-settlement potential habitat map, however it appears that all (or virtually all) records and observations occur *after* 1800. The earliest records cited were from the Lewis and Clark expedition that took place from 1803 to 1806. The next was Swainson and Richardson in 1831.

To determine of the oldest museum collections in the U.S. had sage grouse specimens, the online collections were accessed at the National Museum Of Natural History and the American Museum of Natural History. Of 144 museum records of *Centrocercus* in the

National Museum of Natural History, the earliest were from 1853 in Washington (records: A10019, A10021, and A10022). Of 14 records in the American Museum of Natural History, the earliest specimen was from 1887 in Montana (record: SKEL 60). These post-1800 museum and historical records were plotted on the pre-1800 pre-settlement potential habitat map.

S04.6) An alternative hypothesis that could explain the lack of early sage grouse sightings in northern Montana, Alberta, and Saskatchewan, was not considered by Schroeder et al. (2004). Schroder et al. (2004) included northern Montana east of the Continental Divide and areas north, including Alberta and Saskatchewan in pre-settlement habitat. However, the Lewis and Clark expedition did not report sage grouse along the Missouri River east of the Rocky Mountains in Montana. Rather than considering the possibility of a recent (post-1800) range expansion due to post Little Ice Age warming, Schroeder et al. chose to explain away the absence of sage grouse sightings: *"It is possible that Lewis (Moulton 1987) and others might not have observed sage-grouse because of low densities along their primary travel corridors. Periodic fluctuations in the abundance of sage-grouse (or cycles, Rich 1985) may also have had an impact."* The alternative hypothesis, that this was a range expansion that followed the Little Ice Age, was not considered by the authors.

Review of Schroeder et al. (2004):
DISTRIBUTION OF SAGE-GROUSE IN NORTH AMERICA

Review by: (This reviewer wishes to remain anonymous.)

S04.7) I'm fundamentally bothered in this paper by what appears to be a circular definition of the limits of historic range. On page 366, the authors state:

> *"Habitats without known use by sage-grouse were excluded from the presettlement distribution of potential habitat, even if there were scattered observations or recoveries of sage-grouse."*

Then on page 372 they state:

> *"The location of some observations and museum specimens were outside the perimeter we delineated for the presettlement distribution of potential habitat. There have been numerous (italics added) observations of sage-grouse in areas outside big-sagebrush dominated habitats, particularly in Colorado, Kansas, Nebraska, Oklahoma, and the Dakotas. Because of these observations and the large area (italics added) involved, our distribution of potential habitat may be a conservative estimate of the total amount of area occupied in the past."*

Evidently the authors are limiting potential habitat to big-sagebrush dominated areas despite an abundance of records in a large area that is not big-sagebrush dominated. Further, they evidently assume that these records did *not* represent "use" by sage-grouse – a bold conclusion difficult to justify. Range of any species presumably should be defined by regular presence of the birds, not by presence of habitat types that the authors deem to be essential to the species, because they just might be wrong about habitat dependencies of the species. Surely with all species one can expect to have occasional occurrences outside normal range, but when one obtains "numerous observations" of birds outside of designated range, it's time to redefine the range of the species. If this means that one also has to redefine "acceptable" habitat for the species, so be it. Perhaps sage-grouse do not really require big-sagebrush dominated habitat. This would be my conclusion from what is presented here, and it makes me suspicious that the authors have greatly underestimated the true size of presettlement range. Kansas and Oklahoma, which evidently hosted numerous early observations of the species, are not even close to the presettlement range identified by the authors. Something is evidently amiss here, and this seems to be recognized in a left-handed sort of way in the second quote above. This paper should present full data on records outside the presettlement range identified by the authors (not just museum specimens and published locations (Fig. 1)), but also the rough locations of the 830 observations that were not mapped in this figure.

S04.8) I'm surprised that the reviewers for this paper evidently did not give the authors a hard time on these matters prior to publication. It looks basically like a huge area east of the east boundary of "presettlement range", extending from Texas to the Dakotas might also have been true sage-grouse habitat, though not necessarily optimal habitat. How this

might have affected the conclusions of other authors who have relied on this paper should be examined closely. Judging simply from the information provided by the authors in this paper, their presettlement range is probably not reliable.

S04.9) By comparison, consider the case of Swainson's Warbler, a species that has been long famed for its association with cane thickets in the southeastern United States. Yet another population of this species was discovered in the 1930s in rhododendron-mountain laurel thickets of the Appalachians, and this demonstrated that the species was not a true cane specialist. The Appalachian population would have been ruled out by the sort of logic employed in this sage-grouse paper. Common habitat associations do not prove strict dependencies and we are well advised not to underestimate habitat tolerances of a species, especially when true limiting factors for populations are not thoroughly understood. Attempts to define habitat tolerances of a species based on small sample sizes are always risky, but "ranges" that *exclude* from consideration areas and habitat types where *numerous* records of a species exist, are surely unjustified.

