Summary Report of Independent Peer Reviews for the U.S. Fish and Wildlife Service Gray Wolf Delisting Review

May 2019

Forward

Atkins North America, Inc. (Atkins), was retained by the U.S. Fish and Wildlife Service (Service) to facilitate an independent scientific review of the Gray Wolf Biological Report (October 31, 2018) and the Proposed Rule for Endangered and Threatened Wildlife and Plants; Removing the Gray Wolf (*Canis lupus*) from the List of Endangered and Threatened Wildlife [50 Code of Federal Regulations (CFR) Part 17, March 15, 2019]. Atkins believes the peer reviewers have successfully met the Service's charge for their reviews, which provide opinions and/or detailed analysis on the scientific data and interpretation of the data in the documents.

Reviewer comments are focused on two questions related to the objectives of the Draft Biological Report: provision of an adequate and concise overview and potential oversights or omissions. Reviewer comments also address five questions regarding the Proposed Delisting Rule: provision of an adequate review and analysis, adequate consideration of impacts of range reduction, reasonable conclusions, oversights or omissions, and demonstrable errors of fact or interpretation.

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1. Introduction

1.1. Background

The U.S. Fish and Wildlife Service (Service) has completed a proposed rule to remove the gray wolf (*Canis lupus*) from the List of Endangered and Threatened Wildlife. In accordance with the Service's July 1, 1994 peer review policy (59 FR 34270) and the Office of Management and Budget's December 16, 2004, Final Information Quality Bulletin for Peer Review, the Service is subjecting this proposal to independent expert peer review. The Service is seeking peer review of the Draft Biological Report and the Proposed Delisting Rule.

1.2. Purpose and Scope of Peer Review

The purpose of this review was to provide external scientific peer reviews of the information in the Draft Biological Report and Proposed Delisting Rule and supporting technical memoranda. The Draft Biological Report and Proposed Delisting Rule peer review was conducted to ensure use of the best scientific and commercial information available and to ensure and maximize the quality, objectivity, utility, and integrity of information upon which the proposal is based, as well as to ensure that reviews by qualified experts are incorporated into the rulemaking process.

The Service asked Atkins to select five peer review participants based on expertise, with due consideration of independence and conflict of interest. While expertise was the primary consideration, reviewers were also selected to represent a diversity of scientific perspectives relevant to the subject.

The reviewers were charged with identifying and characterizing any scientific uncertainties within the two documents and were advised not to provide advice on policy. The reviewers were instructed to provide a written review of the Draft Biological Report and Proposed Delisting Rule, with special emphasis on answering the key questions related to the logic of the Service's assumptions, arguments, and conclusions and to provide any other relevant comments, criticisms, or thoughts. The reviewers did not have a defined format and were free to comment on any aspects of the Draft Biological Report and Proposed Delisting Rule and supporting data to which they felt a comment was warranted. Reviewers were provided the references cited by the Service and also encouraged to include additional references in their review, if appropriate, as well as refer to citations used by the Service.

Specifically, the Service requested that the peer reviewers cover, but not be limited to, the topics listed below and that the reviews focus on how thoroughly and logically the topics have been treated, and how well the conclusions are supported by the data and analyses. Not all reviewers were required to address all issues noted below but instead reviewers were asked to comment on areas within their expertise and were given the option to abstain from other areas.

Questions on the Draft Biological Report for Peer Review:

1. Does the draft report provide an adequate and concise overview of gray wolf (*Canis lupus*) taxonomy, biology, and ecology as well as the changes in the biological status (range, distribution, abundance) of the gray wolf in the contiguous 48 United States over the last several decades?

2. Please identify any oversights or omissions of data or information, and their relevance to the report. Are there other sources of information or studies that were not included that are relevant to the biological report? What are they and how are they relevant?

Questions on the Proposed Rule for Peer Review:

1. Does the proposed rule provide an adequate review and analysis of the factors relating to the persistence of the gray wolf population currently listed under the Endangered Species Act in the contiguous 48 states (human-caused mortality, habitat and prey availability, disease and predation, and effects of climate change)?

2. Have we (the Service) adequately considered the impacts of range reduction (i.e., lost historical range) on the long-term viability of the gray wolf in its remaining range in the lower 48 states (outside of the northern Rocky Mountains) and, if not, what information is missing and how is it relevant?

3. Is it reasonable for the Service to conclude that the approach of Michigan, Wisconsin, and Minnesota to wolf management, as described in their Plans and the proposed rule and in the context of wolf management in the Western Great Lakes area, are likely to maintain a viable wolf population in the Western Great Lakes area into the future?

4. Please identify any oversights or omissions of data or information, and their relevance to the assessment. Are there other sources of information or studies that were not included that are relevant to the proposed rule and, if so, what are they and how are they relevant?

5. Are there demonstrable errors of fact or interpretation? Have the authors of the Proposed Delisting Rule provided reasonable and scientifically sound interpretations and syntheses from the scientific information presented in the Draft Biological Report and the proposed rule? Are there instances in the proposed rule where a different but equally reasonable and sound interpretation might be reached that differs from that provided by the Service? If any instances are found where this is the case, please provide the specifics regarding those particular concerns.

2. Peer Review Process

Atkins was retained by the Service to facilitate the peer review process. The terms of the contract included the following:

- Acceptance of anonymous proposed peer reviewers by the Service (see Section 2.1 for the reviewer selection process);
- Organize, structure, lead, and manage the scientific review;
- Facilitate specific follow-up questions/answers between the Service and the reviewers, without attribution; and
- Prepare and submit a Final Report and Administrative Record to the Service.

Atkins Project Manager, Cheryl Propst, with oversight from Project Director, Matt Cusack, and the assistance of Principal Technical Professional, Don Deis, facilitated this review (i.e., Atkins Team).

2.1. Selection of Reviewers

Atkins was responsible for assigning an experienced, senior, and well-qualified project manager to oversee the selection of five well-qualified, objective, independent reviewers. Qualified reviewers were required by the Service to have the following credentials:

1. Each reviewer must have a Ph.D. or an M.S. with significant experience in Wildlife Biology, Ecology, or Wildlife Management; and
2. In combination, the expertise of qualified reviewers shall include the following; however, each individual is not required to meet all qualifications:
    a. Demonstrated experience or expertise with large carnivore management, especially wolves.
    b. Expert knowledge of conservation biology, wildlife management, demographic management of mammals (especially carnivores), genetics, population modeling, and/or scientific literature on wolves or other carnivores.
    c. Expert knowledge of mammalian taxonomy/systematics.
    d. Experience as a peer reviewer for scientific publications.

The Service awarded the contract for five reviewers, and the Atkins Team identified five individuals who met the selection criteria that were willing and available to participate in the review. During the process of identifying and selecting reviewers, Atkins verified candidates' availability over the period of performance as well as flexibility to accommodate schedule changes. The Service relied entirely on Atkins to identify the qualified reviewers; the Service did not review any peer reviewer qualifications or other potentially identifying documentation.

Atkins used a variety of sources to identify potential peer review candidates and convene an experienced and balanced panel with a regional distribution. Methods included the use of internet searches (e.g., GoogleScholar); recommendations from colleagues; and review of relevant scientific journals, conference proceedings, and other publications. Atkins screened 26 potential candidates with respect to the criteria described above as well as to ensure that they represent different employment affiliations and avoid potential conflicts of interest. Ten of the potential candidates were not responsive to the initial inquiry, seven could not participate due to schedule, two did not meet expertise requirements, and two had conflicts of interest; the remaining five met all criteria and were able to maintain flexibility to accommodate schedule changes. The final composition of reviewers was balanced in terms of expertise in large carnivore management (especially wolves), conservation biology, wildlife management, demographic management of mammals (especially carnivores), genetics, population modeling, and/or scientific literature on wolves or other carnivores. A summary of the peer reviewer selection criteria and their areas of expertise is provided in **Table 1**. Resumes/curricula vitae for the peer reviewers are included in **Appendix A**. Atkins consulted appropriate Service staff as questions from peer reviewers emerged and provided impartiality in interactions between Service technical staff and the reviewers.

Table 1. Summary of Peer Reviewer Selection Criteria and Qualifications

Peer Reviewer Selection Criteria and Expertise	Reviewer 1	Reviewer 2	Reviewer 3	Reviewer 4	Reviewer 5
Criteria 1: Education includes Ph.D. or an M.S. with significant experience in Wildlife Biology, Ecology, or Wildlife Management	Ph.D.	Ph.D.	M.S.	Ph.D.	Ph.D.
Criteria 2a: Demonstrated experience or expertise with large carnivore management, especially wolves	Yes	Yes	Yes	Yes	Yes
Criteria 2b: Expert knowledge of conservation biology, wildlife management, demographic management of mammals (especially carnivores), genetics, population modeling, and/or scientific literature on wolves or other carnivores	Yes	Yes	Yes	Yes	Yes
Criteria 2c: Expert knowledge of mammalian taxonomy/systematics	Yes	Yes	Yes	Yes	Yes
Criteria 2d: Experience as a peer reviewer for scientific publications	Yes	Yes	Yes	Yes	Yes
Area of expertise	Population genetics and conservation biology	Conservation biology	Wolf biology	Coexistence and conflicts between people and wildlife	Animal ecology and conservation

Atkins used a Conflict of Interest disclosure form provided by the Service, as well as information from the Scope of Work to establish screening criteria for candidates regarding balance of expertise, independence, and conflict of interest. The conflict of interest form includes a detailed definition of conflict of interest, as well as questions related to personal or professional relationships and investment, property, or other interests that may constitute a conflict. The conflict of interest forms completed and signed by the reviewers are included in **Appendix B**.

2.2. Document Review and Report Development

Upon award of the contract, the Atkins Team coordinated with the Service's technical representatives, Maricela Constantino and Ellen Van Gelder, to discuss the scope of the review and address any questions. Ms. Constantino distributed the Draft Biological Report, Proposed Delisting Rule, and supporting literature cited in the Draft Biological Report and/or Proposed Delisting Rule to Atkins for performance of the peer review.

Once under contract, the Atkins Team coordinated individually with the reviewers to describe the scope of services to be provided to the Service, including the charge to the reviewers and peer review schedule. The Atkins Team convened a kick-off teleconference with each reviewer following distribution of the Draft Biological Report, Proposed Delisting Rule, and supporting literature to review materials before commencing the review and answer any questions. Atkins emphasized the importance of each reviewer focusing on the scope of the peer review and not commenting on areas that are the purview of the government (e.g., policy). Atkins also directed each reviewer to maintain their anonymity (as per a signed non-disclosure agreement with Atkins) and make certain their review comments consist of unbiased assessments. Additionally, Atkins informed the reviewers that all peer review-related inquiries from outside sources must be forwarded to the Atkins' project manager; reviewers were instructed to not communicate with those inquiring about the review.

As questions or issues emerged from individual reviewers, responses (from Atkins, or Service staff via Atkins) were communicated to all reviewers via email to provide all reviewers with similar directions. If necessary, a teleconference between Atkins and an individual reviewer was convened for further clarification.

During the review process, requests were received from two reviewers for references cited in the Draft Biological Report and/or Proposed Delisting Rule but not already provided by the Service. One reviewer requested three references cited in the Draft Biological Report and one reference cited in the Proposed Delisting Rule; a second reviewer requested two references cited in the Draft Biological Report. Atkins coordinated with the Service and delivered the requested documents to all reviewers within one business day. On May 1, 2019, Ms. Constantino made available additional references cited by the Service in the Draft Biological Report but not previously provided; Atkins distributed these references to all reviewers within the same business day. On May 3, 2019, Atkins noticed that one of the reviews received had mentioned two citations within the Draft Biological Report that had not been provided by the Service. Atkins coordinated with the Service and provided the documents within the same business day.

Reviewers submitted their individual review comments to the Atkins Team by May 3, 2019, and Atkins submitted the unmodified reviews to the Service on May 3, 2019. All attribution was removed and replaced with a number (i.e., Reviewer 1, Reviewer 2, etc.). On May 14, 2019, a teleconference was held between Atkins and the Service to discuss agency questions and comments. As noted above, one of the reviews received on May 3, 2019, had mentioned two citations within the Draft Biological Report that had not been provided by the Service. Given that the Service provided additional reference materials cited in the Draft Biological Report after reviewers submitted their reviews, Atkins, at the direction of the Service, encouraged the reviewers to ensure that all materials provided by the Service were considered in their reviews and that full and complete reviews were provided without any limitations. On May 14, 2019, the Service sent requested clarification questions to Atkins regarding individual peer reviewer responses; the Service had clarification questions for Reviewers 1, 2, and 4. Individual comments were provided to the associated reviewers via email on May 15, 2019. Updated reviews and responses, or confirmation that no changes were made to the original reviews, were submitted to Atkins by all reviewers by May 24, 2019. The final compiled individual reviews and responses to clarification questions are included in this document as **Appendix C**.

All relevant documents that have been maintained throughout the peer review process were compiled and provided to the Service for the Administrative Record. Example files include emails from Service staff to Atkins providing direction or clarification, the scope of services included in each reviewer's subcontract, emails from Atkins to reviewers providing direction or clarification, and emails documenting draft and final report submittals.

Appendices

Appendix A. Reviewer Curricula Vitae

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Curriculum Vitae for Reviewer 1

Dr. Fred W. Allendorf

CURRICULUM VITAE

FRED W. ALLENDORF August 2018
E-mail: fred.allendorf@gmail.com

Division of Biological Sciences
University of Montana
Missoula, MT 59812
USA

MILITARY SERVICE: U.S. Army, 1965-1968 (Vietnam, 1966-1967)

EDUCATION: B.S., Zoology, Pennsylvania State University, 1971
M.S., Fisheries, University of Washington, 1973
Ph.D., Fisheries and Genetics (Interdisciplinary Individual PhD), University of Washington, 1975
(co-directors, Fred Utter and Joe Felsenstein)

POST-DOCTORAL: Research Fellow, 1975-1976, Aarhus University, Denmark (Freddy Christiansen)
NATO/NSF Fellow, 1978-1979, Nottingham University (Bryan Clarke)

RESEARCH INTERESTS: Population Genetics and Conservation Biology

APPOINTMENTS

2009-present	Regents Professor of Biology Emeritus, University of Montana
2013-2016	Adjunct Professor of Biology, Victoria University of Wellington, New Zealand
2013	Fulbright Senior Specialist, University of Western Australia
2013	Visiting Scientist, Hatfield Marine Science Center, Oregon
2005-2012	Professorial Research Fellow, Victoria University of Wellington, New Zealand
2012	Visiting Scientist, Hawai'i Institute of Marine Biology, UH-Manoa
2010-2011	Lavern Weber Visiting Scientist, Hatfield Marine Science Center, Oregon
2009-2010	Distinguished Visiting Scientist Fellowship, CSIRO, Tasmania, Australia
2007-2008	Gledden Visiting Senior Fellowship, University of Western Australia
2004-2008	Regents Professor of Biology, University of Montana
2000-2001	Fulbright Senior Scholar, Victoria University of Wellington, New Zealand
1997-1998	Hill Visiting Professor, University of Minnesota
1992-1993	Visiting Professor, University of Oregon
1989-1990	Program Director, Population Biology, National Science Foundation
1983-1984	Visiting Scientist, Department of Genetics, University of California, Davis
1976-2004	Assistant, Associate, and Professor of Biology, University of Montana

AWARDS

Awarded Pennsylvania State University College of Science Outstanding Science Alumni Award, 2018

Speaker in President's Distinguished Lecture Series, University of Vermont, April 2017. (Zen & Deep Evolution: When Did Your Life Begin?)

Awarded the *Molecular Ecology Prize* by the journal Molecular Ecology for lifetime achievements in the fields of molecular ecology and conservation genetics, 2015

One of five invited speakers at Crafoord Prize Symposium in Stockholm, 5 May 2015, entitled "Genetic variation in natural populations". The prize was awarded to Richard Lewontin and Tomoko Ohta for "their pioneering

analyses and fundamental contributions to the understanding of genetic polymorphism." The presentation can be viewed at http://www.crafoordprize.se/events/crafoorddays2015.4.76308e0c152098549fa147d.html.
One of four scientists inducted into the inaugural group of the American Fisheries Society's Genetics Section Hall of Excellence, 2014.
Fulbright Senior Specialist, University of Western Australia, 2013
Received American Fisheries Society's Award of Excellence in recognition of outstanding contributions to fisheries science and aquatic biology, 2011
Lavern Weber Visiting Scientist Fellowship, Hatfield Marine Science Center, Oregon, 2010
Distinguished Visiting Scientist Fellowship, CSIRO, Australia, 2009
Gledden Visiting Senior Fellowship, University of Western Australia, 2007
Appointed Regents Professor, University of Montana, 2004
Erskine Fellow, University of Canterbury, New Zealand, 2003
Fulbright Senior Scholar, New Zealand, 2000-2001
Special Achievement Award, Montana Chapter American Fisheries Society, awarded for contributions to conservation of native fishes in Montana.
Hill Visiting Professor, University of Minnesota, 1997
President, American Genetic Association, 1997
Appointed to Oscar Craig Circle of Scholars, University of Montana, 1994
Burlington Northern Faculty Achievement Award, University of Montana, 1991
Elected Fellow, American Association for the Advancement of Science (AAAS), 1987
Burlington Northern Faculty Achievement Award, University of Montana, 1987
University of Montana Distinguished Scholar Award, 1985
European Molecular Biology Organisation (EMBO), Fellowship, University of Stockholm, 1979
NATO/NSF Postdoctoral Fellowship, University of Nottingham, 1978-1979
National Marine Fisheries Service, US NOAA, Special Achievement Award, 1973 and 1975

STUDENT HONORS:

Than Hitt awarded Presidential Early Career Award, 2017.
Andrew Whiteley received a 2003 Raney Award from the American Society of Ichthyologists and Herpetologists for his dissertation research under my direction.
Charles Daugherty elected Fellow of The Royal Society of New Zealand, 1999
Gordon Luikart received a Fulbright Fellowship to spend a year (1994-1995) in Australia as part of his dissertation research under my direction.
Steve Forbes (1990) and Dara Newman (1994) received NSF Dissertation Improvement Grants for their dissertation research under my direction.
Robb Leary received the 1984 Theodosius Dobzhansky Prize by the Society for the Study of Evolution for his dissertation research under my direction (Evolution 38:995).

MAJOR GRANTS

NSF Research Grant. 1980-1983, $70,000
NSF Research Grant, Population Biology, 1980-1982, $60,000
NSF Research Grant, Population Biology, 1983-1986, $121,000
NSF Faculty Research Opportunity Award, 1986, $10,000
US Department of Agriculture Grant, Aquaculture, 1983-1985, $43,000
NSF Research Grant, Population Biology, 1986-1989, $148,000
NSF Dissertation Research Grant, Steve Forbes, 1988-1990, $9,850
NSF Research Grant, Population Biology, 1989-1993, $150,000
NSF Research Grant, Conservation and Restoration Biology, 1993-1998, $265,000
NSF Dissertation Research Grant, Dara Newman, 1992-1994, $9,500

NSF Grant. Training within Environmental Biology. P.F. Kukuk (Project Director), C.A. Brewer and F.W. Allendorf (co-PIs). Graduate Research Traineeship, 1995-2002, $562,500.
NSF Equipment Grant, P. Kukuk, L.S. Mills, and F.W. Allendorf (co-PIs). Acquisition of instrumentation in evolutionary and conservation genetics. $128,000.
Project Director, NSF Postdoctoral Research Fellowship in Biological Informatics, Dr. Eleanor Steinberg. 1999-2001. $100,000.
NSF Small Grant for Experimental Research. Detection of trout species by PCR amplification of DNA from stream water. $47,000. 2000-2002.
Exxon Valdez Oil Spill Trustee Council, Construction of a linkage map for the pink salmon genome, 1996-2004, $1,420,000
The Royal Society of New Zealand. Marsden Fund. Do mutations in mitochondrial DNA affect population viability? Co-PI with Neil Gemmell, Canterbury University. $NZ480,000, 3 years.
NSF Small Grant for Experimental Research, A population genomic approach to understand life history variation and sex chromosome evolution in rainbow trout. 2005-2006, $44,000.
Council for International Exchange of Scholars Fulbright Alumni Initiative Program. Collaborative Program in Conservation Biology. 2004-2006. $24,000. Co-PI with C.H. Daugherty.
National Center for Ecological Analysis & Synthesis (NCEAS) and National Evolutionary Synthesis Center (NESCent) jointly funded Working Group. Genetic monitoring: Development of tools for conservation and management. $114,800, 2007-2010. Co-PI with M.K. Schwartz.
NSF. Opportunities for Promoting Understanding through Synthesis (OPUS). Population genetics and management of exploited populations. 2008-2012, $128,653. DEB 0742181. PI with co-PI G.Luikart.
Australian Research Council Linkage Grant. Co-PI with PI, Paul Sunnucks, Monash University. Genomics for persistence of Australian freshwater fish. $AU455,000. 2012-2016.
NSF, Evolutionary Processes. Evolutionary mechanisms influencing the spread of hybridization: genomics, fitness, and dispersal. 2013-2018, $600,000. DEB 1258203. Co-PI with G. Luikart, W.H. Lowe, and C.C. Muhlfeld.

ASSOCIATE EDITORSHIPS: Evolution (1987-1990)
Journal of Heredity (1986-1989)
Molecular Biology and Evolution (1994-1996)
Conservation Genetics (1999-2005)
Proceedings of Royal Society B: Biological Sciences (2008-2014)

EDITORIAL BOARDS: Molecular Biology and Evolution (1983-1989)
Progressive Fish Culturist (1986-1989)
Conservation Biology (1990-1993)
Conservation Biology (Special Assigning Editor, 2002-2010)
Molecular Ecology (1991-1995)
New Zealand Journal of Marine and Freshwater Research (2003-2012)
Evolutionary Applications (2011-present)
Journal of Heredity, Review and Perspective Editor (2013-present)

PROFESSIONAL SERVICE:

Panel Member, Population Biology and Physiological Ecology, NSF (1987-1989)
Panel Member, International Program, NSF (1987)
Panel Member, Conservation and Restoration Biology, NSF (1991-1992; 1995)
Council Member, American Genetic Association (1986-1989)
Genetics Nomenclature Committee, American Fisheries Society (1986-1991)
Member, Committee on the Protection and Management of Pacific Northwest Anadromous Salmonids, National Research Council (1992-1995)
Chair, Committee of Visitors, Systematic and Population Biology Programs, NSF (1993)
Member, AAAS Council, Biological Sciences Division (1996-1998)

President, American Genetic Association (1997)
Panel member, Dissertation Improvement Grants, NSF (1997)
Member, Board of Trustees, The Nature Conservancy, Montana Chapter (1995-2001).
Chair, Nominations Committee, American Genetic Association (1999)
Member, Invasive Species Collaboratory, NSF (1999-2004)
Co-Chair, Organizing Committee, 2000 Annual Meeting of Society for Conservation Biology (SCB), Missoula, MT
Panel Member, Evolutionary Genetics, NSF (2003-2007)
Fulbright Program for U.S. Students, Science Proposal Review Panel, 2005-2008.
Member, International Scientific Advisory Panel, Allan Wilson Centre for Molecular Ecology & Evolution, New Zealand. 2005-2016
Panel Member, Organism-Environment Interactions, Division of Integrative Organismal Systems, NSF (2013)
Chair, Molecular Ecology Prize Committee, 2016; Member Molecular Ecology Prize Committee, 2018.

TEACHING

I taught the following courses during my last five years at the University of Montana before I retired:

Genetics and Evolution (Biol 223; lower-division)
Conservation Genetics (Biol 480; upper-division/grad)
Conservation Ecology (Biol 452; field course, Flathead Lake BioStation)
Advanced Population Genetics (Biol 595; graduate)
Micro- and Macro-Evolution, co-taught with Ken Dial (Biol 595; graduate)
Ecology and Buddhism (Environmental Studies 594; graduate)

Nicky Nelson (Victoria University of Wellington, VUW, New Zealand) and I taught a **Conservation Biology** class by videoconference jointly at VUW and University of Montana. This course was part of collaborative program in Conservation Biology funded by a grant to myself and Charles Daugherty (VUW) from the Council for International Exchange of Scholars Fulbright Alumni Initiative Program.

I co-taught **Applied Conservation Genetics** at the National Conservation Training Center (NCTC) of the US Fish & Wildlife Service almost annually for 10 years. In addition, I have taught a similar course at several universities throughout the world (University of Western Australia, Victoria University of Wellington (New Zealand), University of Minnesota, and the University of Oregon). I also taught **Conservation Genetics Workshops** in Thailand in 2004 and 2006, and at the National Zoological Gardens of South Africa and the South African Institute for Aquatic Biodiversity in 2012 and 2016.

I co-taught **Conservation Genetics** at the Organization for Tropical Studies, Las Cruces Biological Station, Costa Rica, May-June 2014. I also co-taught a short-course in **Conservation Genetics** at the Universidad Nacional, San Jose, Costa Rica in November 2017.

I have taught the following graduate courses at the Hatfield Marine Science Center of Oregon State University since 2011:

Genetics of Exploited Populations (FW 599) Co-taught with Kathleen O'Malley
Genetic Monitoring (FW 599) Co-taught with Scott Baker and Sue Haig

GRADUATE STUDENTS DIRECTED

The success of my former graduate students has been my most gratifying achievement. Nine of them currently have tenure-track faculty positions in the US, Canada, and New Zealand. Eight of them currently hold research positions with agencies or as post-docs. Eight of my MS students went on to work in research positions for management agencies.

Mitchell, N. J. 1977. Genetic variation in populations of *Peromyscus maniculatus* in northwestern Montana. M. A.

Wishard (now Seeb), L. N. 1977. Larval growth in *Rana pretiosa*: Ecological and genetic factors. M.A. (co-director)
O'Malley, D. 1977. Inheritance of isozyme variation and heterozygosity in ponderosa pine. M. A., Botany.
Daugherty, C.H. 1979. Population ecology and genetics of *Ascaphus truei*: An examination of gene flow and natural selection. Ph.D. (co-director).
Phelps, S.R. 1980. Genetic population structure of the kokanee, *Oncorhynchus nerka*, in Flathead Lake, Montana. M. A.
Woods, J.H. 1982. Amount and distribution of isozyme variation in ponderosa pine from eastern Montana. M. S., Forestry. (co-director).
Aronson, M.E. 1985. Effect of the *Pgm1-t* regulatory gene on liver metabolism in rainbow trout. M. A.
Danzmann, R.G. 1986. Biochemical genetics of developmental rates of rainbow trout. Ph. D.
Ferguson, M.M. 1986. Gene regulation and developmental divergence in salmonid fishes. Ph. D.
Leary, R.F. 1986. Genetic control of meristic variation in salmonid fishes. Ph. D.
Everett, R.J. 1986. The population genetics of Arctic grayling (*Thymallus arcticus*) of Montana. M. A.
Forbes, S.H. 1990. Mitochondrial and nuclear genotypes in trout hybrid swarms: tests for gametic equilibrium and effects on phenotypes. Ph.D.
Gellman, W.A. 1991. Sex linkage of two isozyme loci in rainbow trout. M.A.
Sage, G.K. 1993. Population genetic analysis of westslope cutthroat trout in the Bob Marshall Wilderness. M. A.
Chadde, S. 1994. Hatchery supplementation of declining populations: Fitness and role in conservation. M. A.
Newman, D. 1996. Importance of genetic factors on fecundity and survival of small populations. Ph. D.
Luikart, G. 1992. Conservation genetics and mtDNA variation in bighorn sheep. M. A.
1997. Usefulness of molecular markers for detecting population bottlenecks and monitoring genetic change. Ph. D.
Kanda, N. 1998. Genetic population structure and conservation of bull trout (*Salvelinus confluentus*). Ph.D.
Pilgrim, K. 1999. Identification of the sex-determining locus in pink salmon. M.A.
Thelen, G. 1999. Heterozygosity and fitness in rainbow trout: Marker loci or chromosomal segments? M.A.
Smithwick, P. 2000. Development of nuclear DNA markers to detect hybridization between cutthroat and rainbow trout. M.A.
Tallmon, D. 1995. Genetics, metapopulation structure, and conservation of salmonid fishes. M.A.
2001. Ecological and genetic effects of forest fragmentation on California red-backed voles. Ph.D. (co-director)
Anderson, L.P. 2002. Population genetics and conservation of the freshwater mussel *Margaritifera falcate* from the northwestern United States. M.A.
Hitt, N. 2002. Introgressive hybridization between westslope cutthroat trout and rainbow trout: the role of limiting factors in the Flathead River system, Montana. M.A. (co-director)
Funk, W.C. 2004. Ph.D. Patterns and consequences of dispersal in Columbia spotted frogs (*Rana luteiventris*). (co-director).
Whiteley, A.R. 2005. Ph.D. Genetic and morphological diversity in the mountain whitefish, *Prosopium williamsoni*. (co-director)
Hastings, K. Ph.D. 2005. Long-term persistence of isolated fish populations in the Alexander Archipelago. (co-director)
Ramstad, K. 2006. Ph.D. Colonization and local adaptation of sockeye salmon (*Oncorhynchus nerka*) in Lake Clark, Alaska.
Boyer, M. M.S. 2006. Rainbow trout invasion and the spread of hybridization with native westslope cutthroat trout.
Gardipee, F. MA. 2007. Development of fecal DNA sampling methods to assess genetic population structure of Greater Yellowstone Bison. (co-director)
Pierson, J. Ph.D. 2010. Genetic population structure and dispersal of two North American woodpeckers in ephemeral habitats. (co-director)
Short Bull, R. MS. 2011. The importance of replication in landscape genetics: the American black bear in the Rocky Mountains. (co-director)
Bingham, D. MS. 2011. Conservation genetics of sauger in the Upper Missouri River drainage.
O'Brien, M. MS. 2012. Brucellosis transmission between wildlife and livestock in the Greater Yellowstone Ecosystem: Inferences from DNA genotyping. (co-director)

Tucker, Jody. Ph.D. 2013. Assessing changes in connectivity and abundance through time for fisher in the southern Sierra Nevada. (co-director)

Kardos, Marty. Ph.D. 2013. The genetic basis of fitness: detecting inbreeding depression and selective sweeps in bighorn sheep. (co-director).

Taylor, Helen. Ph.D. 2014. Victoria University of Wellington. Ecology and genomics in a recovering species: a study of little spotted kiwi (*Apteryx owenii*). (co-director)

Addis, Brett. MS. 2013. Genetic structure and disease prevalence among boreal toads (*Bufo boreas boreas*) in Glacier National Park. (co-director)
Ph.D. The role of selection in maintaining variation in dispersal distance in the stream salamander *Gyrinophilus porphyriticus*. (co-director)

POST-DOCTORAL RESEARCHERS DIRECTED

Paul Spruell, 1995-2000, Conservation genetics of bull trout

Eleanor Steinberg, 1999-2001, NSF Postdoctoral Research Fellowship in Biological Informatics. Genomics of pink salmon.

John Wenburg, 2000-2001, Hybridization and conservation of trout species

Megan McPhee, 2006-2008, Sex chromosome evolution and life history variation in rainbow trout. (Co-director)

Sebastien Paquette, 2008-2011, Victoria University of Wellington, Use of genomics to identify important stocks of marine fish. (Co-director)

Kristina Ramstad, 2010-2014, Victoria University of Wellington, Conservation genomics of little spotted kiwi and rowi. (Co-director)

Morten Limborg, 2012-2014. Genomics of anadromy in salmonid fishes. University of Washington. (Member of supervisory committee)

GROUP LEADERSHIP

Mike Schwartz and I co-directed a Working Group entitled "Genetic monitoring: Optimal design and development of tools for data analysis" that was jointly funded by the National Center for Ecological Analysis and Synthesis (NCEAS) and the National Center for Evolutionary Synthesis (NESCent). Members included C. Scott Baker (USA), David Gregovich (USA), Michael M. Hansen (Denmark), Jennifer Jackson (UK), Kate Kendall (USA), Linda Laikre (Sweden), Kevin McKelvey (USA), Maile Neel (USA), Isabelle Olivieri (France), Nils Ryman (Sweden), Ruth Short Bull USA), Jeff Stetz USA), Dave Tallmon (USA), Christina Vojta (USA), Don Waller (USA), Robin Waples (USA).

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Interviewed (Evolution and the Future) by Kim Hill on New Zealand National Radio on 7 May 2011. http://www.radionz.co.nz/national/programmes/saturday/audio/2488696/fred-allendorf-evolution-and-the-future
Interviewed (Genetics and Conservation Biology) on What Now by Ken Rose (KOWS, Occidental, CA) on 19 December 2011.
Interviewed (Unnatural Selection) by Adam Hart on BBC4 Radio Science Feature on 8 February 2016.

EDITED COLLECTIONS

Editor for special issue of the Journal of Heredity (Aug-Sep 1998) containing papers resulting from American Genetics Association Presidential Symposium (Conservation and Genetics of Marine Organisms).
Editor for Special Section of Conservation Biology (Feb 2003; Population Biology of Invasive Species). These papers were presented in a symposium at the annual meeting of the Society for Conservation Biology in Hilo, Hawaii, 2001.

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Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Curriculum Vitae for Reviewer 2

Dr. Charles (Carlos) Carroll

Curriculum Vitae

Carlos Carroll

Personal Information

Address: Klamath Center for Conservation Research, PO Box 104, Orleans, CA 95556
E-mail: carlos@klamathconservation.org

Professional Qualifications

2000 Ph.D., Forest Science Oregon State University, Corvallis, Oregon

1997 M.S., Wildlife Science Oregon State University, Corvallis, Oregon

1994 B.A., Biology University of California Santa Cruz, Santa Cruz, California

Employment History

2004-present	Conservation Science Advisor, Wilburforce Foundation, Seattle, WA
1999-present	Director, Klamath Center for Conservation Research, Orleans, CA
1997-1999	Research Ecologist, Conservation Biology Institute, Corvallis, OR
1995-1997	Ecologist, USDA Forest Service, Pacific Southwest Experiment Station, Redwood Sciences Laboratory, Arcata, CA

Professional Contributions and Distinctions

Member-at-Large, Society for Conservation Biology (Global) Board of Governors, 2011-2014

President, Society for Conservation Biology North America, 2014-2017

Treasurer, Society for Conservation Biology North America, 2017-present

Publications

Refereed Articles

Carroll, C., Parks, S.A., Dobrowski, S.Z. and Roberts, D.R., 2018. Climatic, topographic, and anthropogenic factors determine connectivity between current and future climate analogs in North America. Global Change Biology Early View.

Belote, R.T., Carroll, C., Martinuzzi, S., Michalak, J., Williams, J.W., Williamson, M.A. and Aplet, G.H., 2018. Assessing agreement among alternative climate change projections to inform conservation recommendations in the contiguous United States. Scientific Reports 8:9441.

Stralberg, D., Carroll, C., Pedlar, J.H., Wilsey, C.B., McKenney, D.W. and Nielsen, S.E., 2018. Macrorefugia for North American trees and songbirds: Climatic limiting factors and multi-scale topographic influences. Global Ecology and Biogeography 27:690-703.

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Carroll, C., B. Hartl, G.T. Goldman, D.J. Rohlf, A. Treves, J.T. Kerr, E.G. Ritchie, R.T. Kingsford, K.E. Gibbs, M. Maron, J.E.M Watson. 2017. Defending the scientific integrity of conservation-policy processes. Conservation Biology 31:967-975.

Littlefield, C. E., B. H. McRae, J. Michalak, J. J. Lawler, C. Carroll. 2017. Connecting today's climates to future analogs to facilitate species movement under climate change. Conservation Biology 31:1397-1408.

Belote, R. T., M. S. Dietz, P. S. McKinley, A. A. Carlson, C. Carroll, C. N. Jenkins, D. L. Urban, T. J. Fullman, J. C. Leppi, G. H. Aplet. 2017. Mapping Conservation Strategies under a Changing Climate. Bioscience 67: 494-497.

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Carroll, C. 2014. Can a conservation-oriented scientific society remain relevant in the 21st century?. Conservation Biology 28:1137-1138.

Rohlf, D. J., C. Carroll, B. Hartl. 2014. Conservation-reliant species: Toward a biology-based definition. Bioscience 64:601-611.

Schumaker, N. H., A. Brookes, J. R. Dunk, B. Woodbridge, J. A. Heinrichs, J. J. Lawler, C. Carroll, D. LaPlante. 2014. Mapping sources, sinks, and connectivity using a simulation model of northern spotted owls. Landscape Ecology 29:579-592.

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Carroll, C., and D. Miquelle. Spatial viability analysis of Amur tiger Panthera tigris altaica in the Russian Far East: the role of protected areas and landscape matrix in population persistence. Journal of Applied Ecology 43:1056-68.

Zielinski, W. J., C. Carroll, and J. Dunk. 2006. Using landscape suitability models to reconcile conservation planning for two key forest predators. Biological Conservation 133:409-430.

Carroll, C., R. Rodriguez, C. McCarthy, and K. Paulin. 2006. Resource selection function models as tools for regional conservation planning for Northern Goshawk in Utah. Studies in Avian Biology 31:288-298.

Carroll, C. M. K. Phillips, C. A. Lopez-Gonzalez, and N. H. Schumaker. 2006. Defining recovery goals and strategies for endangered species: the wolf as a case study. Bioscience 56:25-37.

Zielinski, W. J, R. L. Truex, F. V. Schlexer, L. A. Campbell, and C. Carroll. 2005. Historical and contemporary distributions of carnivores in forest of the Sierra Nevada, California, U.S.A. Journal of Biogeography 32:1385-1407.

Carroll, C., R. F. Noss, P. C. Paquet and N. H. Schumaker. 2004. Extinction debt of protected areas in developing landscapes. Conservation Biology 18:1110-1120.

Carroll, C., R. F. Noss, P. C. Paquet, and N. H. Schumaker. 2003. Use of population viability analysis and reserve selection algorithms in regional conservation plans. Ecological Applications 13:1773-1789.

Carroll, C., M. K. Phillips, N. H. Schumaker, and D. W. Smith. 2003. Impacts of landscape change on wolf restoration success: planning a reintroduction program based on static and dynamic spatial models. Conservation Biology 17:536-548.

Noss, R. F., C. Carroll, K. Vance-Borland, and G. Wuerthner. 2002. A multicriteria assessment of the irreplaceability and vulnerability of sites in the Greater Yellowstone Ecosystem. Conservation Biology 16:895-908.

Carroll, C., R. F. Noss, and P. C. Paquet. 2001. Carnivores as focal species for conservation planning in the Rocky Mountain region. Ecological Applications 11:961-980.

Zielinski, W. J., K. M. Slauson, C. R. Carroll, C. J. Kent, and D. G. Kudrna. 2001. Status of American martens in coastal forests of the Pacific states. Journal of Mammalogy 82:478-490.

Carroll, C., W. J. Zielinski, and R. F. Noss. 1999. Using survey data to build and test spatial habitat models for the fisher (Martes pennanti) in the Klamath region, U.S.A. Conservation Biology 13:1344-1359.

Noss, R. F., J. R. Strittholt, K. Vance-Borland, C. Carroll, and P. Frost. 1999. A conservation plan for the Klamath-Siskiyou ecoregion. Natural Areas Journal 19:392-411.

Book Chapters

Carroll, C., W. D. Spencer, and J. C. Lewis. 2012. Use of Habitat and Viability Models in Martes Conservation and Restoration. Pages 429-450 in K. Aubry, ed. Biology and Conservation of Martens, Sables, and Fishers: A New Synthesis. Cornell University Press, Ithaca, NY.

Carroll, C. 2006. Linking connectivity to viability: insights from spatially-explicit population models of large carnivores. Pages 369-389 in K. Crooks and M. A. Sanjayan, eds. Connectivity Conservation. Cambridge (UK): Cambridge University Press.

Carroll, C., R. F. Noss, N. H. Schumaker, and P. C. Paquet. 2001. Is the return of the wolf, wolverine, and grizzly bear to Oregon and California biologically feasible?. Pages 25-46 in D. Maehr, R. F. Noss, and J. Larkin, editors. Large Mammal Restoration: Ecological and Sociological Challenges in the 21st Century. Washington, DC. Island Press.

Cooperrider, A., R. F. Noss, H. H. Welsh, Jr., C. Carroll, W. Zielinski, D. Olson, S. K. Nelson, and B.G. Marcot. 2000. Terrestrial fauna of redwood forests. Pages 119-163 in R. F. Noss, editor, The redwood forest: history, ecology, and conservation of the coast redwoods. Island Press, Covelo, California.

Computer Software

Carroll, C. 2011. Connectivity Analysis Toolkit. Klamath Center for Conservation Research, Orleans, CA.

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Curriculum Vitae for Reviewer 3

Adrian P. Wydeven

Curriculum vitae:

Adrian P. Wydeven

Timber Wolf Alliance, Chair of Advisory Council
Certified Wildlife Biologist

Cell phone 715-580-0616; email adrianwydeven@cheqnet.net

Education:

M.S. Iowa State University, Ames, IA, Wildlife Ecology, May 1979
Thesis: Food habits and ecological relationships of elk to other herbivores in Wind Cave National Park

B.S. University of Wisconsin-Stevens Point, Stevens Point, WI May 1976
Biology and Wildlife Management

Positions

2017- present Retired, but provide consulting services to Timber Wolf Alliance, NPS and other organizations. Conduct volunteer wildlife surveys for WDNR

2015-2017 Timber Wolf Alliance Coordinator with the Sigurd Olson Environmental Institute, at Northland College, Ashland, WI

2013-2015 Forest Wildlife Habitat Specialist/ Wildlife Biologist with Wisconsin Department of Natural Resources, Ashland, WI

1993-2013 Mammalian Ecologist/ Conservation Biologist & director of Wisconsin wolf management program, WI DNR Park Falls, WI

1990-1993 Nongame Biologist and director of Wisconsin wolf recovery program, Wisconsin Department of Natural Resources, Park Falls, WI

1982-1990 Wildlife Manager, Wisconsin Department of Natural Resources, in Oshkosh ('82-'83), Appleton ('83-'84), and Shawano ('84-'90)

1980-1982 Assistant Wildlife Area Manager, Missouri Department of Conservation, Bowling Green, MO

1979-1980 Part-time positions in wildlife management for the U.S. Fish and Wildlife Service in Nebraska, Missouri DOC in SW MO, and Iowa Conservation Department in Boone, IA

Between 1990 and 2013 I headed up the Wisconsin wolf recovery program, which has included intense monitoring of the state wolf population, educating people about wolves, working with many different researchers on wolves, assisting graduate students and serving on committees, chairing a state wolf science advisory committee, working closely with wolf depredation management, and interacting with wolf specialists across North America and Europe. I also have worked closely with other endangered, rare and nongame mammals in the state including bats, American marten, cougars, Canada lynx, moose, and small mammals, including having chaired committees on bat and American marten conservation. In my last position with WDNR I worked to promote, enhance and protect forest habitat for wildlife species across the State of Wisconsin.

In September 2015 I started a position with the Timber Wolf Alliance at Northland College in Ashland to promote wolf conservation through education and training programs. I stepped down from this position in June 2017, but continue to consult and advise with Timber Wolf Alliance, and serve as the chair of the advisory council to TWA.

Professional Activities:

Member Wisconsin Green Fire, promoting science-based management of Wisconsin natural resources 2017-present
Wisconsin Master Naturalists, 2017 to present
Member of team informing USFWS on Mexican wolf recovery 2015-2016
Certified Wildlife Biologist with The Wildlife Society since 1993.
Board of Directors of the Cable Natural History Museum 2006-2011
Executive Board, Wis. Chap. Wildlife Society, 1988-1990
Chair of the Wisconsin wolf advisory and science committee, 1992-2013
Member Timber Wolf Alliance advisory council 1990-present
Member of Loon Watch, Sigurd Olson Environmental .Inst. 1991-1995
Member USFWS Eastern Gray Wolf recovery team 1997-present
Sigurd Olson Environmental Institute, advisory board 1997-2003
Chair of Wisconsin Bat Advisory Committee 1995-1997
Chair of Wisconsin Marten Advisory Committee 2000-2006
President, Wis. Chap. The Wildlife Society 2004-2005
Organizer of symposium on Recovery of Gray Wolves in Great Lakes 2005
Member of Midwest Wolf Stewards since 1992
Past member WI DNR elk and furbearer advisory committees
State of Wisconsin licensed hunting guide

Teaching Activities:

Lecturer for wildlife classes, and public workshops at Northland College, ongoing
Guest speaker on wolves, cougars and other wildlife in WI and other states, ongoing
Adjunct instructor for Northland College (WI) and Central Michigan University
Honorary Associate with University of Wisconsin-Stevens Point
Guest lector for universities and colleges in Wisconsin, Michigan, Vermont, New Hampshire, Maine, and Sweden
Wolf ecology classes for Timber Wolf Alliance Pigeon Lake station 1992-2008, North Lakelands Discovery Center 2009-2012, Kemp Station 2016, and Forest Lodge 2017-2018.
Mammal tracking classes in 2 -4 classes each fall since 1995

Graduate Student Committees:

A. Thomas Honeyager, 1992. PhD Geography, U.WI-Milwaukee
Alexia Sabor, 1998. MS Conservation Biology, U. WI-Madison
Joanne Finnell, 2000, MS. Environmental Science, U. WI.-Green Bay
Jason A. Hawley, 2005, MS. Wildlife Biology, C. MI Univ.
Shawn T. Rossler, 2007, MS. Wildlife Biology, C. MI Univ.

Elizabeth A. Berkley, 2008, MS, Wildlife Ecology, U. WI-Madison
Christine Anhalt, 2011, MS, Wildlife Ecology, U.WI-Madison
Jennifer Stenglein, 2014, PhD, Wildlife Ecology, U WI-Madison
Erik Olson, 2013, PhD, Conservation Biology, U WI-Madison

Awards:

Best Paper in Discipline of Landscape Ecology in 1997, from International Association of Landscape Ecology, with David Mladenoff, Theodore Sickley, and Robert Haight for "A Regional Landscape Analysis and Prediction of Favorable Gray Wolf Habitat in Northern Great Lakes Region". Conservation Biology 9: 279-294.

Outstanding Alumni in 2006 in the College of Natural Resources, University of Wisconsin-Stevens Point;

Cooperative Conservation Award, for Science in Support of Recovery of Western Great Lakes Wolves with 9 others from the U.S. Department of the Interior in May 9, 2007.

Secretary's Pride Award, for WI DNR, Resource Sustained Achievement Award, honorable mention, May 11, 2011.

Wisconsin Award, Wisconsin Chapter of The Wildlife Society, March 13, 2013.

Secretary's Pride Award for WI DNR, Resource Sustained Achievement Award, May 6, 2013.

Partner Award from Wisconsin USDA-APHIS Wildlife Services, June 5, 2013

Jim McDonough Award from The Wildlife Society at annual conference in Milwaukee, WI, October 6, 2013.

US Fish and Wildlife Services, Endangered Species Recovery Champion, May 19, 2014.

Recent Publications

Olson, E.K. and A.P. Wydeven. 2018. Wolf population goals for Wisconsin: opinions of The Wisconsin Chapter of The Wildlife Society. Timber Wolf Alliance, Technical Report, Sigurd Olson Environmental Institute, Northland College, Ashland, Wisconsin, USA.
https://www.northland.edu/news/wisconsin-wildlife-professionals-prefer-higher-wolf-goal/

Stenglein, J.L., A.P. Wydeven, and T. R. Van Deelen. 2018. Compensatory mortality in a recovering top carnivore: wolves in Wisconsin, USA (1979-2013). Oecologia

187:99-111.

Olson, E.R., S.M. Crimmins, D.E. Beyer Jr., D.R. MacNulty, B.R. Patterson, B.A. Rudolph, A.P. Wydeven, and T. Van Deelen. 2017. Flawed analysis and unconvincing interpretation: a comment on Chapron and Treves 2016. Proceeding Royale Society B 284: 20170273. http://dx.doi.org/10.1098/rspb.2017.2073 http://rspb.royalsocietypublishing.org/content/royprsb/284/1867/20170273.full.pdf

Hohenlohe, P.A., L.Y. Rutledge, L.P. Waite, K.R. Andrews, J.R. Adams, J. W. Hinton, R.M. Nowak, B.R. Patterson, A.P. Wydeven, P.A. Wilson, and B. N. White. 2017. Comment on "Whole genome sequence analysis of coyote and gray wolf". Science Advances 3: doi: 10.1126/sciadv.1602250 http://webpages.uidaho.edu/hohenlohe/Hohenloheetal2017.pdf

Jara, R.F., A.P. Wydeven, and M. D. Samuel. 2016. Gray wolf exposure to emerging vector-borne diseases in Wisconsin with comparison to domestic dogs and humans. PLOS/One 17pp, DOI:10.1371/journal.prone,0165836

Hawley, J.E., P.W. Rego, A.P. Wydeven, M.K. Schwartz, T.C. Viner, R. Kays, K.L. Pilgrim, and J.A. Jenks. 2016. Long-distance dispersal of subadult male cougar from South Dakota to Connecticut documented with DNA evidence. Journal of Mammalogy 97:1428-1434.

Wydeven, A.P. 2016. Book Review, The Real Wolf: Science, Politics and Economics of Co-Existing with Wolves in Modern Times. Journal of Wildlife Management 80:1334-1335.

Smith, D.W., P.J. White, D. R. Stahler, A. Wydeven, and D.E. Hallac. 2016. Managing wolves in the Yellowstone area: balancing goals across jurisdictional boundaries. The Wildlife Society Bulletin 40: 436-445.

Wydeven, A.P. and E. R. Olson. 2015. Swings in management challenges wolf conservation in Wisconsin. International Wolf Magazine. Fall 2015, 25 (3):4-7.

Stenglein, J.L., T.R. Van Deelen, A.P. Wydeven, D.J. Mladenoff, J.E. Wiedenhoeft, N.K. Businga, J.A. Langenberg, N.J. Thomas, and D.M. Heisey. 2015. Mortality patterns and detection bias from carcass data: An example from wolf recovery in Wisconsin. Journal of Wildlife Management. 77: 1173-1184.

Stenglein, J.L., J.H. Gilbert, A.P. Wydeven, and T.R. Van Deelen. 2014. An individual-based model for southern Lake Superior wolves: A tool to explore the effect of human-caused mortality on a landscape of risk. Ecological Modeling 302: 13-24.

Olson, E.R., A. Treves, A.P. Wydeven, and S.J. Ventura. 2014. Landscape predictors of wolf attacks on bear-hunting dogs in Wisconsin,USA. Wildlife Research 41: 584-597.

http://www.publish.csiro.au/paper/WR14043.htmhttp:/www.publish.csiro.au/paper/WR14043.htm2014

Olson, E.K., J.L. Stenglein, V. Shelly, A.R. Rissman, C. Browne-Nunez, Z. Voyles, A.P. Wydeven, and T.R. Van Deelen. 2014. Pendulum swings in wolf management led to conflict, illegal kills, and legislated wolf hunt. Conservation Letters. 1-10.

Anhalt, C.M., T.R. VanDeelen, R.N. Schultz, and A.P. Wydeven. 2014. Effectiveness of a simulated pack to manipulate wolf movements. Human-Wildlife Interactions. 8 (2): 38-45.

Wydeven, A.P. 2013. Wolf 475 of the dog killer pack in Wisconsin. Pp. 104-112 in R.P. Thiel, A.C. Thiel, and M. Strozewski, editors. Wildlife Wolves We Have Known: Stories of Wolf Biologists' Favorite Wolves, International Wolf Center, Minneapolis, MN, USA 246pp.

Bouchard, K., J.E. Wiedenhoeft, A.P. Wydeven, and T.P. Rooney. 2013. Wolves Facilitate the recovery of browse sensitive understory herbs in Wisconsin Forests. Boreal Environment Research 18 (Suppl. A): 43-49.

Callan, R., N.P. Nibblelink, T.P. Rooney, J.E. Wiedenhoeft, and A.P. Wydeven. 2013. Recolonizing wolves trigger a trophic cascade in Wisconsin (USA). Journal of Ecology 101: 837-845.

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Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Curriculum Vitae for Reviewer 4

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Adrian Treves

Formal education

1991–1997 Ph.D. Harvard University, Human Evolutionary Biology
1987–1990 B.A. Rice University, Houston, TX, double major Biology & Anthropology

Professional Positions held since Ph.D.

2017– Professor, Nelson Institute for Environmental Studies, University of Wisconsin–Madison (2011–2017 Associate Professor, 2007–2011 Assistant Professor)
2006–2007 Senior Administrative Program Specialist, Office of the Director, Nelson Institute for Environmental Studies, University of Wisconsin–Madison
2005–2007 Executive Director, COEX: Sharing the Land with Wildlife, Madison, WI
2005–2006 Visiting Assistant Professor Conservation Biology Program, Makerere University, Kampala, Uganda,
2003–2005 Extension Coordinator, Wildlife Conservation Society, Bronx, NY
2000–2003 Research Fellow, Conservation International, Center for Applied Biodiversity Science, Washington, D.C.
1997-2000 Post-doctoral Research Associate and Lecturer, Department of Psychology, University of Wisconsin–Madison
1999–2000 Post-doctoral Lecturer, Department of Zoology, University of Wisconsin–Madison

Honors and awards

2004–2018 Keynote speaker for 12 meetings
2017 Winner of the Clements Prize for Outstanding Research & Education
2018 Nominated for the Indianapolis Prize for Conservation
2015–2017 Honored Instructor selected by students 3 years running http://www.housing.wisc.edu/residencehalls-academics-honoredinstructors.htm#acc-panel-2535
2010 Award for Best monitoring & evaluation methods, Rainforest Alliance Eco-Index
1987–1990 National Merit Scholar

Papers published in refereed journals

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superscripts: undergraduate (u), graduate (g), or post-doctoral (p) mentees as co-authors

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82. Treves, A., Palmqvist, P. 2007. Reconstructing hominin interactions with mammalian carnivores (6.0 - 1.8 Ma). In Primate Anti-Predator Strategies. Eds. Nekaris, K.A.I., Gursky, S.L. Springer, New York. p. 355-381.

83. Treves, A., Naughton-Treves, L. 2005. Evaluating lethal control in the management of human-wildlife conflict. In People and Wildlife, Conflict or Coexistence? Eds. Woodroffe, R., Thirgood, S., Rabinowitz, A. Cambridge University Press, Cambridge. p. 86-106.

84. Treves, A., Brandon, K. 2005. Tourist impacts on the behavior of black howler monkeys (Alouatta pigra) at Lamanai, Belize. In Commensalism and Conflict: The primate-human interface. Eds. Paterson, J. Wallis, J. American Society of Primatology, Norman, OK, p. 146-167.

85. Treves, A., Pizzagalli, D. 2002. Vigilance and perception of social stimuli: Views from ethology and social neuroscience. In The Cognitive Animal: Empirical and Theoretical Perspectives on Animal Cognition. Eds. Bekoff, M., Allen, C. Burghardt, G. MIT Press, Cambridge, MA. p. 463-469.

86. Treves, A. 2002. Predicting predation risk for foraging, arboreal monkeys. In Eat or Be Eaten: Predator Sensitive Foraging in Nonhuman Primates. Ed. Miller, L.A. Cambridge University Press: Cambridge. p. 222-241.