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Appendix 1. Review team bios. (Note: The professional affiliations of reviewers are listed, however, the statements and opinions of reviewers are solely their own professional judgments.).

Dr. Rob Roy Ramey II, Wildlife Science International, Inc.
Dr. Ramey has 32 years of experience with research, management, and recovery of endangered species in the U.S. and abroad. He provides measured and impartial review of scientific information and counters misinformation with facts and sound arguments. Dr. Ramey has extensive experience working with threatened and endangered species, including fieldwork on the front lines of research and recovery of California condors, California and Peninsular Ranges bighorn sheep, argali, peregrine falcons, and African elephants. He has also conducted original research or provided scientific review on a wide variety of proposed and ESA-listed species, subspecies, and Distinct Vertebrate Population Segments, including: coastal California gnatcatcher, western snowy plover, northern spotted owl, California brown pelican, Gunnison sage grouse, greater sage grouse, Sierra Nevada bighorn sheep, desert bighorn sheep in the Peninsular Ranges of California, Mexican wolf, Canadian lynx, arroyo toad, California tiger salamander, Pacific pocket mouse, two subspecies of meadow jumping mouse, multiple subspecies of beach mice in Florida, and Delta smelt. Clients have included a wide variety of governmental agencies, companies, tribes, individuals, NGOs, and law firms.

Dr. Vernon C. Bleich, (Eastern Sierra Center for Applied Population Ecology) recently retired as a senior biologist from California Department of Fish and Game. He is recognized as an expert on four federally endangered taxa: Stephens kangaroo rat, Amargosa vole, Sierra Nevada bighorn sheep, and Peninsular bighorn sheep. He has served as a member of the Peninsular Bighorn Sheep Recovery Team and the Sierra Nevada Bighorn Sheep Science Team, and oversaw preparation of the Sierra Nevada Bighorn Sheep Recovery Plan. His research has included projects on sage grouse, as well as wildlife responses to mining and habitat fragmentation in the west. He recently participated in a critical review of sage grouse population trends derived from lek count data (organized by WSI).

Dr. Mathew Cronin, Research Assistant Professor, University of Alaska, Fairbanks.
Dr. Cronin has conducted research and published on numerous ESA-listed species and issues, including the coastal California gnatcatcher, polar bear, and beluga whale. His research and publication on the gnatcatcher revealed the arbitrary criteria used to describe this and other subspecies and DPS designations. The polar bear and beluga whale population genetic research did not support proposed DPS designations. In 2005 he reported that, contrary to expectations, the caribou population that utilizes Prudhoe Bay had more than doubled in size over the previous two decades. Dr. Cronin submitted extensive comments to the Fish and Wildlife Service on the proposed listing of polar bears as threatened.

Annonymous - (U.S. Fish and Wildlife Service, Patuxent Wildlife Research Center - retired).

Annonymous is an extensively published research ornithologist, who's foresight as a recovery team leader led to two successful recovery programs, saving these species from extinction.

Dr. John D. Wehausen, Research Scientist, White Mountain Research Station, University of California, San Diego (retired).
Dr. Wehausen's expertise is in population estimation, demography, taxonomy, and epistemology. He has served as a member of the Peninsular Bighorn Sheep Recovery Team and the Sierra Nevada Bighorn Sheep Science Team. He worked on preparation of the Sierra Nevada Bighorn Sheep Recovery Plan, and consulted on the Coachella Valley Habitat Conservation Plan. He shares authorship on three papers synonymizing the so-called Preble's meadow jumping mouse with the nearby subspecies, Z. h. campestris (and two response papers defending that synonymy). He recently participated in a critical review of sage grouse population trends derived from lek count data (organized by WSI).

Dr. Robert Zink, Breckenridge Chair in Ornithology, Bell Museum; Professor, University of Minnesota.
Dr. Zink has published extensively for 25 years in the field of population genetics and phylogeography. Many of his papers have dealt with species of conservation concern, such as the California Gnatcatcher and the Black-Capped Vireo. He has also published a series of papers on the genetics of Hawaiian freshwater fishes. He was an invited instructor at a workshop sponsored by the USFWS on genetic studies in conservation. He has provided written comments on genetic work related to the Gunnison Sage Grouse, Snowy Plover (western) and Spotted Owl. Dr. Zink brings a critical and objective viewpoint to the discussion of the relevance and role of genetic studies in conservation.

Dr. Laura M. Brown, Wildlife Science International, Inc. (Editor)
Dr. Brown's expertise is in ornithology, habitat fragmentation, and making scientific writing accessible to the public and decision makers.