87. Treves, A., Baguma, P. 2002. Interindividual proximity and surveillance of associates in comparative perspective. In The Guenons: Diversity and Adaptation in African Monkeys. Eds. Glenn, M.E., Cords, M. Kluwer Academic Publishers, NY, p. 157-168.

88. Treves, A. 2001. Baboons. p.82-84; Carnivores. p. 165-169; Infanticide. p.856-858. In Magill's Encyclopedia of Science: Animal Life. ed. C.W. Hoagstrom. Pasadena, CA: Salem Press.

89. Treves, A. 2000. Prevention of infanticide: The perspective of infant primates. In Infanticide by males and its implications. Eds. van Schaik, C., Janson, C. Cambridge University Press. p. 223-238.

Book chapters co-authored (peer-reviewed)

superscripts: undergraduate (u), graduate (g), or post-doctoral (p) mentees as co-authors

90. Ohrens, O.[g], Santiago-Ávila, F.J.[g], Treves, A. 2018. The twin challenges of preventing real and perceived threats to livestock. Pages xx-yy In <u>Human-Wildlife Interactions: Turning Conflict into Coexistence</u>. eds. Frank, B. Marchini, S., Glikman, J. Cambridge University Press: Cambridge.

91. Santiago-Ávila, F.J.[g], Lynn, W.S., Treves, A. 2018. Inappropriate consideration of animal interests in predator management: Towards a comprehensive moral code. Chap. 12, In <u>Large Carnivore Conservation and Management: Human Dimensions and Governance</u>. ed. T. Hovardos, Routledge, New York.

92. Naughton-Treves, L, L'Roe, J., L'Roe, A. and A. Treves. 2018. Changes in elephants, bushpigs, farmers, and fears: Comparing local perceptions of crop raiding at Kibale National Park, 1994 and 2012. pp. xx-xx. in Human-Wildlife Conflict: an Interdisciplinary Approach. K. Hill et al. (Eds.) Berghahn Books, London.

93. Bruskotter J.T., Treves A., Way J.G. 2014. Carnivore Management. pp. 83-90 in B.S. Steel editor. Science and Politics: An A-To-Z Guide to Issues and Controversies. CQ Press, Thousand Oaks, CA, USA.

94. Zimmermann, A., Baker, N., Inskip, C., Linnell, J.D.C., Marchini, S., Odden, J., Rasmussen, G., Treves, A. 2010. Contemporary views of human-carnivore conflicts on wild rangelands. in Wild Rangelands: Conserving Wildlife While Maintaining Livestock in Semi-Arid Ecosystems. Eds. du Toit, J., Kock, R., Deutsch, J.C. Wiley-Blackwell, London. p.129-151.

95. Sillero-Zubiri, C., Sukumar, R., Treves, A. 2007. Living with wildlife: the roots of conflict and the solutions. In Key Topics in Conservation Biology. Eds. MacDonald, D., Service, K. Oxford, Oxford University Press, p. 266-272.

96. Miller, L.E., Treves, A. 2007 edition 1 and 2010 edition 2. Predation on Primates: Past Studies, Current Challenges, and Directions for the Future. In Primates in Perspective. Eds. Campbell, C.J., Fuentes, A., MacKinnon, K.C., Panger, M., Bearder, S.K. Oxford University Press, NY. p. 525-542.

97. Naughton-Treves, L., Treves, A. 2005. Socioecological factors shaping local tolerance of crop loss to wildlife in Africa. In People and Wildlife, Conflict or Coexistence? Eds. Woodroffe, R., Thirgood, S., Rabinowitz, A. Cambridge University Press, Cambridge. p. 253-277.

98. Wydeven, A.P., Treves, A., Brost, B. [u], Wiedenhoeft, J. 2004. Characteristics of wolf packs in Wisconsin: Identification of traits influencing depredation. In People and Predators: from Conflict to Coexistence. Eds. Fascione, N., Delach, A., Smith, M. Island Press, Washington, DC. p. 28-50.

99. Boinski, S., Treves, A., Chapman, C.A. 2000. A critical evaluation of the influence of predators on primates: Effects on group movement. In On the Move: How and Why Animals Travel in Groups. Eds. Boinski, S., Garber, P. U. Chicago Press, p. 43-72.

100. Naughton-Treves, L., Rose, R., Treves, A. 2000. Social and spatial dimensions of human-elephant conflict in Africa: Literature review and two case studies from Uganda & Cameroon. IUCN African Elephant Specialist Group, Gland, Switzerland.

Non-peer-reviewed scientific writing

superscripts: undergraduate or post-bacc. (u), graduate (g), or post-doctoral (p) as co-authors

101. LTE Chicago Tribune August 12, 2017 http://www.chicagotribune.com/news/opinion/letters/ct-protect-gray-wolves-so-our-children-can-see-them-20170811-story.html
102. Artelle KA, Reynolds JD, A. T, Walsh JC, C. PP, Darimont CT. 2018. Distinguishing science from “fact by assertion” in natural resource management. **Science Advances eLetter** 4:eaao0167.
103. Treves, A. 2016. Wolf delisting decision not based on the facts. **The Register-Guard**, Eugene, Oregon, 15 February.
104. Treves, A., M. Krofel, and J. V. Lopez-Bao. 2016. Missing wolves, misguided policy. **Science eLetter** 350:1473-1475.
105. Krofel, M., Treves, A., Ripple, W. J., Chapron, G., López-Bao. J.V. 2015. “Super-predator” humans: Integrating evolutionary perspectives into large carnivore management. **Science** (Letter) 350 (6260): 518.
106. Naughton, L., Treves, A. 2015. Chimpanzee: Caught between two worlds. in: No More Endings: Saving Species One Story at a Time. (ed. A. Hegan) http://www.allisonhegan.com/book-description.html
107. Treves, A., Bergstrom, B., Parsons, D., Paquet, P. Thiel, R.P. 2014. Letter to the USFWS describing concerns about use of the best available science in the State of Wisconsin’s post-delisting monitoring report on gray wolves. 15 August 2014 at http://faculty.nelson.wisc.edu/treves/.
108. Vucetich, J. A., J. T. Bruskotter, R. O. Peterson, A. Treves, T. Van Deelen, and A. M. Cornman. 2013. Evaluating the scientific soundness of plans for harvesting wolves to manage depredations in Michigan. Little River Band of Ottawa Indians Natural Resources Report No. 2013-3.
109. Schloegel, C., Jones, T.[g], Zug, B.[g], Achig, L., Treves, A. 2011. Don Oso Program Develops Participatory Monitoring Protocol for Andean Bears in Southern Sangay National Park, Ecuador. **International Bear News** 20(2): 23-25. (Editorial review only)
110. Zapata, J., R. Wallace, A. Treves, A. Morales 2011. Guía de acciones para el manejo de conflictos entre humanos y animales silvestres en Bolivia. WCS y SERNAP, La Paz.
111. Jones, T.[g], Treves, A. 2010. Identificación de individuos y probabilidad de detección del oso andino usando métodos de trampas cámara. Report to the Ministry of the Environment, Quito, Ecuador.
112. Treves, A., Schloegel, C. 2010. Monitoring and enforcing payment for ecosystem services programs: Lessons learned. **Land Tenure Brief** 14.
113. Jones, T.[g,] Zug, B.[g], Treves, A. 2010. Credible conservation: Using biodiversity monitoring to support incentive programs that protect endangered wildlife. **Land Tenure Brief** 13.
114. Treves, A., Jones S.M.[g] 2009. Market financing for biodiversity conservation and strategic tradeoffs for wildlife-friendly eco-labels. **Land Tenure Brief** 10.
115. Treves, A. 2009. Open season or open debate? **International Wolf Center Magazine**, Spring 2009 19:1.

116. Treves, A. 2007. Balancing the Needs of People and Wildlife: When Wildlife Damage Crops and Prey on Livestock. **Land Tenure Brief** 7.
117. Treves, A., Naughton-Treves, L., Wydeven, A., Schanning, K. 2007. Public Opinion of Wolf Management in Wisconsin 2003-2005. Appendix H2, Wolf Management Plan Revision, Wisconsin Department of Natural Resources, Madison, WI.
118. Treves, A. 2004. Vigilance. p. 62-65 in The Encyclopedia of Animal Behavior, Vol. 2. Ed. M. Bekoff, Greenwood Press, Portsmouth, NH.
119. Bruner, A., Naughton-Treves, L., Gullison, T., Treves, A., Saenz, M., Brandon, K., Rice, R. 2004. Land use, zoning and management costs in Ecuador's forest protected areas. Center for Applied Biodiversity Science, Washington, D.C.
120. Treves, A. 2004. Human and Ecological Dimensions of a Conservation Controversy: Book review of Knight (2003) Waiting for Wolves in Japan. Oxford University Press, Oxford. **Conservation Biology** 18(4): 1664-1165. (Editorial review only)
121. Treves, A., Karanth, K.U. 2003. Human-carnivore conflict: Local solutions with global applications. **Conservation Biology** 17(6): 1489-1490.
122. Treves, A. 2003. Modeling vigilance remains unrealistic. **Behavioural Processes** 63(3): 137-138. (Editorial review only)
123. Treves, A. 2002. Wolf justice: managing human-carnivore conflict in the 21st century. **Wolf Print** 12:6-9.
124. Naughton-Treves, L., Treves, A. 2002. Wolves in Dairyland. **Wildlife Conservation** 105(1): 10.
125. Treves, A., Naughton-Treves, L. 1997. Caught between two worlds. **Wildlife Conservation** August 1997, 100(4): 52-55.
126. Treves, A. 1997. Self-protection in Primates. PhD dissertation, Harvard University.
127. Treves, A., Naughton-Treves, L. 1996. Case study of a captive chimpanzee temporarily released with wild conspecifics. In **Proceedings of the Chimpanzoo Conference**, Jane Goodall Institute, West Palm Beach, FL, September 1994.

Keynote Oral Presentations

1. U.S. Senate Briefing hosted by Senators Cory Booker (D-NJ) and Tom Carper (D-DE) "Effective Non-Lethal Methods for Protecting Livestock from Predators". Invited Keynote Panelist on "Latest research", U.S. Senate Capitol Visitor Center, Washington, D.C., March 6, 2018
2. Antioch University New England, Center for Tropical Ecology & Conservation(keynote), "Rethinking biodiversity preservation and conservation conflicts", A. Treves, 12th Annual Symposium, Antioch, NH, 15 April 2017.
3. Public Interest Environmental Law Conference (Keynote Panel) "Wolves and the public trust", "Predators and the Public Trust", M.C. Wood and A. Treves, Eugene, OR, 4 March 2016.
4. Human-wildlife conflict and coexistence at DICE, University of Kent (keynote), A. Treves, "Predators and the public trust", Canterbury, U.K., 26 May 2016.
5. Wolf Symposium NABU (keynote), A. Treves, "Predators and the Public Trust", Wolfsburg, Germany, September 2015.
6. Brookfield Zoo Wilderness Coalition (keynote) "Predators, public trust, predicting and preventing poaching and predation on property", A. Treves, Chicago, IL, 25 September 2014.

7. Democracy, science, and Advocacy: Wolf and Wildlife Coexistence Conference, Ho Chunk Nation, Friends of the Wisconsin Wolf and Wildlife, Nelson Institute for Environmental Studies (keynote) "Predators and the Public Trust" and "The role of science in the Public Trust". A. Treves, Ho Chunk Casino, WI, 14-15 July 2015.
8. Universidad de Azuay, Cuenca, Ecuador, (keynote in Spanish) "Balancing human needs with carnivore conservation", A. Treves, 10 May 2010.
9. Landowner workshop, Zhoray, Ecuador (keynote in Spanish) "Understanding and managing human-wildlife conflicts", A. Treves, 12 August 2007.
10. Wildlife Conservation Society Conference on Biodiversity Policy in Bolivia (keynote in Spanish) "National policy on interventions to mitigate human-wildlife conflicts", A. Treves, 5 May 2006.
11. Wildlife Conservation Society, Bolivia workshop on Conservation Planning (keynote in Spanish) "Intervenciones for conflicts enters humans y vida silvestre", A. Treves, 10 January 2005.
12. Wildlife Conservation Society, Albertine Rift Program workshop on Conservation Planning (keynote in French and English) "Landscape species", A. Treves, 10 March 2004.

+59 invited presentations available upon request (6 selected below)

13. Eastern Wolf Education Summit A. Treves, "Predators and the Public Trust", Mount Kisco, NY, 1 June 2016.

14. The Humane Society Institute for Science & Policy, "Living Large: Wolves, Bears, Cougars and Humans in North America" The Kellogg Conference Center at Gallaudet University, Washington, DC October 11-13, 2015.

15. Northeast wolf coalition, A. Treves, Benefits and costs of carnivores, 16 November 2014, Concord, MA.

16. Pacific Northwest Wolf Coalition, A. Treves, "Kill to conserve carnivores?" University of Washington, Seattle, WA, 29 October 2014.

17. Large Carnivore Working Group Meeting, A. Treves, Research Update,Yellowstone National Park, WY, 2-3 October, 2014.

18. Michigan Natural Resources Commission, Treves, A., Cornman, A., Mitchell, J., Rabenhorst, M.F., Slomin, M., Vucetich, J. Evaluating the effects of MIDNR lethal control of gray wolves in the Upper Peninsula, Detroit, MI, 7 July 2014.

19. Yale University Large Carnivore Symposium, Treves, A.,Naughton, L., Wolf conservation conflicts in Wisconsin, New Haven, CT, October 2013.

28. Wisconsin Conservation Congress Wolf Committee, Predicting high-risk sites for wolf attack on livestock and bear-hunting dogs, A. Treves, E.R. Olson, Stevens Point, WI September 2011

+61 Contributed Public Presentations at Conferences available upon request

+18 Outreach presentations for practitioners and the public, in three languages including two keynotes and talks on conservation planning (all invited) available upon request

Invited presentations on pedagogy

1. University of Wisconsin–Madison Teaching & Learning Symposium "Active learning in the Lakeshore Nature Preserve", A. Treves, 18 May 2017, Madison, WI.

2. Office of Professional Instructor Development UW System, (poster) Henke, J., Martin, B., Treves, A. Introducing UW-TEACH: Teaching, Exploration and Collaboration Habitat University of Wisconsin–Madison, March 2016, Eau Claire, WI.
3. Teaching Academy Winter Retreat, Treves, A., Martin, E. (co-organizers) RELATE: Rethinking Effective Learning and Teaching Engagement. Why Does the Scholarship of Teaching and Learning Matter? January 2015, Madison WI.
4. University of Wisconsin–Madison Teaching & Learning Symposium Panel "Writing Global Learning Outcomes for your Science Course", M. Van Eyck, L. Van Toll, A. Treves, C. Allen, 23-24 May 2012, Madison WI.
5. University of Arizona–Tucson "Interventions to mitigate human-wildlife conflicts", A. Treves, October 3, 2011, Tucson, AZ.
6. UW–Madison, DoIT ENGAGE Faculty Advisory Group, "Wolf Sim and Risk maps", A. Treves, May 2, 2011, Madison, WI.
7. North Carolina State University, "Teaching and training in human dimensions of fish and wildlife", A. Treves, November 2006, Raleigh, NC.

Teaching

Leadership

2013–2015 Faculty co-chair of the University of Wisconsin–Madison Teaching Academy
2010– Fellow of the University of Wisconsin–Madison Teaching Academy
2013– UCLASS, UW-TEACH, PFoT (see https://teachingacademy.wisc.edu/)

Classroom Teaching

Course name, enrollment (+graduate students), semesters
Total of 30 semesters at university level plus field courses in three countries

1. **Preserving Nature**, 35, Summer 2018–
2. **Introductory Ecology**.120-200, Fall 2011–2018 (except sabbatical 2014)
3. **Wolves, dogs and people** (First year interest group), 11–20, Fall 2015–2017
4. **Conserving Biodiversity** (online), 36–74 (including grads), Spring & Summer 2014–2018
5. **Community Environmental Scholars Program seminar** (co-taught 10% in 2015), 20, Fall 2013–Spring 2017
6. **Large carnivore conservation** (ES400), 8–14, Spring 2008–2014
7. **Conservation Biology**, 28–64 (including grads), Spring 2009–2014
8. **Environmental Planning and adaptive management**, 7 (+4 grads), Fall 2007
9. **Environmental planning & monitoring**, 7, Fall 2005, (Makerere University),
10. **Ethology**, 125 (+grads) Fall 2000
11. **Animal Biology** (co-taught 33%), 811, Spring 2000
12. **Ethology**, 118 (+grads) Fall 1999
13. **Psychometric Methods**, 55 Spring 1999
14. **Animal Behavior: The Primates**, 118 (+grads) Spring 1998

SERVICE

Please see Press and other outreach activities at http://faculty.nelson.wisc.edu/treves/

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Curriculum Vitae for Reviewer 5

Dr. Daniel R. MacNulty

Daniel R. MacNulty

Department of Wildland Resources	435-797-7442
Utah State University	dan.macnulty@usu.edu
5230 Old Main Hill	MacNulty Animal Ecology Lab
Logan, Utah, 84322	

Positions held

2017-present	Associate Professor, Department of Wildland Resources, Utah State University
2011-2017	Assistant Professor, Department of Wildland Resources, Utah State University
2010-2011	Research Associate, Department of Ecology, Evolution, and Behavior, University of Minnesota
2007-2010	Post-doctoral Research Scientist, School of Forest Resources and Environmental Science, Michigan Technological University

Education

2007	Ph.D. in Ecology, Evolution, and Behavior, University of Minnesota
2002	M.S. in Wildlife Conservation, minor in Statistics, University of Minnesota
1995	B.A. in Environmental Studies, minor in Biology, University of Colorado

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Grants Total: $2,261,718 (Total external grants to USU: $834,636; Total internal: $40,000)

2018-2019	*National Geographic Society*. Understanding the fate of wolves, caribou, and muskox in Canada's warming high Arctic. $50,000 (USU: $50,000). **D.R. MacNulty (Sole PI)**
2016-2019	*Utah Division of Wildlife Resources.* Population ecology of Utah moose, Phase II. $421,518 (USU: $160,518), **D.R. MacNulty (Sole PI)**
2015-2020	*Utah State University, Office of Research and Graduate Studies, PhD Research Assistantship Award*. $40,000, **D.R. MacNulty (Sole PI)**
2014-2018	*National Park Service-Cooperative Ecosystem Studies Unit*. Population estimation of northern Yellowstone elk. $100,000 (USU: $100,000), **D.R. MacNulty (Sole PI)**
2014	*BBC Natural History Unit*. Behavior and ecology of High Arctic wolves. $7,769 (USU: $7,769), **D.R. MacNulty (Sole PI)**
2013-2016	*Utah Division of Wildlife Resources.* Population ecology of Utah moose, Phase I. $504,000 (USU: $208,500), **D.R. MacNulty (Sole PI)**
2012-2017	*National Science Foundation*. LTREB: Yellowstone wolves their ecology and community consequences. $450,000 (USU: $301,211), **D.R. MacNulty (Lead PI)**, T. Coulson, D.W. Smith, D.R. Stahler, R.O. Peterson, J.A. Vucetich.

2012-2015 *Natural Environment Research Council (United Kingdom).* Predicting the population consequences of environmental change. $550,000, T. Coulson, **D.R. MacNulty (co-PI)**, D.R. Stahler, D.W. Smith.

2010-2012 *National Science Foundation*. The genomic and ecological context of a major gene under selection in natural populations. $31,000 (USU: $6,638), Collaborator. Robert K. Wayne (Lead PI).

2010 *National Geographic Society.* Establishing an investigation of climate change impacts on wolf-prey dynamics in the High Arctic. $15,000, **D.R. MacNulty (Lead PI),** L.D. Mech, H.D. Cluff.

2010 *Yellowstone Park Foundation*. Influence of age structure on wolf-elk dynamics. $18,000, **D.R. MacNulty (Sole PI)**

2004-2006 *Canon, USA.* Efficacy of an internet camera system for remotely monitoring free-ranging mammals at large spatial and temporal scales. $146,000, **D.R. MacNulty (Lead PI)**, C. Packer.

2004 *University of Wyoming National Park Service Research Center.* Effects of wolf predation risk on bison foraging behavior. $4,500, **D.R. MacNulty (Lead PI)**, D.W. Smith

2003 *Yellowstone Park Foundation.* A behavioral analysis of the effect of predator and prey densities on wolf predation. $14,000, **D.R. MacNulty (Lead PI)**, C. Packer

2003 *Twin Spruce Foundation*. Ecology of fear in a wolf-bison system. $4,000, **D.R. MacNulty (Sole PI)**

2003 *Wolf Recovery Foundation*. Wolf-bison interactions in Yellowstone National Park. $2,500, **D.R. MacNulty (Sole PI)**

2002 *Wolf Recovery Foundation*. Ecology of fear in a wolf-bison system. $1,200, **D.R. MacNulty (Sole PI)**

Publications

* = graduate advisee † = graduate advisee or post-doc in collaborator's lab
** = undergraduate advisee †† = undergraduate advisee in collaborator's lab

<u>Refereed Journal Articles</u>

2018 Metz, M.C., D.J. Emlen, D.R. Stahler, **D.R. MacNulty**¸D.W. Smith, M. Hebblewhite. Predation shapes the evolutionary traits of cervid weapons. *Nature Ecology & Evolution*, https://doi.org/10.1038/s41559-018-0657-5

2018 Kohl, M.T.*, D.R. Stahler, M.C. Metz, J.D. Forester, M.J. Kauffman, N. Varley, P.J. White, D.W. Smith, **D.R. MacNulty**. Diel predator activity drives a dynamic landscape of fear. *Ecological Monographs,* doi.org/10.1002/ecm.1313

2018 Martin, H., L.D. Mech, J. Fieberg, M. Metz, **D.R. MacNulty**, D.R. Stahler, D.W. Smith. Factors affecting gray wolf (*Canis lupus*) encounter rate of elk (*Cervus elaphus*) in

Yellowstone National Park. *Canadian Journal of Zoology*, doi.org/10.1139/cjz-2017-0220

2017 Olson, E.R., S.M. Crimms, D.E. Beyer, **D.R. MacNulty**, B.R. Patterson, B.A. Rudolph, A.P. Wydeven, T. R. Van Deelen. Flawed analysis and unconvincing interpretation: a comment on Chapron and Treves 2016. *Proceedings of the Royal Society B: Biological Sciences* 284:20170273

2017 Tallian, A.*, D.W. Smith, D.R. Stahler, M.C. Metz, R.L. Wallen, C. Geremia, J. Ruprecht*, C.T. Wyman, **D.R. MacNulty**. Predator foraging response to a resurgent dangerous prey. *Functional Ecology* 31:1418-1429.

2017 Tallian, A.*, A. Ordiz, M.C. Metz, C. Milleret, C. Wikenros, D.W. Smith, D.R. Stahler, **D.R. MacNulty**, P. Wabakken, J.E. Swenson, H. Sand. Competition between apex predators? Brown bears decrease wolf kill rate on two continents. *Proceedings of the Royal Society B: Biological Sciences* 284:20162368

2017 Cassidy, K.A.†, L.D. Mech, **D.R. MacNulty**, D.R. Stahler, D.W. Smith. Sexually dimorphic aggression indicates male gray wolves specialize in pack defense from conspecific groups. *Behavioral Processes* 136:64-72.

2016 Ruprecht, J.S.*, K.R. Hersey, K. Hafen**, K.L. Monteith, N.J. DeCesare, M.J. Kauffman, **D.R. MacNulty**. Reproduction in moose at their southern range limit. *Journal of Mammalogy* 97:1355-1365.

2015 Cassidy, K.†, **D.R. MacNulty**, D.R. Stahler, D.W. Smith, L.D. Mech. Group composition effects on aggressive inter-pack interactions of gray wolves in Yellowstone National Park. *Behavioral Ecology* 26:1352-1360.

2014 **MacNulty, D.R.**, A. Tallian*****, D.R. Stahler, D.W. Smith. Influence of group size on the success of wolves hunting bison. *PLoS ONE* 9(11): e112884.

2014 Cubaynes, S.†, **D.R. MacNulty**, D.R. Stahler, K.A. Quimby, D.W. Smith, T. Coulson. Influence of density, prey availability, and social aggression on survival of Yellowstone wolves. *Journal of Animal Ecology* 83:1344-1356.

2013 Stahler, D.R.†, **D.R. MacNulty**, R.K. Wayne, B. vonHoldt, D.W. Smith. The adaptive value of morphological, behavioral and life-history traits in reproductive female wolves. *Journal of Animal Ecology* 82:222-234.

2013 Packer, C., A. Swanson, S. Canney, A. Loveridge, S. Garnett, M. Pfeifer, A.C. Burton, H. Bauer, **D.R. MacNulty.** The case for fencing remains intact. *Ecology Letters* 16:1414-e4.

2013 Packer, C., A. Loveridge, S. Canney, T. Caro, S.T. Garnett, M. Pfeifer, K.K. Zander, A. Swanson, **D.R. MacNulty**, et al. Conserving large carnivores: dollars and fence. *Ecology Letters* 16:635-641.

2012 **MacNulty, D.R.**, D.W. Smith, L.D. Mech, J.A. Vucetich, C. Packer**.** Nonlinear effects of group size on the success of wolves hunting elk. *Behavioral Ecology* 23:75-82.

2012 MacCormick, H.A.††, **D.R. MacNulty**, A. Bailey, A. Bosacker, D. A. Collins, C. Packer. Male and female aggression: Lessons from sex, rank, age, and injury in olive baboons. *Behavioral Ecology* 23:684-691.

2011 Coulson, T., **D.R. MacNulty**, D.R. Stahler, B. vonHoldt, R.K. Wayne, D.W. Smith. Modeling effects of environmental change on wolf population dynamics, trait evolution and life history. *Science* 334:1275-1278.

2011 Troyer, J.L., M.E. Roelke, J.M. Jespersen, N. Baggett, V. Buckley-Beason, **D.R. MacNulty**, M. Craft, C. Packer, J. Pecon-Slattery, S.J. O'Brian. FIV diversity: FIV_{Ple} subtype composition may influence disease outcome in African lions. *Veterinary Immunology and Immunopathology* 143:338-346.

2010 Madden, J.D.††, R.C. Arkin, **D.R. MacNulty**. Multi-robot system based on model of wolf hunting behavior to emulate wolf and elk interactions. In: *Proceedings 2010 IEEE International Conference on Robotics and Biomimetics*, Tianjin, China, pp. 1043-1050.

2009 **MacNulty, D.R.,** D.W. Smith, J.A. Vucetich, L.D. Mech, D.R. Stahler, C. Packer Predatory senescence in aging wolves. *Ecology Letters* 12:1347-1356. (JIF: 14.94; Times cited: 56)

2009 **MacNulty, D.R.,** D.W. Smith, L.D. Mech, L.E. Eberly. Body size and predatory performance in wolves: Is bigger better? *Journal of Animal Ecology* 78:532-539.

2008 **MacNulty, D.R.**, G.E. Plumb, D.W. Smith. Validation of a new video and telemetry system for remotely monitoring wildlife. *Journal of Wildlife Management* 72:1834-1844.

2007 **MacNulty, D.R.,** L.D. Mech, D.W. Smith. A proposed ethogram of large carnivore predatory behavior, exemplified by the wolf. *Journal of Mammalogy* 88:595-605.

2007 Kauffman, M.J., N. Varley, D.W. Smith, D. Stahler, **D.R. MacNulty**, M.S. Boyce Landscape heterogeneity shapes predation in a newly restored predator-prey system. *Ecology Letters* 10:690-700.

2001 Mech, L.D., D.W. Smith, K.M. Murphy, **D.R. MacNulty**. Winter severity and wolf predation on a formerly wolf-free elk herd. *Journal of Wildlife Management* 65:998-1003.

2001 **MacNulty, D.R.**, N. Varley, D.W. Smith. Grizzly Bear, *Ursus arctos*, usurps Bison, *Bison bison*, captured by Wolves, *Canis lupus*, in Yellowstone National Park, Wyoming. *Canadian Field-Naturalist* 115:495-498.

<u>Refereed Books</u>

2015 Mech, L.D., D.W. Smith, **D.R. MacNulty**. Wolves on the hunt: The behavior of wolves hunting wild prey. *University of Chicago Press*, Chicago, Illinois.

Reviews:

Middleton, A. 2016. *The Quarterly Review of Biology* 91:366.

Musiani, M. 2016. *Ecology* 97:1368-1369.

Lake, B. 2015. *Arctic* 68:516-517.

Way, J. 2015. *Canadian-Field Naturalist* 129:297-299.

Donoghue, S. 2015. *Open Letters Monthly*

Organ, J. 2015. *CHOICE-Association of College and Research Libraries*

Wilkinson, T. 2015. *Jackson Hole News & Guide*

Peer-Reviewed Articles

These articles appear in a special issue of the National Park Service publication *Yellowstone Science* commemorating the 20th anniversary of wolf reintroduction to Yellowstone National Park. I served as guest editor and articles were reviewed by at least two NPS scientists.

2016 Stahler, D.R., D.W. Smith, **D.R. MacNulty.** Motherhood of the wolf. Yellowstone Science 24(1):13-16.

2016 **MacNulty, D.R.**, D.R. Stahler, C.T. Wyman, J. Ruprecht*, D.W. Smith. The challenge of understanding Northern Yellowstone elk dynamics after wolf reintroduction. Yellowstone Science 24(1):25-33.

2016 **MacNulty, D.R.**, D.R. Stahler, D.W. Smith. Understanding the limits to wolf hunting ability. Yellowstone Science 24(1):34-36.

2016 Cassidy, K.†, D.W. Smith, L.D. Mech, **D.R. MacNulty**, D.R. Stahler, M.C. Metz. Territoriality and interpack aggression in gray wolves: Shaping a social carnivore's life history. Yellowstone Science 24(1):37-41.

2016 Smith, D.W., R.O. Peterson, **D.R. MacNulty**, M. Kohl***.** The big scientific debate: Trophic cascades. Yellowstone Science 24(1):70-71.

2016 Smith, D.W., T. Wyman, D.R. Stahler, **D.R. MacNulty**. Pelican valley and Mollie's pack. Yellowstone Science 24(1):85-86.

Invited Book Review

2015 **MacNulty, D.R.** A democratic approach to large carnivore conservation. **Book review of** Susan G. Clark and Murray B. Rutherford, editors. 2014. Large carnivore conservation: Integrating science and policy in the North American West. The University of Chicago Press. 407 p. *Ecology* 96:1155-1156.

Letters

2016 **MacNulty, D.R.** How certain is the reduction in wolf population growth rate in years with legalized killing? *Proceedings of the Royale Society B: Biological Sciences*. eLetter: http://rspb.royalsocietypublishing.org/content/283/1830/20152939.e-letters

2014 Pfeifer, M., C. Packer, A.C. Burton, S.T. Garnett, A.J. Loveridge, **D.R. MacNulty**, P.J. Platts. In defense of fences. *Science* 345:389

Agency Reports

2018 Anderson, M., **D. MacNulty**, H.D. Cluff, L.D. Mech. High arctic wolf ecology field report, summer 2017. *Wildlife Research Reports*. Department of Environment, Government of Nunavut, Canada

2017 Anderson, M., **D. MacNulty**, H.D. Cluff, L.D. Mech. High arctic wolf ecology field report, summer 2016. *Wildlife Research Reports*. Department of Environment, Government of Nunavut, Canada.

2016 Anderson, M., **D. MacNulty**, H.D. Cluff, L.D. Mech. High arctic wolf ecology field report, summer 2015. *Wildlife Research Reports*. Department of Environment, Government of Nunavut, Canada.

2015 Anderson, M., **D. MacNulty**, H.D. Cluff, L.D. Mech. High arctic wolf ecology field report, summer 2014. *Wildlife Research Reports*. Department of Environment, Government of Nunavut, Canada.

2017 Robertson, S.*, **D. MacNulty**, K. Hersey. Determinants of population growth in Utah moose. *Biennial Report*. Utah Division of Wildlife Resources, Salt Lake City, Utah.

2015 Ruprecht, J.*, **D. MacNulty**, K. Hersey. Determinants of population growth in Utah moose. *Annual Report*. Utah Division of Wildlife Resources, Salt Lake City, Utah.

2014 Ruprecht, J.*, **D. MacNulty**, K. Hersey. Determinants of population growth in Utah moose. *Annual Report*. Utah Division of Wildlife Resources, Salt Lake City, Utah.

2013 Ruprecht, J.*, **D. MacNulty**, K. Hersey. Determinants of population growth in Utah moose. *Annual Report*. Utah Division of Wildlife Resources, Salt Lake City, Utah.

2004 **MacNulty, D.**, D. Smith. Bison foraging response to the risk of wolf predation in a spatially heterogeneous winter environment: A preliminary assessment. *University of Wyoming National Park Service Research Center Annual Report.* Vol. 28, Article 2.

Publications in progress

<u>Refereed Journal Articles in Review or Revision</u>

Cusack, J.J.†, M.T. Kohl*, T. Coulson, D.R. Stahler, D.W. Smith, **D.R. MacNulty**. Limited evidence for proactive and reactive spatial responses of prey to an active predator. bioRxiv: https://doi.org/10.1101/215475

Kohl, M.T. *, T.K. Ruth, D.R. Stahler, M.C. Metz, D.W. Smith, P.J. White, **D.R. MacNulty**. Selection for predator niches vacancies minimizes a multi-predator threat.

Hoy, S.†, **D.R. MacNulty**, D.W. Smith, D.R. Stahler, X Lambin, R.O. Peterson, J.S. Ruprecht, J.A., Vucetich. Fluctuations in age structure and their variable influence on population growth.

Ruprecht, J.S.*, D.N. Koons, K.R. Hersey, N.T. Hobbs, D.R. MacNulty. The effect of climate on population growth in a cold-adapted ungulate at its equatorial range limit.

<u>Refereed Journal Articles in Preparation</u>

MacNulty, D.R., R. Kindermann*, M.K. Kohl*, D.R. Stahler, D.W. Smith, P.J. White, J.O. Sexton, J.R. Nagol, M.J. Kauffman, D.E. McWhirter A. Middleton. Simpson's paradox reverses an apparent non-consumptive effect of a large carnivore

Smith, L.M. *, D.N. Koons, S. Hoy, D.W. Smith, D.R. Stahler, P.J. White, **D.R. MacNulty.** Selective predation mediates the strength of additive predator-caused mortality.

Tallian, A.*, D.W. Smith, D.R. Stahler, R.L. Wallen, C. Geremia, C.T. Wyman, **D.R. MacNulty.** Bison spatial response to wolf predation risk in an extreme winter environment.

Refereed Book in Preparation

Smith, D.W., D.R. Stahler, **D.R. MacNulty**, editors. Yellowstone wolves: Reintroduction, ecology, behavior, and conservation. *University of Chicago Press,* Chicago, Illinois.

Presentations

Invited oral presentations

2017 **MacNulty, D.R.**, M.K. Kohl, L.M. Smith, A. Tallian, S. Hoy, M.C. Metz, D.R. Stahler, D.W. Smith. Understanding the predatory effects of wolves in northern Yellowstone National Park. Symposia: Has wolf reintroduction to Yellowstone provided evidence for a trophic cascade? *Ecological Society of America* – 102nd Annual meeting, Portland, OR.

2017 **MacNulty, D.R.**, Timid predators, scary prey: why wolves are wimpier than you think think. *Science Unwrapped*. *Utah State University*, Logan, UT.

2017 **MacNulty, D.R.**, Yellowstone wolves: reintroduction and ecology. *Utah Society for Environmental Education* – Annual meeting, Logan Utah.

2017 **MacNulty, D.R.** Yellowstone wolves and the forces that structure a wildland food web. *Department of Wildland Resources, Utah State University*, Logan, UT.

2015 **MacNulty, D.R.** Elk ecology and elk-wolf dynamics in northern Yellowstone. Revisiting Brucellosis in the Greater Yellowstone Area. *National Academy of Sciences*, Bozeman, MT.

2015 **MacNulty, D.R.** Understanding the fate of northern Yellowstone elk in the wolf era. *Wyoming Governor's Brucellosis Coordination Team* – Annual Meeting, Lander, WY.

2015 **MacNulty, D.R**. Hunting behavior of wild wolves. *Stokes Nature Center*, Logan, UT.

2014 **MacNulty, D.R.** Population ecology of moose in Utah. *Swaner Ecocenter*, Park City, UT.

2013 **MacNulty, D.R.** Understanding the predatory power of wolves. *Swaner Ecocenter*, Park City, UT.

2013 **MacNulty, D.R.** Understanding the ecological consequences of wolf reintroduction in Yellowstone National Park. *Intermountain Society of American Foresters* – Annual Meeting, Logan, UT

2012 **MacNulty, D.R.** Behavioral ecology of wolf predation in Yellowstone National Park. *Conservation Ecology Seminar Series, School of Natural Resources and Environment, University of Michigan*, Ann Arbor, MI.

2012 **MacNulty, D.R.** The ecology of predator-prey interactions. Ecology Center *Interdisciplinary Lunch, Utah State University*, Logan, UT.

2012 **MacNulty, D.R.** Research overview and new initiatives. *USGS Utah Cooperative Fish and Wildlife Research Unit Coordinating Meeting*, Utah State University, Logan, UT.

2011 **MacNulty, D.R.** Group hunting behavior of wolves: myths and realities. *College of Natural Resources, Utah State University*, Logan, UT.

2011 **MacNulty, D.R.** Controls on predation in an expanding predator-prey system. *Department of Wildland Resources, Utah State University*, Logan, UT.

2011 **MacNulty, D.R.** Controls on predation in an expanding predator-prey system. *Department of Biology and Wildlife, University of Alaska*, Fairbanks, AK.

2010 **MacNulty, D.R.** Predatory senescence in aging wolves. *Department of Biology, Boise State University*, Boise, ID.

2010 **MacNulty, D.R.** Wolves as a model system for cooperative robots? *College of Computing, Georgia Institute of Technology*, Atlanta, GA.

2006 **MacNulty, D.R.** Efficacy of an internet camera system for remotely monitoring free-ranging mammals at large spatial and temporal scales. *Yellowstone Center for Resources*, Mammoth Hot Springs, WY.

2006 **MacNulty, D.R.** Wolf-bison interactions in Yellowstone National Park. *International Wolf Center*, Minneapolis, MN.

2004 **MacNulty, D.R.** Wolves, bears, and bison: a case study in ecological restoration. *Midwest Wolf Stewards Conference*, Wisconsin Department of Natural Resources, Odanah, WI.

2003 **MacNulty, D.R.** Food for safety trade-offs in a behaviorally responsive predator, *Canis lupus*. *Department of Environmental Science & Policy*, University of California, Davis, CA.

2002 **MacNulty, D.R.** Why natural history matters: an ecological perspective. *Department of Biology*, Northland College, Ashland, WI.

2002 **MacNulty, D.R.** Behavior of wolves hunting elk and bison in Yellowstone National Park. *Sigurd Olson Environmental Institute, Northland College*, Ashland, WI.

Conference oral presentations and abstracts

2018 Robertson, S.D.*, K.R. Hersey, **D.R. MacNulty**. The impacts of winters ticks on Utah Moose. *52nd North American Moose Conference and Workshop*, Spokane, WA.

2018 Robertson, S.D.*, K.R. Hersey, **D.R. MacNulty**. The impacts of winters ticks on Utah Moose. *Utah Chapter of the Wildlife Society* – Annual Meeting, Vernal, UT. *Awarded best student presentation*.

2018 Hallerud, M.A.**, D.C. Stoner, **D.R. MacNulty**. Three years of cougar monitoring using non-invasive methods in the Bear River Mountains. *Utah Chapter of the Wildlife Society*, Vernal, UT

2017 Robertson, S.D.*, K.R. Hersey, **D.R. MacNulty**. Population ecology of Utah moose. *51st North American Moose Conference and Workshop*, Cape Breton, Nova Scotia.

2017 Kohl, M.T.*, T.K. Ruth, D.R. Stahler, M.C. Metz, D.W. Smith, **D.R. MacNulty**. Elk selection for vacant predator niches favors coexistence with wolves and cougars in northern Yellowstone National Park. *Ecological Society of America* – 102nd Annual meeting, Portland, OR. *Awarded the Buell Prize for best student presentation.*

2017 Smith, L.M.*, D.N. Koons, D.W. Smith, D.R. Stahler, P.J. White, and **D.R. MacNulty**. Survival expectations in a wolf-elk system: how selective predation and the environment shift elk senescence. *Ecological Society of America* – 102nd Annual meeting, Portland, OR.

2017 Hoy, S.R.†, **D.R. MacNulty**, D.W. Smith, D.R. Stahler, M.C. Metz, R.O. Peterson, J.A. Vucetich. Gray wolves (*Canis lupus*) continue to selectively kill senescent elk (*Cervus elaphus*), even when they are relatively rare in the population. *Ecological Society of America* – 102nd Annual meeting, Portland, OR.

2017 Smith, D.W., **D.R. MacNulty**, D.R. Stahler, M.C. Metz. Carnivore recovery and Yellowstone's 'unnatural' history. Symposia: Has wolf reintroduction to Yellowstone provided evidence for a trophic cascade? *Ecological Society of America* – 102nd Annual meeting, Portland, OR.

2017 Stahler, D.R., D.W. Smith, R.K. Wayne, and **D.R. MacNulty.** From the gene to ecosystem: surprising ecological and behavioral implications of the black coat color gene revealed by 20 years of the Yellowstone Wolf Project. *Ecological Society of America* – 102nd Annual meeting, Portland, OR.

2017 Smith, L.M.*, D.N. Koons, D.W. Smith, D.R. Stahler, P.J. White, and **D.R. MacNulty**. Survival expectations in a wolf-elk system: how selective predation and the environment shift elk senescence. 12th *International Mammalogical Congress*, Perth, Australia

2017 Kohl, M.T.*, T.K. Ruth, D.R. Stahler, M.C. Metz, D.W. Smith, **D.R. MacNulty**. Elk selection for vacant predator niches favors coexistence with wolves and cougars in northern Yellowstone National Park. *The Wildlife Society* – 24th Annual meeting, Albuquerque, NM.

2016 Tallian, A.*, A. Ordiz, M.C. Metz, C. Milleret, C. Wikenros, D.W. Smith, D.R. Stahler, **D.R. MacNulty**, P. Wabakken, J.E. Swenson, H. Sand. Competition between apex predators? Brown bears decrease wolf kill rate on two continents. *International Conference on Bear Research and Management* – 24th Annual meeting, Anchorage, AK. Awarded best student oral presentation.

2016 Metz, M.C., D.W. Smith, M. Hebblewhite, D.R. Stahler, **D.R. MacNulty**, R.L. Wallen, C. Geremia. Temporal variation in wolf predation dynamics in the multi-prey system of northern Yellowstone. *13th Biennial Scientific Conference on the Greater Yellowstone Ecosystem,* Mammoth Hot Springs, WY.

2015 Cassidy, K.†, L.D. Mech, D.R. Stahler, **D.R. MacNulty**, D.W. Smith. Behavioral sexual dimorphism of Yellowstone gray wolves during aggressive inter-pack interactions. *Animal Behavior Society* – 52nd Annual meeting, Anchorage, AK.

2015 Kohl. M.T.*, **D.R. MacNulty**, D.W. Smith, D.R. Stahler, M.C. Metz, J.D. Forester, M.J. Kauffman. Diel activity pattern of wolves shapes elk response to spatial predation risk in northern Yellowstone. *Ecological Society of America* – 100th Annual meeting, Baltimore, MD.

2015 Ruprecht, J.*, **D.R. MacNulty**, K. Hersey. Modeling historic aerial count data to determine factors influencing population growth of moose in Utah. *49th North American Moose Conference and Workshop*, Middle Park, CO.

2015 Tallian, A.*, **D.R. MacNulty**, D.W. Smith, D.R. Stahler. The role of bison sex and group size in wolf-bison interactions. *European Congress of Mammalogy* – 7th Annual meeting, Stockholm, Sweden.

2014 **MacNulty, D.R.**, Kindermann, R.K.*, D.W. Smith, D.R. Stahler, P.J. White. Selective predation mitigates the impact of wolves on survival of northern Yellowstone elk. *Ecological Society of America* – 99th Annual meeting, Sacramento, CA.

2014 Tallian, A.*, **D.R. MacNulty,** D.R. Stahler, D.W. Smith. Group size effects of a dangerous prey species on predator attack decision and capture success. *Ecological Society of America* – 99th Annual meeting, Sacramento, CA.

2014 Ruprecht, J.*, **D.R. MacNulty**, K. Hersey. Determinants of population growth in Utah moose. *Utah Chapter of The Wildlife Society* – Annual meeting, Saint George, UT.

2014 Ruprecht, J.*, **D.R. MacNulty**, K. Hersey. Body condition and reproduction in moose at their southern range limit. *48th North American Moose Conference and Workshop*, Girdwood, AK.

2014 Quimby, K.†, S. Cubaynes†, **D.R. MacNulty**, D.R. Stahler, D. Smith. Intraspecific aggression affects vital rates and competitive ability in gray wolves (*Canis lupus*) of Yellowstone National Park. *12th Biennial Scientific Conference on the Greater Yellowstone Ecosystem*, Mammoth Hot Springs, WY.

2014 Metz, M.C., D.W. Smith, D.R. Stahler, **D.R. MacNulty**. Assessing the additive and compensatory nature of wolf predation in the multi-prey system of Yellowstone National Park. *12th Biennial Scientific Conference on the Greater Yellowstone Ecosystem*, Mammoth Hot Springs, WY.

2014 Smith, D., K.A. Quimby, D.R. Stahler, M. Metz, E. Stahler, R.T. McIntyre, **D.R. MacNulty**. Age structure and pack composition of an unexploited wolf population in Yellowstone: managing for naturalness and maximizing connectivity. *12th Biennial Scientific Conference on the Greater Yellowstone Ecosystem,* Mammoth Hot Springs, WY.

2014 Metz, M.C., D.W. Smith, D.R. Stahler, **D.R. MacNulty**. Assessing the additive and compensatory nature of wolf predation in the multi-prey system of Yellowstone National Park. *2nd North America Congress for Conservation Biology*, Missoula, MT.

2014 Smith, D.W., D.R. Stahler, M.C. Metz, E. Stahler, R. McIntyre, **D.R. MacNulty**. Age structure and pack composition of an unexploited wolf population in Yellowstone: managing for 'naturalness' in a national park. *2nd North America Congress for Conservation Biology*, Missoula, MT.

2013 **MacNulty, D.R.**, D.W. Smith, D.R. Stahler, R.O. Peterson, J.A. Vucetich. Mechanisms of population regulation in wolves and their community consequences. *Ecological Society of America* – 98th Annual meeting, Minneapolis, MN.

2013 **MacNulty, D.R.** Understanding the limits to wolf predatory power. *International Wolf Symposium*, Duluth, MN

2013 **MacNulty, D.R.**, Stahler, D.R. , D.W. Smith, B. vonHoldt, R.K. Wayne. Reproductive consequences of life history traits, morphology, pack composition, and environmental conditions for female wolves. *International Wolf Symposium*, Duluth, MN

2013 Kohl, M.T.*, **D.R. MacNulty**, J.D. Forester, M.J. Kauffman, D.W. Smith, and D.R. Stahler. Influence of wolf predation risk on elk movement in Yellowstone National Park. *The Wildlife Society* – 20th Annual meeting, Milwaukee, WI.

2013 Kohl, M.T.*, **D.R. MacNulty**, J.D. Forester, M.J. Kauffman, D.W. Smith, D.R. Stahler. Elk spatial response to wolf predation in Yellowstone National Park. *International Wolf Symposium*, Duluth, MN.

2013 Quimby, K.†, **D.R. MacNulty**, D.R. Stahler, D. Smith, L.D. Mech. Group composition effects on inter-pack aggressive interactions of Yellowstone gray wolves. *International Wolf Symposium*, Duluth, MN.

2010 **MacNulty, D.R.**, R.O. Peterson, D.W. Smith, J.A. Vucetich. Direct and indirect density-dependence in wolves. *Midwest Fish and Wildlife Conference* - 71st Annual meeting, Minneapolis, MN.

2010 **MacNulty, D.R.**, L.D. Mech, H.C. Cluff. Assessing the efficacy of a GPS/ARGOS radiocollar for measuring wolf predation on High Arctic ungulates. *North American Caribou Workshop* - 13th Annual meeting, Winnipeg, Manitoba

2010 **MacNulty, D.R.**, J.A. Vucetich, D.W. Smith. Influence of age-structured kill rate on wolf-elk dynamics. *American Society of Mammalogists* – 90th Annual meeting, Laramie, WY.

2010 Smith, D.W., D.R. Stahler, **D.R. MacNulty**, R. Raymond, K. Cassidy, E. Albers. Wolf pack stability promotes dispersal and recovery area connectivity. *Society for Conservation Biology* – 24th Annual meeting, Edmonton, Alberta

2010 Madden, J.D. ††, R.C. Arkin, **D.R. MacNulty**. Multi-robot system based on model of wolf hunting behavior to emulate wolf and elk interactions. *IEEE International Conference on Robotics and Biomimetics*, Tianjin, China.

2009 **MacNulty, D.R.**, D.W. Smith, J.A. Vucetich, L.D. Mech, D.R. Stahler, C. Packer Predatory senescence in aging wolves. *Ecological Society of America* – 94th Annual meeting, Albuquerque, NM.

2008 **MacNulty, D.R.**, D.W. Smith, L.D. Mech. Size-related improvements in hunting ability as a cause of Cope's Rule in carnivores. Joint annual meeting of the *Society for the Study of Evolution*, the *Society of Systematic Biologists,* and the *American Society of Naturalists*, Minneapolis, MN.

2007 **MacNulty, D.R.**, C. Packer, L.D. Mech, D.W. Smith. Communal hunting in wolves: cooperation or cheating? *Animal Behavior Society* - 44th Annual meeting, Burlington, VT.

2003 **MacNulty, D.R.**, D.W. Smith. Wolf aversion to the risk of prey-caused injury and its implications for reducing wolf predation on livestock. *Society for Conservation Biology* - 17th Annual meeting, Duluth, MN.

2003 **MacNulty, D.R.**, D.W. Smith. Group hunting behavior in wolves. *World Wolf Congress*, Banff, Alberta.

2003 **MacNulty, D.R.,** D.W. Smith, L.D. Mech. An analysis of wolf hunting behavior in Yellowstone National Park. *North American Interagency Wolf Conference*. Pray, MT.

2001 **MacNulty, D.R.,** L.D. Mech, D.W. Smith. Factors influencing the outcome of wolf-elk encounters. *American Society of Mammalogists*- 81st Annual meeting, Missoula, MT.

2000 **MacNulty, D.R.,** L.D. Mech, D.W. Smith. Pursuit-deterrence signals in wolf-elk interactions. *Beyond 2000: The Realities of Global Wolf Restoration*, Duluth, MN.

2000 **MacNulty, D.R.,** L.D. Mech, D.W. Smith. Hunting success of gray wolves in Yellowstone National Park. *Beyond 2000: The Realities of Global Wolf Restoration*, Duluth, MN.

Conference poster presentations and abstracts

2017 Robertson, S.D.*, K.R. Hersey, **D.R. MacNulty**. Population ecology of Utah moose. *Utah Chapter of the Wildlife Society* – Annual Meeting, Bryce Canyon, UT

2017 Hallerud, M.A.**, D.C. Stoner, **D.R. MacNulty**. Cougars in Cache: approaching the beast from multiple perspectives. *Utah Chapter of the Wildlife Society* – Annual Meeting, Bryce Canyon, UT

2016 Hallerud, M.**, D. Johnson, T. Jeppson, J. Gardner, R. Jensen, D. Stoner, **D.R. MacNulty**. Monitoring cougar activity with remote-sensor cameras in the Intermountain West. *The Wildlife Society* – 23rd Annual meeting, Raleigh, NC.

2016 Hallerud, M.**, D. Johnson, T. Jeppson, **D.R. MacNulty**. Monitoring cougar activity with remote-sensor cameras in Logan, Utah. *Utah State University Student Research Symposium*, Logan, UT.

2016 Shipp, H.**, **D.R. MacNulty**. Using accelerometer data to remotely assess predation activity of Arctic wolves (*Canis lupus arctos*). *Utah Chapter of the Wildlife Society* - Annual Meeting. St. George, UT.

2016 Shipp, H.**, **D.R. MacNulty**. Using accelerometer data to remotely assess predation activity of Arctic wolves. *Utah State University Student Research Symposium*, Logan, UT

2016 Shipp, H.**, **D.R. MacNulty**. Using accelerometer data to remotely assess predation activity of Arctic wolves. *Utah Research on Capitol Hill*, Salt Lake City, UT

2016 Tallian, A.*, A. Ordiz, M.C. Metz, C. Milleret, C. Wikenros, D.W. Smith, D.R. Stahler, **D.R. MacNulty**, P. Wabakken, J.E. Swenson, H. Sand. Competition between apex predators? Brown bears decrease wolf kill rate on two continents. *Gordon Research Conference: Predator-Prey Interactions, New frontiers in understanding predator-prey interactions in a human-altered world*, Ventura, CA.

2016 Anderson, M., **D.R. MacNulty**, H.D. Cluff, L.D. Mech. Wolf space use and predation patterns in the High Arctic. *North American Caribou Workshop* - 16th Annual meeting, Thunder Bay, Ontario.

2014 Unger, B.**, **D.R. MacNulty**. Is non-suitable habitat causing bobolinks to recede from northern Utah? *Utah State University Student Research Symposium*, Logan, UT.

2014 Kindermann, R.*, **D.R. MacNulty,** D.W. Smith, D.R. Stahler, P.J. White. Predictors of pregnancy rates in Yellowstone elk. *Gordon Research Conference: Predator-Prey Interactions, From Genes to Ecosystems to Human Mental Health*, Ventura, CA.

2014 Kohl, M.T.*, **D.R. MacNulty**, J.D. Forester, M.J. Kauffman, D.W. Smith, D.R. Stahler. Influence of wolf predation risk on elk movement in Yellowstone National Park. *Gordon Research Conference: Predator-Prey Interactions, From Genes to Ecosystems to Human Mental Health*, Ventura, CA.

2014 Tallian, A.*, **D.R. MacNulty,** D.W. Smith, D.R. Stahler. Are wolves more cooperative when hunting formidable prey? *Gordon Research Conference: Predator-Prey Interactions, From Genes to Ecosystems to Human Mental Health*, Ventura, CA.

Teaching

Awards

2014 Utah State University, Quinney College of Natural Resources Teacher of the Year

Instructor – Utah State University

2018-present *Mammalogy* (WILD/BIOL 5580, undergraduate, 3 credits, 30-40 students)
2012-present *Ecology of Animal Populations* (WILD 6400, graduate, 3 credits, 5-20 students)
2014-2016 *Wildland Resource Techniques* (WILD 2400, undergraduate, 3 credits, 48-58 students)
2011-2013 *Wildland Animal Ecology & Identification* (WILD 3610, undergraduate, 4 credits, 34-50 students)

Invited Lecturer – Utah State University

2014-2017 *Principles of Rangeland Management* (WILD 4000, undergraduate, 3 credits 15-20 students)

Instructor – University of Minnesota

2011 *Analysis of Populations* (graduate/undergraduate)
2009 *Introduction to Animal Behavior* (undergraduate)
2006 *Ecology and Management of Wildlife Habitats* (graduate/undergraduate)
1999 *Wildlife ecology and Management Field Course* (undergraduate)

Invited Lecturer – University of Minnesota

2005, 07, 09 *Biology and Management of Large Mammals* (graduate/undergraduate)
2000, 03, 08 *Introduction to Fisheries, Wildlife, and Conservation Biology* (undergraduate)
2007 *Ecology and Management of Wildlife Habitats* (graduate/undergraduate)
2007 *Evolution of Social Behavior* (graduate/undergraduate)
2003 *Zoology* (undergraduate)
2001 *Ecosystem Conservation* (undergraduate)

Teaching Assistant – University of Minnesota

2001-2002 *Zoology* (undergraduate)
1998 *Introduction to Fisheries, Wildlife, and Conservation Biology* (undergraduate)
1998 *General Biology* (undergraduate)

Student Mentoring

Awards

2017 Utah State University, Quinney College of Natural Resources Undergraduate Faculty Mentor of the Year

Graduate Student Advisees

2018-present **Bonnie MacDonald**, M.S., *Behavior and ecology of northern Yellowstone elk*

2017-present **Lainie Brice**, Ph.D. (QCNR-Quinney Research Fellow: $80,000; USU Climate Adaptation Science Fellow: $34,000), *Understanding the indirect influence of wolves on aspen (Populus tremuloides) growth in northern Yellowstone National Park*

2016-present **Lacy Smith**, Ph.D. (NSF-Graduate Research Fellow: $138,000), *Predator-prey dynamics and large predator diversity in Yellowstone National Park.*

2016-present **Sam Robertson**, M.S., *Population ecology of Utah moose.*

2012-2018 **Michel Kohl**, Ph.D. (QCNR-Quinney Research Fellow: $75,000; Ford Foundation Fellow: $25,000; USU-RGS Dissertation Fellow: $8,000), *The spatial ecology predator-prey interactions: a case study of Yellowstone elk, wolves, and cougars.*

2012-2016 **Aimee Tallian**, Ph.D. (NSF-Graduate Research Fellow: $138,000; NSF-Graduate Research Opportunities Worldwide: $29,000; USU-RGS Dissertation Fellow: $8,000; USU Seely-Hinckley Fellow: $3,300), *The behavior and ecology of a cursorial predator and dangerous prey: integrating behavioral mechanisms with population-level patterns in large mammal systems*.

2013-2015 **Joel Ruprecht**, M.S., *The demography and determinants of population growth in Utah moose.*

Co-Advised Post-Doctoral Researchers

2015-present **Sarah Hoy**, post-doc (co-advised with John Vucetich, Michigan Tech. Univerisy), *Population biology of large mammals in Yellowstone and Isle Royale National Parks*

2012-2014 **Sarah Cubaynes**, post-doc (co-advised with Tim Coulson, University of Oxford), *Demography of Yellowstone wolves*

Graduate Student Committee Membership

2011-present I have served on a total of 10 committees for students (5 PhD, 5 MS) advised by other faculty. I currently serve on 5 committees (4 PhD, 1 MS).

Undergraduate Research Advisees

2016-2017	**Nikki Tatton**, Project: *Assessing global positioning system telemetry techniques for estimating wolf predation in the High Arctic.*
2015-2018	**Maggie Hallerud** (Honors, QCNR Undergraduate Research grantee, 2017 QCNR Undergraduate Researcher of the Year, 2018 QCNR Outstanding Senior and Scholar of the Year), Thesis: *Development of the USU cougar project as a long-term wildlife monitoring program*.
2014-2016	**Heather Shipp** (Honors, URCO grantee), Thesis: *Using accelerometer data to remotely assess predation activity in High Arctic wolves.*
2013	**Konrad Hafen**, Project: *Population ecology of Utah moose*.
2013	**Jesse Godbold** (Research Experience Undergraduate; visiting from Eastern Kentucky University), *Population ecology of Utah moose*.
2012-2015	**Bethany Unger** (Honors, URCO grantee), Thesis: *Unsuitable habitat as a cause for declining bobolink populations in northern Utah*.
2012-2013	**Kari Norman** (Honors, Undergraduate Research Fellow), Project: *Pursuit-deterrence signaling in wolf-elk interactions*.

Service

On-campus

2017-present	USU representative for the Rocky Mountain Cooperative Ecosystems Study Unit (RM-CESU)
2017	Member, Faculty hiring committee
2017	Chair, Faculty hiring committee
2014-2017	Participant, Native American STEM Mentorship Program
2014-present	Faculty Advisor, USU Chapter of the Wildlife Society (20-30 students)
2013-2014	Faculty Co-Advisor, USU Chapter of the Wildlife Society
2013-present	Undergraduate Faculty Advisor, Wildlife Science Degree Program, Department of Wildland Resources (21-26 students)
2011-2017	Member, Undergraduate Curriculum Committee, Department of Wildland Resources
2014	Member, Awards committee, Quinney College of Natural Resources
2014	Member, Selection committee for the Department of Wildland Resources Presidential Doctoral Research Fellowship
2013	Member, Ad hoc advisory panel counseling undergraduate students on application to graduate school

Off-campus

(a) Reviewed manuscripts for the following scientific journals and book publishers:

2018 *Behavioral Ecology, Proceedings of the Royal Society Biological Sciences*

2017 *Animal Behavior, Ecology*, *Journal of Animal Ecology* (3x), *Journal of Wildlife Management, Proceedings of the National Academy of Sciences*, *Royal Society Open Science* (3x)

2016 *American Naturalist, Animal Conservation, Behavioral Ecology, Behavioral Ecology & Sociobiology, Biological Conservation*, *Ecosphere*, *Journal of Applied Ecology* (2x), *Journal of Mammalogy, Proceedings of the National Academy of Sciences, Proceedings of the Royal Society Biological Sciences* (2x)

2015 *Biological Conservation, Ecography, Ecosphere, Journal of Animal Ecology* (2x), *PLoS Computational Biology*

2014 *Animal Behavior*, *Animal Conservation* (2x), *Behavioral Ecology, Biological Conservation, Ecosphere* (2x), *Journal of Animal Ecology* (3x), *Journal of Wildlife Management* (2x), *Oecologia, Oikos* (2x), *PLoS ONE, J. of the Royal Society Interface*

2013 *Animals*, *Animal Behavior, Biological Conservation, Ecology, Ecological Applications, Journal of Mammalogy, PLoS ONE* (3x), *Wildlife Biology*

2012 *Ecological Applications*, *Ecological Modeling*, *Journal of Animal Ecology*

2011 *Arctic, Journal of Animal Ecology*

2010 *Journal of Wildlife Management, Oikos, Wildlife Biology*

2009 *Animal Behavior*, *Journal of Animal Ecology* (2x), *Journal of Wildlife Management*

2008 *Journal of Herpetology*, *Journal of Wildlife Management, Oikos*

2007 *Oikos, University of Chicago Press*

(b) Reviewed research proposals for the following agencies/institutions:

Panelist

2016	*National Science Foundation – Doctoral Dissertation Improvement Grants*
2016	*National Science Foundation – Graduate Research Fellowship*

Ad-hoc reviewer

2007, 10, 15	*National Science Foundation*
2012, 15	*National Geographic Society*
2014	*Alberta Conservation Association*
2009	*Yellowstone National Park*

(c) Professional society service

2014 *SEEDS mentor, Ecological Society of America* – 99th Annual meeting

2013 *Student presentation judge, Ecological Society of America* – 98th Annual meeting

(d) Provided interviews and served as expert consultant for the following media projects:

Newspaper articles

2018 USA Today, Sep 7. Yellowstone's wolves are back, but they haven't restored the park's ecosystem. Here's why.

2016 New York Times, May 10. *Study casts doubt on theory that legal hunting reduces poaching*.

2015 Salt Lake Tribune, Mar 18. *Utah sends $500K more to unexplained wolf delisting efforts*.

2015 Salt Lake Tribune, Jan 5. *A case of mistaken identity: Wolves and coyotes*.

Magazine articles
2017 Outside, Oct 30. *A very old man for a wolf*.
2017 High Country News, Jan. 23. *Why OR7 is a celebrity*.
2015 Science, Mar 20. *Lessons from the wild lab: Yellowstone park is a real-world laboratory of predator-prey relations*.
2015 Nature, Jan 20. *Wolf cull will not save threatened Canadian caribou.*
2014 National Geographic (online), May 18. *Lone wolf that took epic journal across West finds a mate*.
2012 National Geographic (online), Feb 3. *Would real wolves act like the wolves of 'The Grey'?*

Books
2015 *In Wolf Country: The Power & Politics of Reintroduction* by Jim Yuskavitch
2014 *National Geographic Kids Mission: Wolf Rescue* by Kitson Jazynka
2008 *Where the Wild Things Were: Life, Death, and Ecological Wreckage in a Land of Vanishing Predators* by William Stolzenburg

Television Documentaries
2018 *Arctic Wolf Pack*. Nature. Gulo Film Productions
2017 *Spy in the Wild*, Episodes 3, 4, & 5. Nature, John Downer Productions
2014, 15 *Snow Wolf Family and Me*, Episode 1 & 2. BBC Two, BBC Natural History Unit.
2015 *The Hunt: In the Grip of the Seasons (Arctic).* BBC One, Silverback Films
2006 *Prehistoric Predators: Dire Wolf*. National Geographic.

Educational Video Games
2014-present *Wolf Quest 2.7 (www.wolfquest.org)*. Eduweb.
2008-2011 *Wolf Quest.* Minnesota Zoo

Highlights of Research Media Coverage

Print

2018 The Economist, Sep 4, *Deer antlers are a dual-use technology*
2018 Billings Gazette, Jul 5, *Yellowstone elk are skilled at working around wolf's schedule*
2017 Billings Gazette, Feb 9, *Carcass-stealing by grizzlies doesn't mean wolves kill more*
2016 Post Register, July 20. *Yellowstone Science looks back at 20 years of wolves*
2015 Utah State Magazine, Spring. *Wolfing it down*
2014 Salt Lake Tribune, Nov 13. *New research shows Yellowstone wolves pick their prey based on pack size*
2014 Standard-Examiner, Nov 13. *Utah researchers learn the secrets of wolf hunting habits in Yellowstone*
2014 Salt Lake Tribune, July 31. *Moose make a surprising move into Utah's Range Creek*
2014 Salt Lake Tribune, May 14. *Utah study: 'Crowded' wolves raid other packs, kill pups*
2012 Salt Lake Tribune, Dec 17. *What's making Utah's moose numbers rise and fall?*
2012 Standard-Examiner, Oct 17. *USU researcher traces factors that govern wolf reproduction*
2011 Salt Lake Tribune, Dec 19. *A coat of many clues*
2011 Science, Dec 2. *Mathematical dances with wolves.*
2011 Salt Lake Tribune, Oct 1. *Big wolf packs not always the best hunters*

Online

2018 ScienceShots, Sep 3. *To grow bigger antlers, these elk risk life and limb*
2018 Earth.com, Jun 22. *Predators in Yellowstone "landscape of fear" may not be so scary*
2017 Phys.org, Apr 10. *In harm's way: wolves may not risk 'prey switching' ecologists say*
2017 New Scientist, Feb 8. *Why grey wolves kill less prey when brown bears are around.*
2016 Utah State Today, Jul 21. *USU ecologist an author, editor of 'Yellowstone Science' wolf issue*
2016 Billings Gazette, Jul 20. *'Yellowstone Science' magazine focuses on wolves*
2015 PBS Newshour, Sep 4. *Did wolves help restore trees to Yellowstone?*
2015 National Science Foundation – Science Nation, Aug 31. *Understanding the ecological role of wolves in Yellowstone National Park*
2014 Phys.org, Nov 12. *Ecologists say larger group aids wolves' bison hunting*
2014 Phys.org, May 13. *Wolves need enemy-free space to raise offspring say ecologists*
2014 Discovery News, May 13. *Wolf survival places a premium on space*
2012 ScienceDaily, Oct. 9. *Healthy mom with lots of help key to thriving brood, wolf study shows*
2012 National Science Foundation Discoveries, Jan 12. *On the hunt, is bigger better for predators like wolves?*
2011 ScienceDaily, Dec 5. *New insights into responses of Yellowstone wolves to environmental change*
2011 LiveScience, Dec 1. *Yellowstone wolves show how animals change with nature*
2011 ScienceNow, Sept 29. *Freeloading wolves*

Radio

2018 Utah Public Radio, Sep 17. *Undisciplined: police and predators*
2018 Utah Public Radio, Jun 8. *Monitoring Utah moose and their calves*
2017 Utah Public Radio, Jul 14. *Securing Utah's moose population*
2016 Utah Public Radio, Jan 29. *Undergraduate researchers share their work with Utah legislators*
2015 KVNU, Oct 19. *Wolf management and 'Wolves on Hunt'*
2015 Utah Public Radio, Sep 16. *New Findings on wolf and elk populations in Yellowstone National Park*
2015 KPCW, Jun 11. *Cool Science Radio: Wolves on the Hunt*
2014 Utah Public Radio, Oct 13. *Wolf dispersal behavior*
2014 Utah Public Radio, May 12. *Territorial behavior limits wolf survival*
2012 Utah Public Radio, Oct 9. *New study may help wolf management in the West*
2012 Utah Public Radio, Sep 28. *Wyoming wolves and the implications of killing them*
2011 Utah Public Radio, Dec 7. *Predicting the effects of environmental change on wildlife*

Professional Development

Research workshops attended

2016 *Bayesian integrated population modeling using BUGS and JAGS.* Instructors: Drs. Marc Kery, Michael Schaub, David Koons. Jul 25-29. Utah State University, Logan, UT.

2015 *Bayesian modeling for ecologists and social scientists*. Instructor: Dr. N.T. Hobbs. Aug 19-28. National Socio-Environmental Synthesis Center, Annapolis, MD.

2011 *Writing Winning Grants*. Dec 1. Utah State University, Logan, UT.

2011 *New Faculty Research Orientation*. Aug 23. Utah State University, Logan, UT.

Teaching workshops attended

2014 *Student-centered learning environments in Canvas.* Sep 22. Utah State University, Logan, UT.

2014 *Flipped class videos: the director's cut workshop*. Instructor: Dr. Rich Etchberger. May 8. Utah State University, Logan, UT.

2011 *New Faculty Teaching Academy.* Aug 16 – Dec 6. Utah State University, Logan, UT.

Professional Associations

American Association for the Advancement of Science
Ecological Society of America

Appendix B. Completed Conflict of Interest Forms

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Conflict of Interest Form for Reviewer 1

Dr. Fred W. Allendorf

U.S. Fish and Wildlife Service
Participation in Peer Review of Scientific Findings in U.S. Fish and Wildlife Service's Proposed Rule to remove the Gray Wolf (Canis lupus) from the List of Endangered and Threatened Wildlife

CONFLICT OF INTEREST DISCLOSURE

NAME: ___Fred W. Allendorf______ TELEPHONE: [redacted]

ADDRESS: [redacted]

EMAIL ADDRESS: fred.allendorf@gmail.com

CURRENT EMPLOYER: Retired

It is essential that a peer reviewer used by the U.S. Fish and Wildlife Service as part of its peer review of proposed listing and proposed critical habitat rules under the ESA report any conflict of interest. For this purpose, **the term "conflict of interest" means any financial or other interest which conflicts with the service of the individual because it (1) could significantly impair the individual's objectivity or (2) could create an unfair competitive advantage for any person or organization.**[1] The term "conflict of interest" means something more than individual bias. There must be an *interest* that could be directly affected by your participation as a peer reviewer.

Conflict of interest requirements are objective standards designed to eliminate certain specific, potentially compromising situations from arising, and thereby to protect the individual (and his or her family members or business associates), the Service, and the public interest. The individual and the Service should not be placed in a situation where others could reasonably question, and perhaps discount or dismiss, the information produced through the peer review simply because of the existence of conflicting interests.

Please provide the information requested below regarding **relevant** organizational affiliations, government service, public statements and positions, research support, and additional information (if any) and complete the signature page following the questionnaire.

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[1] This definition and the other information in these instructions are drawn from the National Academy of Sciences Policy on Committee Composition and Balance and Conflicts of Interest for Committees Used in the Development of Reports (May 12, 2003).

1. EMPLOYMENT AND INVESTMENT INTERESTS.

(a) If you are employed or self-employed, could your current employment or self-employment (or your spouse's current employment or self-employment) be directly affected?

(b) Do you have any current or continuing consulting relationships (including, for example, commercial and professional consulting and service arrangements, scientific and technical advisory board memberships, serving as an expert witness in litigation, or providing services in exchange for honorariums and travel expense reimbursements) that are directly related to the subject matter of the possible government regulatory action or inaction?

(c) To the best of your knowledge, could any financial interests of your (or your spouse's) employer or, if self-employed, your (or your spouse's) clients and/or business partners be directly affected?

(d) If you are or have ever been a U.S. Government employee (either civilian or military), to the best of your knowledge are there any federal conflict of interest restrictions that may be applicable to your service in connection with this peer review?

(e) Do you or your spouse or minor children own directly or indirectly (e.g., through a trust or an individual account in a pension or profit-sharing plan) stocks, bonds or other financial instruments or investments that could be affected, either indirectly or by a direct effect on the business enterprise or activities underlying the investments?

If the answer to all of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is either "no" or "not applicable," check here ✓ (NO).

If the answer to any of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

I am currently serving on the NASEM Wolf Taxonomy Study Committee, but I believe that this is not directly related to the subject matter of the possible government regulatory action or inaction

2. PROPERTY INTERESTS.

(a) Do you, your spouse or minor children own directly or indirectly any property (e.g. real estate, tangible properties or intellectual properties) interests that could be directly affected?

(b) To the best of your knowledge, do any others with whom you have substantial common financial interests (e.g., employer, business partners, etc.) own directly or indirectly any such property interests that could be directly affected?

If the answer to all of the above questions under PROPERTY INTERESTS is either "no" or "not applicable," check here ✓ (NO).

If the answer to any of the above questions under PROPERTY INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

3. RESEARCH FUNDING AND OTHER INTERESTS.

(a) Could research funding and support for you or your close research colleagues and collaborators be directly affected, or

(b) If you have any research agreements for current or continuing research funding or support from any party whose financial interests could be directly affected, and such funding or support is directly related to the subject matter of the regulatory process, do such agreements significantly limit your ability to independently conduct and publish the results of your research?

(c) Do you have any existing professional obligations (e.g., as an officer of a scientific or engineering society) that effectively require you to publicly defend a previously established position on an issue that is relevant to the proposed rule?

(d) Could your service as a peer reviewer create a specific financial or commercial competitive advantage for you or others with whom you have substantial common financial interests?

If the answer to all of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is either "no" or "not applicable," check here _✓_ (NO).

If the answer to any of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

Please **return the form to** [*Contact Information*] **Please retain a copy for your records.**

During your period of service in connection with the activity for which this form is being completed, any changes in the information reported, or any new information, which needs to be reported, should be reported promptly by written or electronic communication to the responsible staff officer.

Fred W. Ay
YOUR SIGNATURE

17 Sep 18
DATE

Reviewed by: Cheryl Propst on behalf of Atkins
[*Regional Liaison*]
U.S. Fish and Wildlife Service
[*Regional Office*]
[*Title*]

11 December 2018
DATE

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Conflict of Interest Form for Reviewer 2

Dr. Charles (Carlos) Carroll

U.S. Fish and Wildlife Service
Participation in Peer Review of Scientific Findings in U.S. Fish and Wildlife Service's Proposed Rule to remove the Gray Wolf (Canis lupus) from the List of Endangered and Threatened Wildlife

CONFLICT OF INTEREST DISCLOSURE

NAME: ______Carlos Carroll___________ TELEPHONE: _[redacted]

ADDRESS: __[redacted]_______________________________________

______[redacted]____

EMAIL ADDRESS: carlos@klamathconservation.org__________________

CURRENT EMPLOYER: ______Self_____________________________

It is essential that a peer reviewer used by the U.S. Fish and Wildlife Service as part of its peer review of proposed listing and proposed critical habitat rules under the ESA report any conflict of interest. For this purpose, **the term "conflict of interest" means any financial or other interest which conflicts with the service of the individual because it (1) could significantly impair the individual's objectivity or (2) could create an unfair competitive advantage for any person or organization.**[1] The term "conflict of interest" means something more than individual bias. There must be an *interest* that could be directly affected by your participation as a peer reviewer.

Conflict of interest requirements are objective standards designed to eliminate certain specific, potentially compromising situations from arising, and thereby to protect the individual (and his or her family members or business associates), the Service, and the public interest. The individual and the Service should not be placed in a situation where others could reasonably question, and perhaps discount or dismiss, the information produced through the peer review simply because of the existence of conflicting interests.

Please provide the information requested below regarding **relevant** organizational affiliations, government service, public statements and positions, research support, and additional information (if any) and complete the signature page following the questionnaire.

---

[1] This definition and the other information in these instructions are drawn from the National Academy of Sciences Policy on Committee Composition and Balance and Conflicts of Interest for Committees Used in the Development of Reports (May 12, 2003).

1. <u>EMPLOYMENT AND INVESTMENT INTERESTS</u>.

(a) If you are employed or self-employed, could your current employment or self-employment (or your spouse's current employment or self-employment) be directly affected?

(b) Do you have any current or continuing consulting relationships (including, for example, commercial and professional consulting and service arrangements, scientific and technical advisory board memberships, serving as an expert witness in litigation, or providing services in exchange for honorariums and travel expense reimbursements) that are directly related to the subject matter of the possible government regulatory action or inaction?

(c) To the best of your knowledge, could any financial interests of your (or your spouse's) employer or, if self-employed, your (or your spouse's) clients and/or business partners be directly affected?

(d) If you are or have ever been a U.S. Government employee (either civilian or military), to the best of your knowledge are there any federal conflict of interest restrictions that may be applicable to your service in connection with this peer review?

(e) Do you or your spouse or minor children own directly or indirectly (e.g., through a trust or an individual account in a pension or profit-sharing plan) stocks, bonds or other financial instruments or investments that could be affected, either indirectly or by a direct effect on the business enterprise or activities underlying the investments?

If the answer to all of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is either "no" or "not applicable," check here _X____ (NO).

If the answer to any of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

2. <u>PROPERTY INTERESTS</u>.

(a) Do you, your spouse or minor children own directly or indirectly any property (e.g. real estate, tangible properties or intellectual properties) interests that could be directly affected?

(b) To the best of your knowledge, do any others with whom you have substantial common financial interests (e.g., employer, business partners, etc.) own directly or indirectly any such property interests that could be directly affected?

If the answer to all of the above questions under PROPERTY INTERESTS is either "no" or "not applicable," check here __X___ (NO).

If the answer to any of the above questions under PROPERTY INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

3. RESEARCH FUNDING AND OTHER INTERESTS.

(a) Could research funding and support for you or your close research colleagues and collaborators be directly affected, or

(b) If you have any research agreements for current or continuing research funding or support from any party whose financial interests could be directly affected, and such funding or support is directly related to the subject matter of the regulatory process, do such agreements significantly limit your ability to independently conduct and publish the results of your research?

(c) Do you have any existing professional obligations (e.g., as an officer of a scientific or engineering society) that effectively require you to publicly defend a previously established position on an issue that is relevant to the proposed rule?

(d) Could your service as a peer reviewer create a specific financial or commercial competitive advantage for you or others with whom you have substantial common financial interests?

If the answer to all of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is either "no" or "not applicable," check here ___X__ (NO).

If the answer to any of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

Please **return the form to** [*Contact Information*] **Please retain a copy for your records**.

During your period of service in connection with the activity for which this form is being completed, any changes in the information reported, or any new information, which needs to be reported, should be reported promptly by written or electronic communication to the responsible staff officer.

______________________________________ _9/17/2018______________
YOUR SIGNATURE DATE

Reviewed by: __on behalf of Atkins_________ 12/11/18__________
[*Regional Liaison*] DATE
U.S. Fish and Wildlife Service
[*Regional Office*]
[*Title*]

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Conflict of Interest Form for Reviewer 3

Adrian P. Wydeven

U.S. Fish and Wildlife Service
Participation in Peer Review of Scientific Findings in U.S. Fish and Wildlife Service's Proposed Rule to remove the Gray Wolf (Canis lupus) from the List of Endangered and Threatened Wildlife

CONFLICT OF INTEREST DISCLOSURE

NAME: Adrian P. Wydeven, TELEPHONE: [redacted]

ADDRESS: [redacted]

EMAIL ADDRESS: adrianwydeven@cheqnet.net

CURRENT EMPLOYER: retired from WI Dep. Natural Resources

It is essential that a peer reviewer used by the U.S. Fish and Wildlife Service as part of its peer review of proposed listing and proposed critical habitat rules under the ESA report any conflict of interest. For this purpose, **the term "conflict of interest" means any financial or other interest which conflicts with the service of the individual because it (1) could significantly impair the individual's objectivity or (2) could create an unfair competitive advantage for any person or organization.[1]** The term "conflict of interest" means something more than individual bias. There must be an *interest* that could be directly affected by your participation as a peer reviewer.

Conflict of interest requirements are objective standards designed to eliminate certain specific, potentially compromising situations from arising, and thereby to protect the individual (and his or her family members or business associates), the Service, and the public interest. The individual and the Service should not be placed in a situation where others could reasonably question, and perhaps discount or dismiss, the information produced through the peer review simply because of the existence of conflicting interests.

Please provide the information requested below regarding **relevant** organizational affiliations, government service, public statements and positions, research support, and additional information (if any) and complete the signature page following the questionnaire.

[1] This definition and the other information in these instructions are drawn from the National Academy of Sciences Policy on Committee Composition and Balance and Conflicts of Interest for Committees Used in the Development of Reports (May 12, 2003).

1. EMPLOYMENT AND INVESTMENT INTERESTS.

(a) If you are employed or self-employed, could your current employment or self-employment (or your spouse's current employment or self-employment) be directly affected?

(b) Do you have any current or continuing consulting relationships (including, for example, commercial and professional consulting and service arrangements, scientific and technical advisory board memberships, serving as an expert witness in litigation, or providing services in exchange for honorariums and travel expense reimbursements) that are directly related to the subject matter of the possible government regulatory action or inaction?

(c) To the best of your knowledge, could any financial interests of your (or your spouse's) employer or, if self-employed, your (or your spouse's) clients and/or business partners be directly affected?

(d) If you are or have ever been a U.S. Government employee (either civilian or military), to the best of your knowledge are there any federal conflict of interest restrictions that may be applicable to your service in connection with this peer review?

(e) Do you or your spouse or minor children own directly or indirectly (e.g., through a trust or an individual account in a pension or profit-sharing plan) stocks, bonds or other financial instruments or investments that could be affected, either indirectly or by a direct effect on the business enterprise or activities underlying the investments?

If the answer to all of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is either "no" or "not applicable," check here _____ (NO).

If the answer to any of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is "yes," check here _X_ (YES), and briefly describe the circumstances below.

a.) I do consulting work for the Timber Wolf Alliance (TWA), at Northland College, Ashland, WI. TWA has not opposed downlisting or delisting wolves in Wisconsin & Michigan in reclassification rule drafts for the Western Great Lakes in 2000, 2006 and 2011. Because of concern by some over the aggressive wolf harvest imposed by the Wisconsin legislature in 2012, some in TWA have become less supportive toward future wolf delisting. It is possible that some of my consulting work for TWA may change slightly with delisting.
My wife, Sarah R. Boles, is a part-time employee for the WI DNR, conducting wolf track surveys and training volunteer wolf trackers. With delisting and completion of the 5-year post-delisting process, her job may eventually be eliminated. Sarah is 64 years old and the eventual elimination of this position is not likely to be hardship.

b.) Through my consulting work with TWA, my travel and expenses to appear before state legislative hearings were covered last January 2018. I spoke out on behalf of TWA against legislation that would have prohibited the WI DNR from surveying, researching or conducting law enforcement work protecting wolves in the state. This legislation had been developed by the WI legislature to try to force Congress to delist wolves. Once wolves are securely delisting, the state legislature will no longer be creating laws to try to pressure Congress, and TWA will not need to send me to Madison to oppose these kind of legislations.

c.) No

d.) No

e.) No

2. PROPERTY INTERESTS.

(a) Do you, your spouse or minor children own directly or indirectly any property (e.g. real estate, tangible properties or intellectual properties) interests that could be directly affected?

(b) To the best of your knowledge, do any others with whom you have substantial common financial interests (e.g., employer, business partners, etc.) own directly or indirectly any such property interests that could be directly affected?

If the answer to all of the above questions under PROPERTY INTERESTS is either "no" or "not applicable," check here __X__(NO).

If the answer to any of the above questions under PROPERTY INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

3. <u>RESEARCH FUNDING AND OTHER INTERESTS</u>.

(a) Could research funding and support for you or your close research colleagues and collaborators be directly affected, <u>or</u>

(b) If you have any research agreements for current or continuing research funding or support from any party whose financial interests could be directly affected, and such funding or support is directly related to the subject matter of the regulatory process, do such agreements significantly limit your ability to independently conduct and publish the results of your research?

(c) Do you have any existing professional obligations (e.g., as an officer of a scientific or engineering society) that effectively require you to publicly defend a previously established position on an issue that is relevant to the proposed rule?

(d) Could your service as a peer reviewer create a specific financial or commercial competitive advantage for you or others with whom you have substantial common financial interests?

If the answer to all of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is either "no" or "not applicable," check here _X_ (NO).

If the answer to any of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

Please **return the form to** [*Contact Information*] **Please retain a copy for your records**.

During your period of service in connection with the activity for which this form is being completed, any changes in the information reported, or any new information, which needs to be reported, should be reported promptly by written or electronic communication to the responsible staff officer.

_Adrian P. Wydeven___ ___ September 17, 2018
YOUR SIGNATURE DATE

Reviewed by: Cheryl Probst

on behalf of Atkins 12/19/18_______
[*Regional Liaison*] DATE
U.S. Fish and Wildlife Service
[*Regional Office*]
[*Title*]

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Conflict of Interest Form for Reviewer 4

Dr. Adrian Treves

U.S. Fish and Wildlife Service
Participation in Peer Review of Scientific Findings in U.S. Fish and Wildlife Service's Proposed Rule to remove the Gray Wolf (Canis lupus) from the List of Endangered and Threatened Wildlife

CONFLICT OF INTEREST DISCLOSURE

NAME: ____Adrian Treves_________ TELEPHONE: _608-890-1450____

ADDRESS: [redacted]

EMAIL ADDRESS: ____________atreves@wisc.edu
CURRENT EMPLOYER: ____________Unievrsity of Wisconsin

It is essential that a peer reviewer used by the U.S. Fish and Wildlife Service as part of its peer review of proposed listing and proposed critical habitat rules under the ESA report any conflict of interest. For this purpose, **the term "conflict of interest" means any financial or other interest which conflicts with the service of the individual because it (1) could significantly impair the individual's objectivity or (2) could create an unfair competitive advantage for any person or organization.**[1] The term "conflict of interest" means something more than individual bias. There must be an *interest* that could be directly affected by your participation as a peer reviewer.

Conflict of interest requirements are objective standards designed to eliminate certain specific, potentially compromising situations from arising, and thereby to protect the individual (and his or her family members or business associates), the Service, and the public interest. The individual and the Service should not be placed in a situation where others could reasonably question, and perhaps discount or dismiss, the information produced through the peer review simply because of the existence of conflicting interests.

Please provide the information requested below regarding **relevant** organizational affiliations, government service, public statements and positions, research support, and additional information (if any) and complete the signature page following the questionnaire.

[1] This definition and the other information in these instructions are drawn from the National Academy of Sciences Policy on Committee Composition and Balance and Conflicts of Interest for Committees Used in the Development of Reports (May 12, 2003).

1. <u>EMPLOYMENT AND INVESTMENT INTERESTS</u>.

(a) If you are employed or self-employed, could your current employment or self-employment (or your spouse's current employment or self-employment) be directly affected?

(b) Do you have any current or continuing consulting relationships (including, for example, commercial and professional consulting and service arrangements, scientific and technical advisory board memberships, serving as an expert witness in litigation, or providing services in exchange for honorariums and travel expense reimbursements) that are directly related to the subject matter of the possible government regulatory action or inaction?

(c) To the best of your knowledge, could any financial interests of your (or your spouse's) employer or, if self-employed, your (or your spouse's) clients and/or business partners be directly affected?

(d) If you are or have ever been a U.S. Government employee (either civilian or military), to the best of your knowledge are there any federal conflict of interest restrictions that may be applicable to your service in connection with this peer review?

(e) Do you or your spouse or minor children own directly or indirectly (e.g., through a trust or an individual account in a pension or profit-sharing plan) stocks, bonds or other financial instruments or investments that could be affected, either indirectly or by a direct effect on the business enterprise or activities underlying the investments?

If the answer to all of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is either "no" or "not applicable," check here __X__ (NO).

If the answer to any of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is "yes," check here __ (YES), and briefly describe the circumstances below.

2. PROPERTY INTERESTS.

(a) Do you, your spouse or minor children own directly or indirectly any property (e.g. real estate, tangible properties or intellectual properties) interests that could be directly affected?

(b) To the best of your knowledge, do any others with whom you have substantial common financial interests (e.g., employer, business partners, etc.) own directly or indirectly any such property interests that could be directly affected?

If the answer to all of the above questions under PROPERTY INTERESTS is either "no" or "not applicable," check here __X___ (NO).

If the answer to any of the above questions under PROPERTY INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

3. RESEARCH FUNDING AND OTHER INTERESTS.

(a) Could research funding and support for you or your close research colleagues and collaborators be directly affected, or

(b) If you have any research agreements for current or continuing research funding or support from any party whose financial interests could be directly affected, and such funding or support is directly related to the subject matter of the regulatory process, do such agreements significantly limit your ability to independently conduct and publish the results of your research?

(c) Do you have any existing professional obligations (e.g., as an officer of a scientific or engineering society) that effectively require you to publicly defend a previously established position on an issue that is relevant to the proposed rule?

(d) Could your service as a peer reviewer create a specific financial or commercial competitive advantage for you or others with whom you have substantial common financial interests?

If the answer to all of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is either "no" or "not applicable," check here __X___ (NO).

If the answer to any of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

Please **return the form to** [*Contact Information*] **Please retain a copy for your records**.

During your period of service in connection with the activity for which this form is being completed, any changes in the information reported, or any new information, which needs to be reported, should be reported promptly by written or electronic communication to the responsible staff officer.

17 September 2018

YOUR SIGNATURE DATE

Reviewed by: Cheryl Propst

____on behalf of Atkins___ January 14, 2019_____
[*Regional Liaison*] DATE
U.S. Fish and Wildlife Service
[*Regional Office*]
[*Title*]

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Conflict of Interest Form for Reviewer 5

Dr. Daniel R. MacNulty

U.S. Fish and Wildlife Service
Participation in Peer Review of Scientific Findings in U.S. Fish and Wildlife Service's Proposed Rule to remove the Gray Wolf (Canis lupus) from the List of Endangered and Threatened Wildlife

CONFLICT OF INTEREST DISCLOSURE

NAME: **Dan MacNulty** TELEPHONE: [redacted]

ADDRESS: [redacted]

EMAIL ADDRESS: **dan.macnulty@usu.edu**

CURRENT EMPLOYER: **Utah State University**

It is essential that a peer reviewer used by the U.S. Fish and Wildlife Service as part of its peer review of proposed listing and proposed critical habitat rules under the ESA report any conflict of interest. For this purpose, **the term "conflict of interest" means any financial or other interest which conflicts with the service of the individual because it (1) could significantly impair the individual's objectivity or (2) could create an unfair competitive advantage for any person or organization.**[1] The term "conflict of interest" means something more than individual bias. There must be an *interest* that could be directly affected by your participation as a peer reviewer.

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(d) If you are or have ever been a U.S. Government employee (either civilian or military), to the best of your knowledge are there any federal conflict of interest restrictions that may be applicable to your service in connection with this peer review?

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If the answer to any of the above questions under EMPLOYMENT AND INVESTMENTS INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

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(c) Do you have any existing professional obligations (e.g., as an officer of a scientific or engineering society) that effectively require you to publicly defend a previously established position on an issue that is relevant to the proposed rule?

(d) Could your service as a peer reviewer create a specific financial or commercial competitive advantage for you or others with whom you have substantial common financial interests?

If the answer to all of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is either "no" or "not applicable," check here __X__ (NO).

If the answer to any of the above questions under RESEARCH FUNDING AND OTHER INTERESTS is "yes," check here ____ (YES), and briefly describe the circumstances below.

Please **return the form to** [*Contact Information*] **Please retain a copy for your records**.

During your period of service in connection with the activity for which this form is being completed, any changes in the information reported, or any new information, which needs to be reported, should be reported promptly by written or electronic communication to the responsible staff officer.

[signature]

January 14, 2019

YOUR SIGNATURE DATE

Reviewed by: [signature: Cheryl Propst]

_on behalf of Atkins____ January 14, 2019_______
[*Regional Liaison*] DATE
U.S. Fish and Wildlife Service
[*Regional Office*]
[*Title*]

Appendix C. Compiled Individual Reviews and Responses to Clarification Questions

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

May 3, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 1 – Dr. Fred W. Allendorf

Review of the Service's Gray Wolf Draft Biological Report (USFWS 2018) and Proposed Delisting Rule.

Consultant: Fred W. Allendorf, fred.allendorf@gmail.com, (406) 529-3283.

Dates of review: 22 April 2019 - 3 May 2019

I have reviewed the gray wolf draft biological report (USFWS 2018) and Proposed Delisting Rule. My expertise is in genetics so most of my comments address genetic aspects of these documents. Overall, the report and the proposed rule provide an adequate review and analysis of the factors relating to the persistence of gray wolves in the lower 48 states.

I had some difficulties in my evaluation because many statements throughout both documents do not include citations for the basis of the conclusion. For example, the following sentence occurs without a citation on page 9658 of the proposed rule: "Wolves in the entity appear to be genetically and demographically healthy". I am a geneticist, but I do not know what is meant by the phrase "genetically healthy". The common absence of citations made it hard to evaluate if the best available information was used and to evaluate the quality of the scientific information,

My primary concern with the proposed delisting rule is the treatment of Distinct Population Segments (DPSs). Only species, subspecies, or vertebrate DPSs can be listed, or delisted, under the Endangered Species Act (ESA). The gray wolf entity proposed for delisting in the rule is not a species or a subspecies. Therefore, it must be designated a DPS to be considered for delisting under the ESA. But, I could not find any treatment of designating DPS status to the "gray wolf entity" proposed to be delisted in the lower 48 states.

Overall, the treatment of the DPS status of gray wolves is very confusing to me in the proposed delisting rule. I appreciate that part of the problem is that actions on gray wolves predate the 1996 policy on designation of DPSs. Nevertheless, it is now 23 years since the DPS policy was established, and it is hard to understand why the treatment of gray wolves under the ESA is still not in compliance with this policy. For example, I do not understand how the Minnesota wolf "population" can still have a different ESA status since it is not a DPS. This needs to be clarified.

My reading of the proposed delisting rule is that there are two currently recognized DPSs: the Northern Rocky Mountain (NRM) DPS and the Western Great Lakes (WGL) DPS. I do not understand the overall DPS status of gray wolves in the lower 48 (excluding, of course, the Mexican gray wolf subspecies) based on this proposed delisting rule. For example, would all gray wolves in the lower 48 states be a single DPS?

Questions on the Draft Biological Report for Peer Review:

*1. Does the draft report provide an adequate and concise overview of gray wolf (*Canis lupus*) taxonomy, biology, and ecology as well as the changes in the biological status (range, distribution, abundance) of the gray wolf in the contiguous 48 United States over the last several decades?*

Yes, except for my comments under (2) below.

2. Please identify any oversights or omissions of data or information, and their relevance to the report. Are there other sources of information or studies that were not included that are relevant to the biological report? What are they and how are they relevant?

It would have been helpful if biological information needed to designating DPS status were also included in the draft biological report (e.g., distinctiveness, significance, etc.).

On page 22, USFWS (1994) is cited for the description of a metapopulations. This citation is not included in the References Cited.

Wheeldon et al. (2012) is cited on the 2nd line of page 20 of the report. However, this citation is not included in the References Cited.

In general, the Literature Cited needs some careful editing. For example, the genus and species (e.g., *Canis lupus*) should be in italics as they are in the rest of the report.

Questions on the Proposed Rule for Peer Review:

1. Does the proposed rule provide an adequate review and analysis of the factors relating to the persistence of the gray wolf population currently listed under the ESA in the contiguous 48-States (human-caused mortality, habitat and prey availability, disease and predation, and effects of climate change)?

It is unclear to me what the “gray wolf population currently listed under the ESA” is referring to here. I do not think that the two listed entities (*C. lupus* in Minnesota, and *C. lupus* in the lower 48 United States and Mexico outside of Minnesota) comprise a single “population”.

2. Have we (the Service) adequately considered the impacts of range reduction (i.e., lost historical range) on the long-term viability of the gray wolf in its remaining range in the lower-48 states (outside of the northern Rocky Mountains) and, if not, what information is missing and how is it relevant?

I cannot address this question since I do not understand the proposed DPS structure of wolves in the lower 48 states. I believe that the long-term viability of each DPS should be considered individually.

3. Is it reasonable for the Service to conclude that the approach of Michigan, Wisconsin, and Minnesota to wolf management, as described in their Plans and the proposed rule and in the context of wolf management in the Western Great Lakes area, are likely to maintain a viable wolf population in the Western Great Lakes area into the future?

Yes, this approach seems reasonable to me, but this is not my area of expertise.

4. Please identify any oversights or omissions of data or information, and their relevance to the assessment. Are there other sources of information or studies that were not included that are relevant to the proposed rule and, if so, what are they and how are they relevant?

As explained above, my primary concern is what I believe to be the inadequate treatment of the DPS structure of gray wolves in the lower 48 states.

5. Are there demonstrable errors of fact or interpretation? Have the authors of the proposed delisting rule provided reasonable and scientifically sound interpretations and syntheses from the scientific information presented in the draft biological report and the proposed rule? Are there instances in the proposed rule where a different but equally reasonable and sound interpretation might be reached that differs from that provided by the Service? If any instances are found where this is the case, please provide the specifics regarding those particular concerns.

I disagree with the statement on page 9649 of the proposed delisting rule that recovery of wolves in the northern Rocky Mountains "required" reintroduction of gray wolves in an experimental population. Gray wolves were recolonizing this region from Canada on their own (Pletscher et al. 1997). The Yellowstone reintroductions might have accelerated recovery, but they were certainly not "required".

Pletscher, D. H., R. R. Ream, D. K. Boyd, M. W. Fairchild, and K. E. Kunkel. 1997. Population dynamics of a recolonizing wolf population. Journal of Wildlife Management 61:459-465.

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Clarification Questions and Responses

May 24, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 1 – Dr. Fred W. Allendorf

GRAY WOLF 2019 PROPOSED RULE: PEER REVIEW

Clarification Questions

Peer Reviewer #1

- Pg 1, Paragraph 2: The reviewer states that "many statements throughout both documents do not include citations" and that there is a "common absence of citations". The reviewer provides one example from the rule. It is unclear which other statements the reviewer refers to. Can the reviewer identify the other statements they found that they feel should, but don't currently, include citations?
- Throughout the document: The peer reviewer indicates that they do not understand our treatment of DPSs. We assessed the status of a single entity. This single entity is composed of the two currently listed gray wolf entities, combined. The proposed rule represents an analysis of the status of gray wolves in this single entity. Provided this clarification, does the reviewer wish to add any further comments to their review?

1. Pg 1, Paragraph 2: The reviewer states that "many statements throughout both documents do not include citations" and that there is a "common absence of citations". The reviewer provides one example from the rule. It is unclear which other statements the reviewer refers to. Can the reviewer identify the other statements they found that they feel should, but don't currently, include citations?

There are many places in the proposed ruling that lack citations. For example, the statement in the middle of the first column on page 9655 about genetic studies on wolves on the west coast and the northern Rocky Mountains. However, such citations might not be required in the rule, but they should be included in the Draft Biological Report.

The rest of my comments refer to the Draft Biological Report. When I refer to a paragraph, it includes only those paragraphs starting on the page mentioned.

Cronin et al. (2015) is cited in the 2nd paragraph on page 3, but it is not included in the References Cited.

There is no citation for the following sentence in the 2nd paragraph on page 4: *For example, while genetic studies have found indications that Pacific coastal wolves in southeastern Alaska and British Columbia constitute a distinctive and largely isolated group, there is now evidence of admixture of coastal and inland wolves where the two forms meet in Washington.*

There is no citation for the following statement in the 3rd paragraph on page 10: *Prior to European settlement, wolves occupied both the upper and lower peninsulas of Michigan and likely occurred in every county in the state.*

There is no year included for the Beyer et al. citation in the next sentence.

There are no citations included in the 2nd and 3rd paragraphs on page 11.

There is no citation for the topic sentence of the 2nd paragraph on page 12.

There are no citations for the topic sentences of the 2nd and 3rd paragraphs on page 14.

There are no citations for the topic sentences of the 1st, 2nd, 3rd, and 4th paragraphs on page 15.

There is no citation for the topic sentence of the 3rd paragraph on page 17.

There is no citation for the topic sentence of the 3rd paragraph on page 18.

There are no citations for the many statements about genetic analysis in the paragraph continuing on page 20.

There are no citations for the topic sentences of the 1st and 2nd paragraphs on page 21.

There is no citation for the topic sentence of the 1st paragraph on page 23.

There are no citations for the topic 3rd and 4th paragraphs on page 23.

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

May 3, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 2 – Dr. Charles (Carlos) Carroll

Review of the Draft Biological Report

Does the draft report provide an adequate and concise overview of gray wolf (Canis lupus) taxonomy, biology, and ecology as well as the changes in the biological status (range, distribution, abundance) of the gray wolf in the contiguous 48 United States over the last several decades?
Please identify any oversights or omissions of data or information, and their relevance to the report. Are there other sources of information or studies that were not included that are relevant to the biological report? What are they and how are they relevant?

Firstly, I want to acknowledge the challenge that conservation of large carnivore species such as the wolf poses for listing, delisting, and recovery planning, and hence the difficulties that the Service faced when preparing this rule. I also want to commend the Service and the contractor for instituting a rigorous peer review process.

I found the biological report generally accurate but with some key omissions that need to be addressed in order for the report to provide a comprehensive information base for the rule. My suggestions are as follows:

1) The report should reference the recently released National Academies of Sciences report (NASEM 2019; citations listed at end of review) on the Mexican wolf and red wolf. Although these taxa themselves are not part of the listed entity that is the subject of the rule, the report contains much relevant information on wolf taxonomy and distribution in general.

2) I have provided citations for several other recently published relevant research papers (Hedrick et al. 2019, Hendricks et al. 2019, Robinson et al. 2019) at the end of this review.

3) The report's discussion of sustainable mortality rates (p 6) should clarify that broad-scale source-sink dynamics over areas larger than many demographic study areas can cause high local mortality rates to appear sustainable because the population is being sustained by immigration from source habitat.

4) The report states (p 6) that wolves "quickly expand and colonize nearby areas or even those that are separated by a broad area of unsuitable habitat". Th term "quickly" is ambiguous, I suggest replacing with e.g. "within decades" (which is quick for range expansion in a large mammal but may not seem quick to some readers). The rapidity of population establishment in new areas varies with the extent of intervening unsuitable habitat between the source

population and newly colonized area, as witnessed in the delay between initial dispersals and pack establishment in the Cascade Range of the Pacific Northwest and California.

5) The report states that forest is a key characteristic defining suitable habitat for wolves (p 7). Although forest types have emerged as predictive in several empirical habitat models in the western US, wolves globally are also found commonly in non-forest (e.g. grassland) ecosystems. It is likely that forest habitat is an indirect proxy for the reduced vulnerability to anthropogenic mortality in forested vs open habitat, not a direct habitat component.

6) Similarly, the statement (p 7) that wolves were absent from arid deserts and mountaintops should be qualified (e.g. changed to "existed at lower densities") since wolves are known to inhabit arid ecosystems, and available historic accounts for wolf distribution in the western US are not comprehensive.

7) It is increasingly evident from range expansion in both the Great Lakes region (e.g., central Wisconsin) and Europe that wolves can persist in semi-developed landscapes if anthropogenic mortality is kept relatively low.

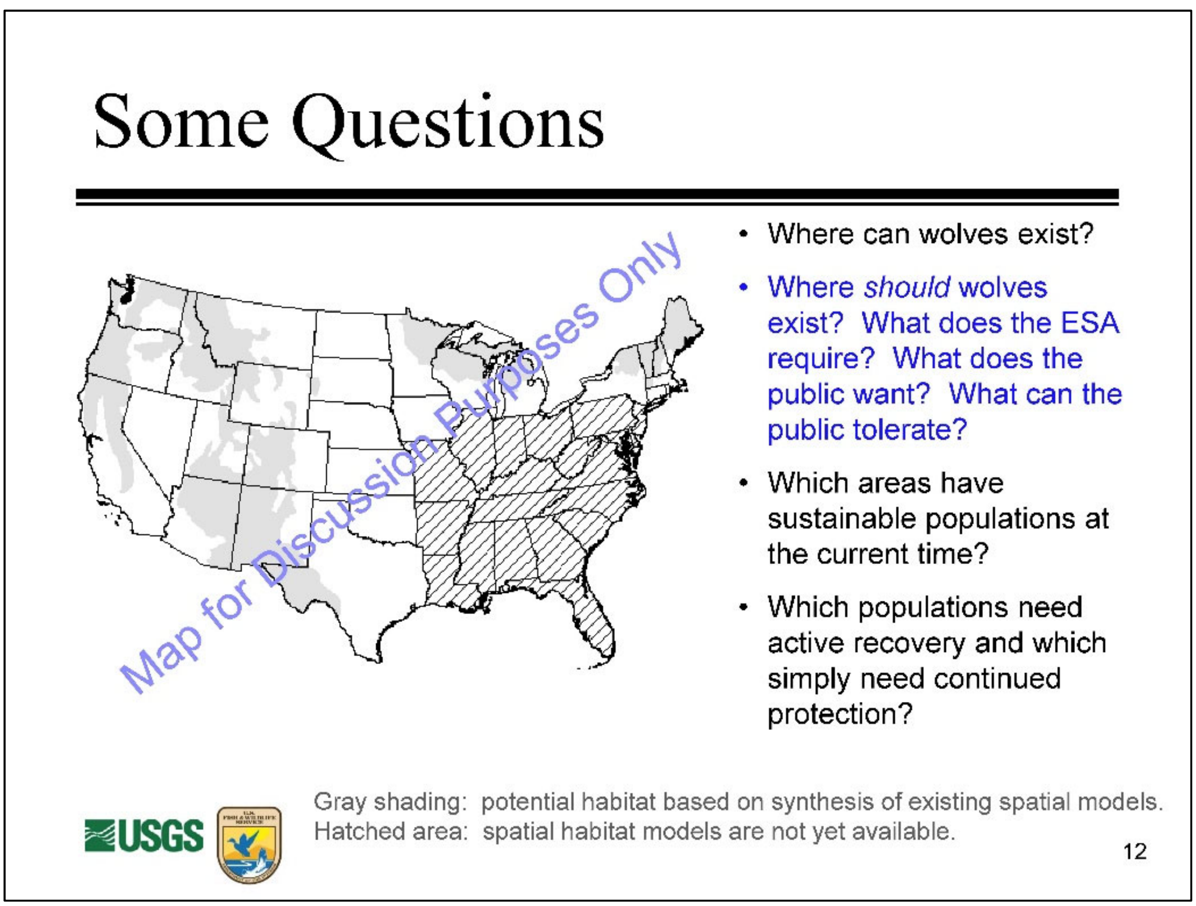
8) The report (p 23 and elsewhere) provides an incomplete and simplistic characterization of wolf metapopulation structure, which is characterized by complex genetic clines driven by historical biogeographic factors, isolation by distance, and association with particular ecosystems. I review this at greater length below in the context of its implications for the delisting rule.

9) The report combines detailed description of the distribution of suitable wolf habitat in some regions with the almost complete omission of such information in other regions. In this respect, the report is inconsistent with previous iterations of wolf listing and delisting rules, which at least attempted a more geographically complete treatment of the distribution of suitable habitat. An example of such an omission is the lack of discussion of the distribution of suitable habitat in Colorado and Utah, when compared for example to discussion of habitat distribution in the Pacific Northwest and Great Lakes region, where even unoccupied suitable areas such as the Olympic Range are described. Regarding Colorado and Utah, the information in the report comprises only one paragraph on historical extirpation in those states, and then one sentence on recent recolonization process (p 21: "Since the early 2000s, there have been ten confirmed

records of individual wolves in South Dakota, nine in Utah, and five in Colorado (Jimenez et al. 2017; USFWS 2018, unpublished data)”). It would be relevant to mention specific dispersal events such as the 2014 dispersal from the Northern Rockies to Arizona that are particularly relevant to wolf metapopulation structure. Similarly, the report states (p 20) “Although potential source populations of the eastern wolf phenotype (as discussed in Taxonomy, above) occur north of the St. Lawrence River in Quebec and Ontario, Canada, within the recorded dispersal capability of a wolf (Thiel and Wydeven 2012), and eastern wolf-coyote hybrids do occasionally disperse southward into the Northeast from Canada, we currently have no information indicating that wolves have formed breeding pairs in the Northeast U.S.”. This omits mention of the fact that dispersal of wolves (not wolf-coyote hybrids) from Quebec to the northeastern US has been documented since 2004 (Villemure & Jolicoeur 2004).

10) I would suggest that the report include a comprehensive map of suitable wolf habitat throughout the extent of the listed entity, such as was prepared by the USGS in 2011 (Runge 2011, Figure 1 below).

Figure 1. Map of potential habitat from Runge (2011).



Review of the Proposed Rule

Are there demonstrable errors of fact or interpretation? Have the authors of the proposed delisting rule provided reasonable and scientifically sound interpretations and syntheses from the scientific information presented in the draft biological report and the proposed rule? Are there instances in the proposed rule where a different but equally reasonable and sound interpretation might be reached that differs from that provided by the Service? If any instances are found where this is the case, please provide the specifics regarding those particular concerns.

Although I found the biological report on the whole accurate (with some notable omissions), I found that the proposed rule did not build on the assembled scientific information to provide coherent factual support or logical explanation for the agency's conclusions. Peer reviewers have been instructed not to provide advice on policy, but were requested to assess the logic of the rule's "assumptions, arguments, and conclusions". Therefore, while I do not attempt comprehensive legal or policy review here, I do discuss the policy context where necessary to place factual and logical omissions and errors in context, and to demonstrate why these omissions and errors are relevant and important in leading the agency to reach an erroneous conclusion.

The key conclusion put forth in the proposed rule is that the loss of all gray wolf populations outside the core Great Lakes occupied range in Minnesota and Wisconsin would not represent a threat to the listed entity (i.e., would not trigger relisting as threatened or endangered), as other populations are not essential to viability of the species. See p. 9683 "The metapopulation in the Great Lakes area contains sufficient resiliency, redundancy, and representation to sustain populations within the gray wolf entity over time. Therefore, we conclude that the relatively few wolves that occur outside the Great Lakes area within the gray wolf entity, including those in the west coast States and lone dispersers in other States, are not necessary for the recovered status of the gray wolf entity." The validity of the rule ultimately rests on this central claim, and the authors fall back on it to defend any errors or omissions in other aspects of the analysis. That is, the above claim implies that any deficiencies in the rule's analysis of wolf populations in other regions such as Pacific Northwest is immaterial given that these populations are not necessary for delisting of gray wolves. However, this central claim is itself contingent on factual omissions and errors which misinterpret key aspects of wolf ecology and genetics as I detail below. This renders the logical arguments and conclusions of the rule as a whole suspect.

The history of gray wolf listing under the ESA is relevant here. The Service shifted in 1978 from listing gray wolf subspecies individually to listing the gray wolf at the species level, but stated "The Service, however, can offer the firmest assurance that it will continue to recognize valid biological subspecies for purposes of its research and conservation programs." (43 FR 9610). More than four decades later, the Service is in effect attempting an end run around this earlier commitment to geographically comprehensive conservation, by proposing to delist the species *Canis lupus* based on the recovery of one subspecies (*C. l. nubilus* following Nowak (1995), although the taxonomy of Great Lakes wolves remains in dispute). The approach quoted above would, for example, not consider as important any threats to the gray wolf ecotype inhabiting Pacific coastal rainforest ecosystems (an ecotype overlapping the distribution of the subspecies Young and Goldman (1944) termed *C. l. fuscus*) that has colonized the US Pacific Northwest (Hendricks et al 2018).

The rule justifies this inconsistency with previous Service actions regarding gray wolf listing and delisting based on two conclusions (p 9685): 1) wolves disperse widely, and therefore can be treated as a single metapopulation spanning the entire continent, and 2) any peripheral populations such as those in the Pacific Northwest are connected to larger populations outside the US (e.g., in Canada) which will purportedly ensure their continued viability. I describe below why these conclusions are erroneous.

Does the proposed rule provide an adequate review and analysis of the factors relating to the persistence of the gray wolf population currently listed under the ESA in the contiguous 48-States (human-caused mortality, habitat and prey availability, disease and predation, and effects of climate change)?
Please identify any oversights or omissions of data or information, and their relevance to the assessment. Are there other sources of information or studies that were not included that are relevant to the proposed rule and, if so, what are they and how are they relevant?
<u>Treatment of genetic threats</u>

A key oversight in the proposed rule lies in the lack of detail and rigor in the treatment of genetic issues. The rule states (p 9658): “While range reduction may also result in changes in genetic diversity and gene flow, or cause changes in population demographics, we do not address genetic diversity or demographics of the gray wolf entity below because we are not aware of any information indicating that these are potential threats to wolves in the gray wolf entity. Wolves in the entity appear to be genetically and demographically healthy. Not only do they include wolves of differing and mixed genetic origin, but they exist as part of larger metapopulations—adverse effects resulting from genetic drift, demographic shifts, and local environmental fluctuations can be countered by influxes of individuals and their genetic diversity from other subpopulations of the metapopulation."

This description represents an extreme oversimplification of the genetic structure of wolf metapopulations at regional and continental extents. Gray wolf metapopulation structure is characterized by complex genetic clines at several spatial scales, driven by historical biogeographic factors, isolation by distance, and association with particular ecosystems. It is not sufficient to simply give a boilerplate summary such as the above without providing detail as to existing knowledge of genetic structure and the potential for delisting to affect metapopulation demography and genetics. I provide here a few relevant citations as a place to start. VonHoldt et al. (2011) found that that "even within gray wolves, a species with high dispersal abilities, regional and continental patterns of genetic subdivision are found." Schweizer et al. (2016) found "that local adaptation can occur despite gene flow in a highly mobile species". Leonard et al (2005) documented patterns of genetic intergradation in wolves within the contiguous US, and Carmichael et al. (2007) found that "genetic structure in wolves correlates strongly to transitions in habitat type". Hendricks et al. (2019) focused specifically on genetic substructure and adaptive uniqueness in North American gray wolves and reviewed conservation strategies that would provide increased protection for the diversity of *C. lupus* ecotypes in the contiguous US. All of these references except for the last two are listed in the biological report, so the issue here is that the proposed rule misinterprets the state of knowledge and/or does not address the implications of these studies.

A large variation in genetically effective population size and consequently severity of potential genetic threats exists between large core populations (e.g., Minnesota) and small peripheral populations (e.g., Pacific Northwest). Genetic threats to persistence have been documented in small wolf populations such as those occurring on Isle Royale, in Scandinavia, or in the Mexican wolf subspecies (Hedrick et al. 2014, Hedrick 2017) and should be anticipated in other relatively isolated wolf populations over time. Two recent studies of the Isle Royale population provide broadly applicable insights concerning management of genetic variation in fragmented landscapes (Hedrick et al. 2019, Robinson et al. 2019). Robinson et al. (2019) concluded that “the risk of inbreeding depression is higher for genomes recently originating from a historically large population” as would be the case for recolonizing wolf populations in the western US. Delisting, due to its potential effect on anthropogenic mortality in dispersing wolves, could be reasonably projected to reduce metapopulation connectivity, so this risk needs to be assessed in detail. As Wayne & Hedrick (2011) conclude “Genetic rescue is a reality in large carnivores and genetically effective migration is a critical variable in population management.”

Have we (the Service) adequately considered the impacts of range reduction (i.e., lost historical range) on the long-term viability of the gray wolf in its remaining range in the lower-48 states (outside of the northern Rocky Mountains) and, if not, what information is missing and how is it relevant?

The importance of the omissions and misinterpretations contained within the proposed rule’s treatment of range can best be understood in the context of how the term “range” as used in the ESA has been interpreted in the past. The ESA’s defines an endangered species as one “at risk of extinction throughout all or a significant portion of its range”. The Service has in the past suffered defeat in the courts, especially in relation to efforts to delist large carnivores, as its interpretation of the “significant portion of its range” clause was judged inadequate because it effectively rendered the clause superfluous. Courts to date have deferred to the Service’s interpretation of “range” as indicating current rather than historic range, with the caveat that the effect of loss of historic range on the species must be substantively considered. Most recently, in two cases, including one referenced in the rule (*Desert Survivors v. Dep’t of the Interior*), the courts have additionally required that the Service’s interpretation of “significant” in “significant portion of its range” must consider historic range, and not render the “significant portion of its range” clause duplicative of “endangered or threatened throughout” a species range. My review of the proposed rule’s treatment of the ecological concept of range and the specifics of range dynamics in the gray wolf suggests that it fails the test because it does not substantively consider significance and effects of historic range loss in a manner which is not duplicative to assessing whether a species is at risk throughout its range.

Range as an ecological concept is both temporally dynamic and scale-dependent. As any delineation of historic range is contingent on timeframe, so is the definition of current range. The delineation of range also is dependent on spatial scale. Gaston & Fuller (2009) reviewed the "difficulties posed by range size measurement (especially those of range discontinuities when measuring EOO, and spatial scale when measuring AOO)", with extent of occurrence (EOO) being the area which lies within the outermost geographic limits to the occurrence of a species, and the area of occupancy (AOO) being where within those outermost limits over which it actually occurs.

Climate change is an additional factor that can accentuate the rate of change in a species' range (Chen et al. 2011). The proposed rule contains only a brief statement that wolves will be unaffected by climate change, but this ignores issues regarding conservation of ecotypic variation and adaptive potential within the species. Since wolf populations are known to be associated with specific ecosystems (Carmichael et al. 2007), shifts in ecosystems caused by climate change may be expected to alter distribution and viability of certain wolf ecotypes.

For an example of the implications of a dynamic range, in 2013 when the Service last issued a proposed delisting rule for the wolf, no breeding pairs or packs were present in California, and thus under the Service's definition, it was not included within the range of the species. However, in 2019 at the time of the current proposed rule, at least one pack or breeding pair is known to inhabit California, so the state is treated as within the species' range.

Occasional dispersing wolves have been documented in several states in the western, midwestern, and northeastern US. Given that many of these wolves are not collared, there is inherent uncertainty as to occupancy status in these areas in the period between initial exploratory dispersals and first establishment of a breeding pair. If implementation of the current proposed rule were to be delayed due to litigation, which seems possible given the history of wolf delisting efforts, it is likely that breeding pairs will exist in some areas which are now experiencing exploratory dispersals, and thus these areas will become part of the species' current range.

This example demonstrates the definition of temporally dynamic species' distribution or range based on single snapshot in time is inherently problematic. Leaving aside the fact that it does not consider the fate of species such as the California condor for which the last individuals were taken into captivity, leaving no current range, such a static interpretation of a dynamic ecological status also creates perverse incentives, in that an area which holds a small population of a species would be exempted from actions related to species' recovery once those individuals had been killed. This interpretation also creates incentives for legislation such as enacted in Utah which seek to prevent establishment of individuals of a species within a state, thus hindering recovery. Utah Senate Bill 36 directs that "The division (Division of Wildlife Resources] shall manage wolves to prevent the establishment of a viable pack in all areas of the state where the wolf is not listed as threatened or endangered . . . until the wolf is completely delisted under the Act and removed from federal control in the entire state."

The treatment of range (and significance as described below) in the proposed wolf rule is markedly inconsistent with how range is treated for other species that have been delisted or proposed for delisting under ESA. When the Bald Eagle was delisted in 2007, populations had been restored in five regional recovery areas covering virtually all of the birds' historic range. Even limiting the comparison to other large mammalian carnivores, the Service's grizzly bear recovery strategy does not claim that recovery in one region (e.g., the NCDE or Yellowstone ecosystem) renders recovery efforts elsewhere unnecessary. In fact, areas such as the North Cascades which may currently host only transient individuals are nonetheless the object of substantial recovery planning efforts.

The rule should address these omissions in part by presenting information comprehensively evaluating the significance of loss of historically-occupied range on the genetic and demographic structure of the wolf metapopulation as a whole and within specific ecotypes and subspecies. The rule should also assess the significance of range loss to the broader suite of "esthetic, ecological, educational, historical, recreational, and scientific" values that the ESA's preamble describes as among the values which the act seeks to conserve.

The authors of the rule mention (p 9684) that they have not yet fully considered recent court rulings on the issue of range and significance, and state that "for the purposes of the analysis here, in determining whether any portions may warrant further consideration because they may be significant, we screen by looking for portions of the species' range that could be significant under **any reasonable definition of ''significant''** that relates to the conservation of the gray wolf entity"(emphasis added). I describe below why the rule's proposed treatment of significance does not meet the test imposed by the courts, and I suggest that a reasonable definition that better meets the test would be derived from the definition already in use by the Service to define Distinct Population Segments (DPS)(USFWS/NMFS 1996). However, this discussion first requires consideration the relevance of the 3Rs to wolf recovery.

Resiliency, redundancy and representation

The rule makes the case that it has incorporated consideration of the "3Rs" in its assessment. For example, on p. 9685: "gray wolves are a highly adaptable generalist species capable of long distance-dispersal. Therefore, we do not find that these portions may be significant under any reasonable definition of ''significant'' because they are not biologically important to the gray wolf entity in terms of its resiliency, redundancy, or representation" and, regarding the Pacific Northwest populations: "the gray wolves in this area are an extension of a large metapopulation of wolves in the northern Rocky Mountains and western Canada (i.e., they are not an isolated population with unique or markedly different genetic or phenotypic traits that is evolving separate from other wolf populations). Therefore, for the purpose of assessing the status of the gray wolf entity under the Act, we do not find that this portion may be significant under any reasonable definition of ''significant'' because it is not biologically important to the gray wolf entity in terms of its resiliency, redundancy, or representation."

These statements misinterpret both wolf ecology and the 3Rs themselves. Firstly, as I have described above, describing *C. lupus* as a "generalist" with long-distance dispersal capabilities effectively ignores ecotypic and subspecific variation and the fact that dispersal, even over distances commonly seen in wolves, does not erase metapopulation genetic structure over a continental extent. Secondly, the conservation principles of resiliency, redundancy and representation (the '3R' criteria) as developed by Shaffer and Stein (2000) are quite different than as presented in the rule. The 3Rs in essence state that, to be considered recovered, a species should be present in many large populations arrayed across a range of ecological settings. Redundancy of subpopulations in a metapopulation enhances the viability of each due in part to “spreading of risk”, since episodic threats such as disease outbreaks or long-term trends such as climate change may not affect all subpopulations equally.

Although representation and preservation of genetic diversity and genetic evolutionary potential are important goals, they form only part of Shaffer and Stein (2000)’s concept of representation, which they defined as a species’ presence across the diversity of ecosystems inhabited by the species and by the species’ role in ecosystem processes. Representation applies primarily to a population itself (e.g., by examining whether the species absence in a portion of its range would have significant ecological consequences or whether a given portion of a species range includes ecosystem types not found elsewhere in the species range) rather than to a population’s contribution to the entire species. Representation should thus be the factor among the 3Rs of most assistance in evaluating whether a portion of a species current range is significant (Carroll et al. 2010).

Shaffer and Stein (2000) conclusion regarding the importance of representation is particularly relevant to the wolf: “we will be challenged to recognize our conservation targets in a way that captures the full spectrum of such natural variation across the landscape, and on a geographic scale that can truly encompass this ecological diversity and its attendant processes. The principle of representation—saving some of everything—will require identifying conservation targets not simply as species and communities but as the complexes of populations, communities, and environmental settings that are the true weave of biodiversity.”

Significance in the context of distinct population segments (DPS)

Amendments to the ESA in 1978 allowed distinct population segments (DPS) of vertebrates, as well as species and subspecies, to be protected under the Act. The Services (FWS and NMFS) developed a policy in 1996 defining what constitutes a DPS based on 2 criteria: discreteness and significance. The criteria for discreteness and significance in the DPS policy are relevant for the evaluation of significance in the context of the proposed wolf rule, both in a general sense and because the rule reviews whether certain regions (the Pacific Northwest) would qualify for designation as DPS. Discreteness requires either marked separation from other populations of the same taxon as a consequence of physical, physiological, ecological, or behavioral factors (which may be demonstrated by genetic or morphological discontinuity), or delimitation by international governmental boundaries between nations with significant differences in management or conservation status of the species (Waples et al. 2018). Significance in turn can be determined by any of the following: persistence of the discrete segment in an ecological setting unusual or unique for the taxon; evidence that loss of the discrete segment would result in a significant gap in the range of the taxon; evidence that the discrete segment represents the only surviving natural occurrence of a taxon that may be more abundant elsewhere as an introduced population outside its historical range; and evidence that the discrete segment differs markedly from other populations of the taxon in its genetic characteristics. (USFWS & NMFS 1996, Waples et al. 2018). Even given the detailed qualitative definitions for discreteness and significance in the DPS policy, various authors have proposed different levels of emphasis and quantitative thresholds for the degree of isolation (distinctness) and adaptation (significance) that merit DPS designation (as summarized in Fig. 2 below (adapted from Waples (2018)) in the context of Evolutionarily Significant Units (ESUs)).

A reasonable definition of "significance" that would meet the requirements imposed by *Desert Survivors v. Dep't of the Interior* would be based on the criteria that the Service uses to determine significance when evaluating whether a region qualifies as a distinct population segment (DPS)(Fig. 2). A rigorous example of such an evaluation applied to a wolf taxon (the red wolf (*C. rufus*)) is given in Waples et al. (2018). However, if a region is evaluated in this manner and fails to meet the significance test for DPS status (i.e., a distinct listable entity), then the second step would be to evaluate whether the region qualified as a significant portion within a larger listable entity due to notable ecological or other characteristics as described above in the context of DPS criteria (Fig. 2).

When considering significance in the context of DPS designations for a particular species, the Services is required to conduct a well-reasoned analysis that is consistent with analyses conducted for other species as well as with previous treatment of that particular taxon, or rationally explains any inconsistencies. This is not the case however with the proposed rule, as is shown by a comparison of the current rule to previous wolf planning efforts.

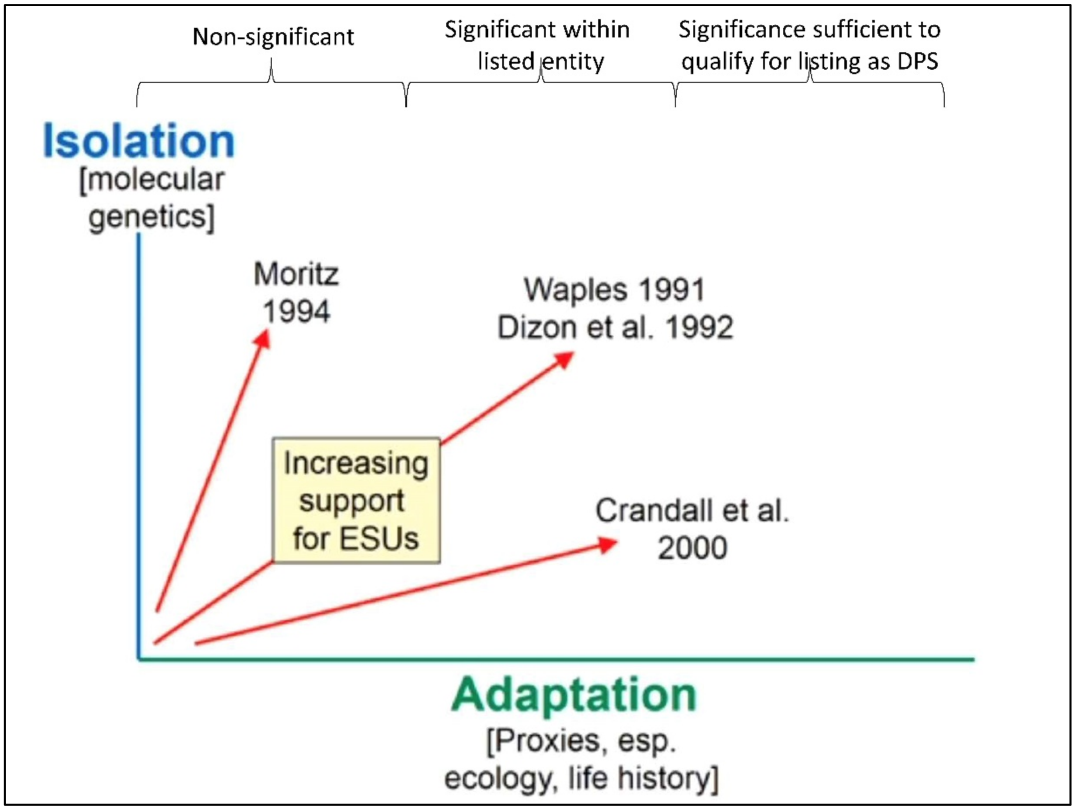
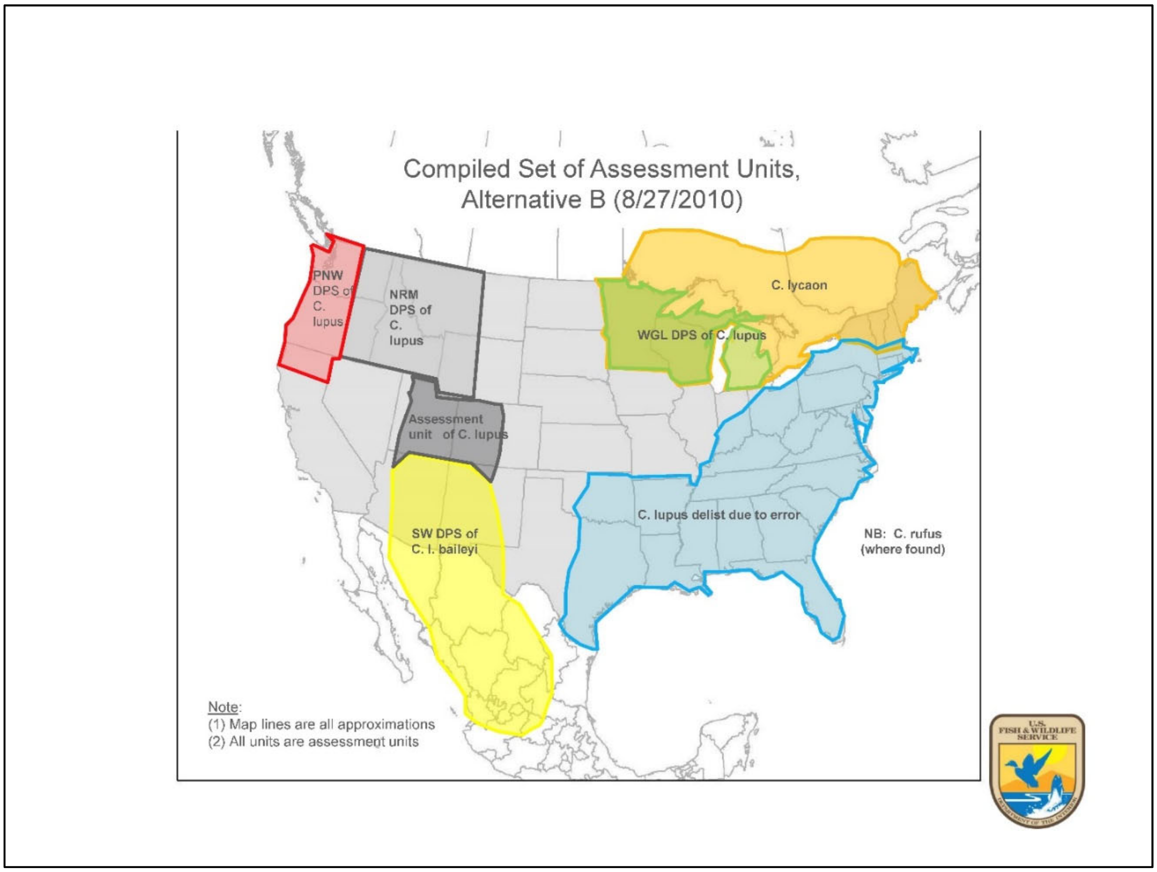


Figure 2. Conceptual diagram comparing several systems for ESU identification, adapted from Waples (2018), with upper brackets added for this review.

In 2008, the FWS embarked on an effort to develop a National Wolf Strategy through the use of a "Structured Decision Making" (SDM) process designed to develop a comprehensive strategy for gray wolf conservation by identifying appropriate wolf listing units within the broader continental distribution of the species as a whole (76 Fed. Reg. 26086). Despite its flaws, this process at least provided a comprehensive analysis of what recovery efforts would be appropriate in the different regions which still held suitable habitat for the species. This current proposed rule, in contrast, omits substantive treatment of two regions which were previously considered to merit consideration because they hold substantial suitable habitat: the Colorado/Utah assessment unit and the area of the northeast US proposed in the SDM process to be occupied by the eastern wolf (putative *C. lycaon*). Additionally, although the current rule includes an analysis of whether the Pacific Northwest qualifies as DPS, this analysis is flawed as described below.

Figure 3. Regional assessment units for the gray wolf developed as part of the 2008-2011 SDM process (Runge 2011).



<u>Consideration of a potential Pacific Northwest DPS</u>

The following text from p 9685 (emphasis and numbers in brackets added) encapsulates within a single paragraph many of the misinterpretations of the 3Rs, distinctness, significance, and wolf biology present in the rule:

> "the west coast portion of the gray wolf entity, where wolves exist in small numbers in California, western Oregon, and western Washington, also is not biologically important to the gray wolf entity in terms of resiliency, redundancy, or representation. This portion is not important to the gray wolf entity in terms of resiliency or redundancy because [1] **wolves occur in small numbers in this portion and include only a few breeding pairs**. Because these wolves represent the expanding front of a recovered and stable source metapopulation, and are

therefore [2] **not an independent population** within the gray wolf entity, the small number of wolves there [3] **do not contribute meaningfully to the ability of any population**, in the NRM or Great Lakes area, **to withstand stochastic events**, nor to the entire entity's ability to withstand **catastrophic events**. This portion is also not important in terms of representation, because (1) [4] **gray wolves are a highly adaptable generalist carnivore capable of long-distance dispersal**, and (2) the [5] **gray wolves in this area are an extension of a large metapopulation** of wolves in the northern Rocky Mountains and western Canada (i.e., they are not an [6] **isolated population with unique or markedly different genetic or phenotypic traits that is evolving separate from other wolf populations**)."

The statements identified by numbered elements are erroneous for several reasons. In terms of items [1] and [3], the current size of these recolonizing populations does not indicate their size and demographic contribution to the metapopulation over the long term, which will be determined by extent of suitable habitat and other factors. Despite the current small population size, the existence of such a peripheral (and likely larger) population at a future time could well contribute to metapopulation resiliency, e.g., by its differential exposure to disease outbreaks or climate change. Even if the Service considers "range" (spatial distribution) only in terms of "current range", that does not imply that they should only consider population abundance as of the year of the proposed rule, rather than over the foreseeable future.

In terms of [2] and [6], discreteness and significance of a population does not require complete reproductive isolation from other populations, as can be seen in designation of regional salmon populations as valid ESUs despite the fact that a portion of each population will stray between ESUs (Waples 1996, 2018). In 2007, the rule to delist the NRM wolf DPS correctly stated that the "the DPS policy does not require complete separation of one DPS from other populations, but instead requires some ''marked separation.'' Thus, if occasional individual wolves or packs disperse among populations, the NRM DPS could still display the required discreteness. Based on the information presented above, we have determined that NRM gray wolves are markedly separated from all other gray wolf populations in the U.S." (73 FR 10519).

It is inconsistent and illogical for the Service to now argue that such occasional dispersal prevents recognition of a DPS.

Secondly, the coastal rainforest ecotype which is the source of a portion of the individuals comprising the Pacific Northwest wolf populations, has been shown to possess “markedly different genetic or phenotypic traits” (Hendricks et al. 2018). Hendricks et al. (2019) concluded that wolf packs in Washington state that have a dominant coastal ancestry should be a priority for conservation given their unique evolutionary heritage and adaptations. Additionally, the criterion for significance is for a species to **inhabit** a unique ecological setting, and does not require that a recolonizing population already be “genetically adapted to a unique ecological setting”.

Lastly, in terms of items [4] and [5], as described above, characterization of the gray wolf simply as a “generalist” and vagile species, as if this implies lack of significant variation within the continental extent of the species’ distribution, is not consistent with data showing substantial genetic and ecotypic variation within the metapopulation. Hendricks et al. (2019) reviews the multiple studies demonstrating that North American wolves are morphologically and genetically differentiated on a local scale. Rather than erroneously claiming that wolves form an undifferentiated continent-wide metapopulation, the Service should use information on the extent and nature of local adaptation to inform conservation actions to preserve the evolutionary potential and adaptive capacity of gray wolf populations.

Additionally, the Service cannot evade the ESA’s mandate to protect US populations of a species by pointing to populations present in other nations, especially as is the case where the international boundary marks significant differences in management and conservation status of the species. Such contrasts across international boundaries are common for large carnivores (Thornton et al. 2018). The gray wolf is widely hunted in both British Columbia and Alberta, and rapid suburban/exurban development of areas in southern British Columbia such as the Okanagan and Fraser valley is reducing and will continue to reduce connectivity between Canada and the US Pacific Northwest (Krosby et al. 2016). In terms of connectivity between populations in the Pacific Northwest (PNW) and the Northern Rocky Mountains (NRM), the proposed rule states that the NRM is legally a distinct entity from the listed entity so the region's management (e.g., whether hunting regulations l;imit the likelihood of dispersal to other regions) will not be addressed in the rule, but the rule then claims that the NRM population's assumed demographic support to PNW populations via dispersal renders the PNW populations superfluous to the listed entity as a whole. This inconsistent treatment of the role of the NRM population is problematic.

In contrast to approach taken in the current rule, Waples et al. (2018) provides an example of a rigorous application of the discreteness and significance tests to evaluating whether a wolf taxon constitutes a valid DPS. Applying the same process to Pacific Northwest wolf population, I would conclude that marked separation can be established as a consequence of up to four factors: physical (separation by larger inland populations by areas of unsuitable habitat), ecological (occupation of coastal rainforest ecosystems), genetic (discontinuity in neutral molecular genetic data as established by Hendricks et al. (2018)), and due to international governmental boundaries which separate US populations from coastal rainforest wolves in Canada. Once discreteness has been established it is only necessary to meet a single significance element to be considered a DPS (Waples et al. 2018). However, Pacific Northwest wolves merit significance both due to their persistence in a unique ecological setting, which is used as a proxy for adaptive genetic differences, and due to the fact that loss of the population would result in a significant gap in the range of the taxon.

Omission of discussion of the Colorado/Utah region and the northeastern US

The rule is inconsistent in its treatment of unoccupied suitable habitat. For example, areas currently unoccupied by wolves such the Olympic Range of Washington or unoccupied areas within the Great Lakes region are described and considered in detail (p 9663-4), but other regions holding large areas of suitable wolf habitat which were considered in depth in earlier wolf rulemaking, particularly Colorado/Utah and the northeast US, are only mentioned in passing in the current rule.

The rule is notably silent on the distribution of suitable habitat in northeastern US. In this respect, the current rule is inconsistent with previous Service documents such as the 1992 Great lakes wolf recovery plan, the documents developed during the 2008-2011 structured decision-making process (e.g. Runge 2011), and the 2013 proposed national wolf delisting rule, all of which included analysis of habitat in the northeastern US. The 2013 proposed wolf delisting rule (78 FR 35664) was never finalized because scientific peer reviewers found that the rule's treatment of wolf taxonomy in the northeast US was flawed (National Center for Ecological Analysis and Synthesis 2014). Perhaps in an attempt to avoid this controversy, these taxonomic questions regarding northeast wolves, and any other aspect of the role of the northeast US in recovery, are ignored in the rule, based on the previously-quoted assertion that any populations outside the Great Lakes region are unnecessary for recovery of the species.

Currently the rule only states "The [1992] Revised Recovery Plan identified potential gray wolf reestablishment areas in northern Wisconsin, the Upper Peninsula of Michigan, the Adirondack Forest Preserve of New York, a small area in eastern Maine, and a larger area of northwestern Maine and adjacent northern New Hampshire" but does not further discuss habitat or potential recovery in the eastern US outside of the Great Lakes. At a minimum, a detailed rationale is needed for why the current proposed rule is inconsistent with the 2013 proposed rule and previous recovery plans in omitting treatment of the northeast US.

Secondly, the rule omits discussion of availability of suitable habitat or potential for wolf recovery in Colorado or Utah except to state "Confirmed records of individual wolves have been reported from North Dakota, South Dakota, Utah, Colorado, Nevada, Missouri, Indiana, Illinois, Nebraska, and Kansas." As shown in Figure 3, the Colorado/Utah region qualified as an "assessment unit" in the 2008-2011 SDM process. Habitat modeling has suggested that Colorado alone could support a population of over 1000 wolves, which would constitute the second or third largest state wolf population in the contiguous US, and thus a "core" population for sustaining the species' viability (Carroll et al. 2006).

Additionally, the Colorado/Utah region was designated an "assessment unit" in the 2008-2011 SDM process because of its key location connecting northern gray wolves with southwestern wolf populations. Although largely spurious arguments have been made that such dispersal could genetically swamp Mexican wolf populations, Hedrick (2018) demonstrated why this connectivity would instead be a positive factor due to its potential to provide genetic rescue of Mexican wolf populations and enhance adaptive potential in both taxa. Such north-south connectivity is also likely to be important in facilitating range shifts under climate change, thus increasing resiliency of both species.

The recent NASEM report summarizes information on this topic as follows: "Hendricks et al. (2018) agreed that the southern clade had a wide distribution, which would imply that gene flow was naturally extensive across the recognized limit of the subspecies and that there may have been an admixture of Mexican gray wolves with other wolf populations to the north. It is generally accepted that with highly mobile species such as wolves, subspecies boundaries may include large zones of intergradation (Schweizer et al., 2016b) and that **admixture in individuals within such zones might enhance their adaptive potential** (Hedrick, 2013)…individuals with Mexican gray wolf ancestry coexisted with Northern Rocky Mountain wolves (*C. l. irremotus*) outside of the presently defined Mexican gray wolf historical range and, therefore, that **these areas may represent appropriate habitat for both wolf ecotypes**." (NASEM 2019, emphasis added).

For those regions (Colorado/Utah, the northeastern US) where breeding pairs or packs are not yet documented, but multiple exploratory dispersals have been recorded, the ESA's mandate for "institutionalized caution" towards preventing extinction would suggest in-depth consideration and potentially inclusion within the definition of "range". Applying the discreteness and significance evaluation process of Waples et al. (2018) to the Colorado/Utah region and the northeastern US, I would conclude that marked separation can be established as a consequence of several factors: physical (separation from other populations by areas of unsuitable habitat), ecological (occupation of unique ecosystems as delineated by ecoregional boundaries and related data (Waples et al. 2018)), and in the case of the northeastern US, due to international governmental boundaries which separate the northeast US from eastern wolves in Canada. Both the Colorado/Utah region and the northeastern US hold areas of suitable habitat which may merit significance due to their unique ecological setting and the fact that loss of the population would result in a significant gap in the range of the taxon.

Although the Service may ultimately conclude against designation of a DPS (i.e., a separate listable entity) in these regions, Colorado/Utah and the northeastern US do meet one or more of the criteria for "significance", and thus merit evaluation as to their significance

within a listable entity in the context of *Desert Survivors v. Dep't of the Interio*r (Fig. 2). If the Service concludes against considering these regions significant, the rule needs to rationally explain in some detail why this decision was reached, and explain inconsistencies with previous wolf recovery planning and rulemaking.

The current proposed rule represents a departure from previous delisting rules for the gray wolf and other species in that all but one region of a continent-spanning current range is found to be non-significant to the listed entity. This interpretation is inconsistent with the requirement imposed by *Desert Survivors v. Dep't of the Interio*r, which requires the Service to substantively consider the significance of portions of the range in a manner which is not duplicative to considering whether a species as a whole is at risk of extinction. If applied generally to other species, the interpretation proposed in the current wolf rule would represent a major scaling back of ESA recovery efforts, one which is clearly at odds with the purpose of the Act.

Is it reasonable for the Service to conclude that the approach of Michigan, Wisconsin, and Minnesota to wolf management, as described in their Plans and the proposed rule and in the context of wolf management in the Western Great Lakes area, are likely to maintain a viable wolf population in the Western Great Lakes area into the future?

The rule does in my view provide a rational explanation as to why it concludes that wolf management in the Great Lakes states would lead to a regional wolf population which was not subject to demographic risks in the short term. However, while it is clear that the ESA does not require species to be restored "everywhere", this is not the same as concluding in favor of the central argument of the proposed rule, which is that recovery in one region (the Great Lakes) is sufficient to delist a species formerly distributed across the continent. The literature reviewed above documenting substantial regional-scale genetic variation in North American wolves leads me to conclude that the rule does not provided adequate support for either the conclusion (p 9683) that the "metapopulation in the Great Lakes area contains sufficient resiliency, redundancy, and representation to sustain populations within the gray wolf entity over time", or the conclusion that "wolves that occur outside the Great Lakes area within the gray wolf entity, including those in the west coast States and lone dispersers in other States, are not necessary for the recovered status of the gray wolf entity."

My role as a peer reviewer here is to assess the factual support and logical coherence of the rule as currently written, not to assess whether a general conclusion, e.g. whether delisting is justified in the Great Lakes region, could be supported based on a more reasoned and narrowly-focused rule. In order to advance a novel minimalist interpretation of the ESA, the rule is forced into numerous factual misinterpretations and omissions regarding wolf demography and genetics, as well as unexplained inconsistencies with previous rulemaking. I sympathize with state management agencies in the Great Lakes, as the rule appears to be holding prospects for delisting in that region hostage to the Service's efforts to advance broader shifts in interpretation of the ESA for widely-distributed species.

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Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Clarification Questions and Responses

May 24, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 2 – Dr. Charles (Carlos) Carroll

GRAY WOLF 2019 PROPOSED RULE: PEER REVIEW

Clarification Questions

Peer Reviewer #2

- On p.2, item 3 (about high mortality sustained by immigration), is there empirical support for this happening in wolf populations that we should include?
- On p.7, the sentence that begins “A large variation in genetically effective population size exists….” Is there data for this assertion? Two of the three examples given (Isle Royale and Mexican wolf) are very different from the situation described in the proposed rule, in that the western wolves are small in number, but very much connected to (often dispersing from) larger populations.
- On p.10, the sentence that begins “The rule should address these omissions….” Is there information available about how the metapopulation or genetic structure has changed or been affected by range contraction that we should include?

1. On p.2, item 3 (about high mortality sustained by immigration), is there empirical support for this happening in wolf populations that we should include?

The prevalence of regional-scale source-sink population dynamics, which allow population persistence in high mortality sink areas via dispersal from low mortality source areas, is well documented in wolves as well as other large carnivores. Hayes and Harestad (2000) provide one of the earlier studies on this topic. Adams et al. (2008) and Schmidt et al. (2017) provide more recent examples, also from the boreal region. Smith et al. (2016) provide a more general discussion of the issue in relation to a population in the lower 48 states (Northern Rocky Mountains). Source-sink dynamics have been notable in the Northern Rocky Mountains, especially in northwest Montana and the Greater Yellowstone region, where much ungulate winter range lies outside of protected areas (Fritts and Carbyn 1995). Several studies of source-sink dynamics in mountain lion populations are also relevant to wolves (Stoner et al. 2006, Robinson et al. 2008, Andreasen et al. 2012).

2. On p.7, the sentence that begins "A large variation in genetically effective population size exists…." Is there data for this assertion? Two of the three examples given (Isle Royale and Mexican wolf) are very different from the situation described in the proposed rule, in that the western wolves are small in number, but very much connected to (often dispersing from) larger populations.

Firstly, I would challenge the authors to provide support for their statement that peripheral wolf populations in the western US are "very much connected to" each other. One cannot assume from the fact that a peripheral population is established by one or several initial dispersals from another source population, that therefore genetically effective dispersal between the two populations is either currently sufficient to prevent emergence of genetic threats, or that a sufficiently high dispersal rate will continue to be the case in the future in the face of regulatory changes (e.g., delisting) and ongoing landscape change (e.g., development patterns). The relatively slow pace of range expansion in the Pacific states suggests that even assuming that large suitable habitat areas (e.g., Olympic Range) are eventually colonized, some areas may continue to be only occasionally connected by genetically effective dispersal to the metapopulation as a whole.

Secondly, although the Mexican wolf serves primarily as an example of the prevalence of genetic threats in small wolf populations, both the Isle Royale and Scandinavian wolf populations provide examples of threats brought about by low but non-zero rates of dispersal from a larger source population. Hedrick et al. (2014; Table 4) lists dispersal of 9 wolves in 4 separate events to Isle Royale, and several other dispersals are known but incompletely documented, or have occurred subsequent to Hedrick et al. (2014)'s tally. Additionally, a wolf recently reintroduced to Isle Royale is known to have dispersed to the mainland across the ice. The southern Scandinavian population also has been documented to receive low rates of dispersal from larger source populations in Finland/Karelia (Bruford 2015, Laikre et al. 2016), comparable to what might be experienced by smaller populations in the western United States and elsewhere. I would also direct the authors to Robinson et al. (2019), who concluded that "the risk of inbreeding depression is higher for genomes recently originating from a historically large population" as would be the case for recolonizing wolf populations in the western US. Wayne & Hedrick (2011) also conclude "Genetic rescue is a reality in large carnivores and genetically effective migration is a critical variable in population management."

Given the accumulated weight of evidence concerning genetic threats to small populations from both these examples and analogous examples from other species, the burden of proof is on the authors of the rule should they contend that such effects would not be expected to occur in small and semi-isolated wolf populations such as occur in the lower 48 states.

3. On p.10, the sentence that begins "The rule should address these omissions…." Is there information available about how the metapopulation or genetic structure has changed or been affected by range contraction that we should include?

Despite the significant obstacles to developing a detailed description of historic (i.e., pre-range-contraction) metapopulation genetic structure of *Canis lupus* in North America, much progress has been made in recent years. Leonard et al. (2005) provides an early example of such an analysis, but all of the studies, especially Hendricks et al. (2019), cited in the paragraph below from the original review (p. 6) provide relevant information and cite other relevant literature. As Hendricks et al. (2019) state, while "the resolution of conservation genetic analyses has been limited until recently due to technological and computational challenges associated with genotyping multiple loci at once…development of high-throughput genotyping methods have enabled conservation genomics studies of wolves in North America". Therefore, one would be hard pressed to reconcile recent research cited below and in the original review with a characterization of the historic and current North American wolf population as genetically unstructured due to assumed high dispersal ability (as is done in the rule). Hendricks et al. (2019) focused specifically on genetic substructure and adaptive uniqueness in North American gray wolves, and review changes that occurred as the historic Holarctic distribution of the wolf across widely varying environments was reduced due to range contraction.

The related conclusion from my peer review that "Gray wolf metapopulation structure is characterized by complex genetic clines at several spatial scales, driven by historical biogeographic factors, isolation by distance, and association with particular ecosystems" is also supported by many recent studies. VonHoldt et al. (2011) found that that "even within gray wolves, a species with high dispersal abilities, regional and continental patterns of genetic subdivision are found." Schweizer et al. (2016) found "that local adaptation can occur despite gene flow in a highly mobile species". Leonard et al (2005) documented patterns of genetic intergradation in wolves within the contiguous US, and Carmichael et al. (2007) found that "genetic structure in wolves correlates strongly to transitions in habitat type". Given the geographic and ecotypic structure these studies document as present in the historic and current metapopulation, it follows that absence of wolves from specific geographies and ecosystems would necessarily result in changes in metapopulation and genetic structure.

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Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

May 24, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 3 – Adrian P. Wydeven

Review of *Gray Wolf Biological Report* by US Fish and Wildlife Service (2018)

In general the report provides a reasonable summary of taxonomy, ecology, biology, original distribution and current status of gray wolves (*Canis lupus*) across the US, but there are areas that need clarification, additional important literature that can be cited, and taxonomical treatment of wolves seems somewhat arbitrary. Some of the updating of literature is due to the publication of important works since the Review was written, but some involved overlooked publications.

I will discuss several of these concerns below. I will refer to the document as the Review. Additional publications to the original document will be listed below under "Additional Literature Citations". I will use initials USFWS for the U.S. Fish and Wildlife Service, and ESA for the Endangered Species Act.

The taxonomy discussion leaves some confusion on how the term eastern wolf is used. In the original listing of gray wolves in eastern US in 1974, the USFWS used the term eastern timber wolf, classified as *Canis lupus lycaon* (USFWS 1978, USFWS 1992). The listing by the ESA 1978 eliminated subspecies designation and listed all gray wolves (*Canis lupus)* in the lower 48 states as endangered, except those in Minnesota being listed as threatened. But recovery plans for gray wolves in US from Minnesota eastward continued to refer to this as "Recovery Plan for the Eastern Timber Wolf" (USFWS 1992). Since the 1974 listing, the perceptions of subspecies distribution have been drastically reduced for *Canis lupus lycaon* (Nowak 2002, 2003, 2009, Chambers et al. 2012, Thiel and Wydeven 2012), and considerable research suggests it is actually a distinct species *Canis lycaon* (Wilson et al. 2000, Wilson et al. 2003, Kyle et al. 2006, Rutledge et al. 2010a, Rutledge et al. 2010b, Wilson et al. 2012, Dinets 2015, Rutledge et al. 2015,Hohenlohe et al. 2017, Heppenheimer et al. 2018). The wolves living in the Great Lakes region have since been reclassified as *Canis lupus nubilus* (Nowak 1995, 2003) or a variety *Canis lupus* that has hybridized with *Canis lycaon*, and referred to as Great Lakes wolves (Wheeldon and White 2009, Fain et al. 2010, Wheeldon et al. 2010, Chambers et al. 2012). Thus the Recovery Plan for the Eastern Timber wolf evolved into a recovery plan for gray wolves in eastern US, but is not a recovery plan for the eastern wolf (*Canis lycaon*). Care needs to be made in this document to differentiate between gray wolves in the eastern US, and eastern wolves.

In the discussion of the eastern wolf (*Canis lycaon*) in third paragraph, p. 1, the review lists several ways that it has been described including, a separate species, a subspecies of gray wolf, or a hybrid between gray wolves and coyotes. The review failed to acknowledge that eastern wolves have also be described as the same species as red wolf (Wilson et al. 2000, Wilson et al. 2003, Kyle et al. 2006, Dinet 2015,Wilson et al. 2012) , or a hybrid between red wolves and gray wolves (Nowak 2003, Nowak 2009). Thus there is evidence that eastern wolves are the same species that USFWS continues to designate as highly endangered (National Academy of Science, Engineering, and Medicine 2019), and not an eastern version of the gray wolf that USFWS believes has recovered in the US.

The last paragraph at the bottom of page 2, has some inappropriate citations. The statement "These recolonizing wolves were more similar to eastern wolves (Mech et al. 1995; Wheeldon and White 2009)" is misleading. Mech et al. 1995 does not address wolf taxonomy. Wheeldon and White (2009) examine historical wolf samples from Minnesota and Wisconsin, and point out that these wolves were similar admixture of gray wolves/eastern wolves that currently exist in the area. The statement "more similar to eastern wolves" is confusing and misleading. Leonard and Wayne (2008) argue that the original wolves that occurred in the Great Lakes have not been restored, but both Mech (2009) and Wheeldon and White (2009) refute this argument. Mech (2010) also does point out that wolves recolonizing the western Great Lakes do have more pure gray wolf genetics compared to the more eastern/gray wolf admix population that originally occurred in the region. But for the most part there is strong evidence that the genetic mix of wolves re-establishing in the region is similar to the wolves that historically occupied the area.

I have some concerns about the summary paragraph on taxonomy on page 4. The following is not completely accurate, " In the eastern United States and area around the Great Lakes, wolves appear to have a complex evolutionary history in which hybridization with coyotes likely has played a significant role, and where there is uncertainty regarding the number of valid wolf species (reviewed by Waples et al.2019)." This is a mischaracterization of Waples et al. (2018), and mainly accepts concepts presented by von Holdt et al. 2016, but ignores the challenges presented by Hohenlohe et al. (2017). Ongoing hybridization with coyotes is an issues for eastern wolves and red wolves, but not Great Lakes wolves (Hohenlohe et al. 2017). Most scientists who accept the eastern wolf as a distinct species, do accept Great Lakes wolves as gray wolves or gray wolf that have hybridized with eastern wolves, and the Great Lakes wolves are not the same as eastern wolves (Rutledge et al. 2012, Rutledge et al. 2015, Hohenlohe et al. 2017, Heppenheimer et al. 2018).

In the last two sentences in that paragraph, the USFWS seems to try to provide rational for using the term "eastern wolves" instead of "eastern gray wolves" or "gray wolves in eastern US" . I believe in so doing, USFWS further confuses the issue, instead of providing clarity. This seems to disregard concepts such the precautionary principle. There is general agreement that Great Lakes wolves are a variety of gray wolves, although some researchers have confused/misrepresented Great Lakes wolves as same as eastern wolves (von Holdt et al 2016, but see Hohenlohe et al. 2017). But there is general scientific agreement that eastern wolves and red wolves deserve separate conservation consideration as unique ecotypes, ecological surrogates, distinct population segments, or unique species (Randi 2010, Wayne and Shaffer 2016, Heppenheimer et al. 2018, Waples et al 2018). Assessments done recently by the National Academies of Science, Engineering and Medicine (NASEM 2019), does designate Mexican gray wolves (*Canis lupus baileyi*) as a valid subspecies and red wolves (*Canis rufus*) as valid taxonomic species, using similar metrics that have been used to argue that eastern wolves (*Canis lycaon*) is a valid taxonomic species.

On pages 5, the second paragraph discusses divergence of gray wolves from coyotes cites only fossil record data from Nowak (2003) of 1.8 to 2.5 million years ago but does not include more recent genetic assessment by von Holdt et al. (2016). Von Holdt et al. (2016) suggest that divergence may have occurred as recently as 51,000 years ago, but re-examination of their data suggest that divergence probably occurred > 80,000 years ago (Hohenlohe et al. 2017). In the examination by NASEM (2019) of the Von Holt et al. (2016) data suggest 82,000-175,000 years divergence between red wolves and coyotes, and >160,000 years divergence between gray wolves and coyotes. Although there is some disagreements, the genetic assessments suggest much more recent divergence between gray wolves and coyotes than detected in the fossil record.

Also on the same paragraph Honeycutt (2010) is used as reference on dog domestication, and includes range from 13,000 to 135,000 years ago. More recent assessments suggest timing was 20,000 -40,000 years ago, with earliest estimate of wolf-dog divergences 36,900-41,500 years ago (Skoglund et al. 2015, Botigue et al. 2017).

The second last paragraph on page 5 on pack territories may want to include some information on territories in the Great Lakes region. In recent years, average territory size in Minnesota was about 139 to 192 km^2 (Erb et al. 2018), in Wisconsin averaged 136 km^2 (Wydeven et al. 2009b), and in Michigan about 287km^2 (Potvin et al. 2005)

On p. 6, the third paragraph, starts "Wolves of all ages disperse…" is misleading. Gray wolves rarely disperse before 10 months of age, and most commonly disperse between 1-2 years of age (Gese and Mech 1991, Treves et al. 2009, Jimenez et al. 2017). When dispersal does occur among pups it generally occurs in late winter as these wolves are approaching their first birthday. Dispersal age as young as 4.5 months have been detected in eastern wolves (Mills et al. 2008), but has not been found in gray wolves.

Fourth paragraph p. 6 discusses wolf population dynamics. The statement , "However, there is some evidence to suggest that wolves may be regulated by density dependence …", could include additional citation such as, Van Deelen (2009), Stenglein et al. (2015a), O'Neil et al. (2017), and Stenglein et al. (2018). The statement should perhaps be changed to "..there is 'considerable' evidence to suggest..".

The same paragraph includes the following statement, "Where harvest occurs…. mortality rates of 17% to 48% .." and lists Fuller et al. (2003) indicating +/- 8%. But a careful read of Fuller et al. (2003, p.184-185) would suggest that human caused mortality can be as high as 27 to 35% before a wolf population is likely to decline. The review may also want to cite Stenglein et al. (2015b) using modeling of the Wisconsin wolf population demonstrates that a 30% annual harvest would on the average reduce the wolf population by 65% over 20 years.

On p. 7, the second paragraph lists habitat suitability modeling for central and eastern USA. Important publications missing from the list include by Gehring and Potter (2005), Smith et al. (2016), and Stricker et al. (2019). The discussion should include that, as wolves have begun to saturate areas of suitable habitat, they have shown greater tolerance of higher road densities than during initial colonization, though road density, lack of agricultural land and forest cover continue to be important predictors of suitable wolf habitat (Mladenoff et al. 2009).

Page 7, last paragraph discussion of questioning the reported historical absence of gray wolves in California, should also cite Schmidt (1991).

The discussion on at the bottom of pages 7 and top of page 8, and Figure 1 discusses original distribution of gray wolves. The review uses older information by Nowak (1995) instead of using the more updated information from Nowak (2002, 2003, and 2009). Nowak (2002, 2003, and 2009) updated his distribution maps of gray wolves, eastern wolves, and red wolves after examination of additional specimens and considering additional data from other others. His map shows red wolves extending from Texas to Maine and to southern Maritime Provinces of Canada. Nowak (2002, 2003 and 2009) only shows gray wolves east of Mississippi around the Great Lakes and a narrow border along the Canadian border. All know historical samples of wolves east of the Mississippi River, and east and south of the Great Lakes have been demonstrated to be red or eastern wolves (Nowak 2002, Wilson et al. 2003, Rutledge et al. 2010b, Brzeski et al. 2016). Thus there are no scientific specimens to demonstrate any presence of gray wolves in this region in historical times. The 1995 map fails to capture this more updated information. The later maps represent the best available science for demonstrating the original distribution of gray wolves, eastern wolves and red wolves in North America.

It may be useful on Figure 1 to include marks on states in which dispersing gray wolves have appeared. It appears that west of the Mississippi River, the only states in historical gray wolf range that have not seen dispersers are Oklahoma and Texas (Arkansas and Louisiana were outside gray wolf range). Such a map would further demonstrate the level of recovery gray wolves have attained.

The first paragraph on page 10 discusses historical distribution of wolves in Minnesota. The review should perhaps include information that bounties ended in Minnesota in 1965, and wolf hunting and trapping ended in the Superior National Forest in 1970, where most wolves lived. The eliminations of the bounties and early levels of protection may have started the initial increase in wolf numbers and distribution, because wolf estimates in the early 1970s seemed to be higher than low levels achieved in 1950s and 1960s (Erb and Don Carlos 2009).

The second paragraph on page 10 discusses historical wolf populations in Wisconsin. Along with end of bounties, the review should include that in 1957 wolves were listed as a protected species, which was probably the first time any US state listed them as protected (Wydeven et al. 2009). Although the protection only arrived shortly before wolves were considered extinct in Wisconsin (Thiel 1993, Wydeven et al. 2009).

The third paragraph on page 10 discusses wolves in Michigan. It includes the statement "Wolves were state-listed in 1965." This is misleading. "State-listed" implies listing as endangered or threatened. In 1965 wolves in Michigan were listed as protected (Beyer et al. 2009), similar the designation in Wisconsin in 1957 (Wydeven et al. 2009). Wolves were added to the state list of endangered species in Michigan in 1976 (Beyer et al. 2009).

On page 10, the second last paragraph discusses historical gray wolf distribution in the northeast US. The opening statement, "Although historical abundance data are unavailable, gray wolves are known to have ranged in suitable habitat throughout the Northeast…" is misleading. All that is really known is that wolves roamed the region, but it is not known whether these were eastern wolves, red wolves, gray wolves, or hybrids of these. Later in the paragraph there is discussion that historical samples in the region were of eastern or red wolves, as I have stated above. There are no known historical samples of gray wolves from this region. Nowak (2002, 2003, 2009), shows the Northeast as range of the red wolf and eastern wolf. The possibility of gray wolf range is only shown along the extreme western and northern edge of the region. The exact boundaries are not known, and distribution range probably varied over time, but there is no solid historical evidence of gray wolves in Northeast USA.

The historical patterns discussed in the last paragraph on page 10 just refers to wolves, but more likely eastern or red wolves, and not likely gray wolves.

The last paragraph on page 11 ends with the statement, "Although it was suspected that wolves inhabited Wisconsin at this time, it was not until 1979 that wolf presence was confirmed in the state." This statement is misleading. Breeding wolves were detected in Wisconsin in 1978 (Thiel and Welch 1981). Wolf monitoring started in Wisconsin in 1979, because it was apparent that wolves had returned to the state (Wydeven et al. 2009). Retrospective assessments suggested wolves had returned to Wisconsin in about 1975-1976 (Wydeven et al. 2009).

The last paragraph on page 13 discussed minimum counts of wolves in Montana, and compares to the current system of Patch Occupancy Modeling (POM). It might be useful to compare POM to minimum counts so that readers can more easily determine how the wolf population is changing. Something like the following may be useful, "With changes in status of wolves, Montana began to use Patch Occupancy Modeling to estimate wolf numbers, along with conducting minimum counts. During the period 2007 through 2012 minimum counts fell just slightly below the lower end of the 95% confidence intervals for POM (MFWP 2018). After 2012 minimum counts represented only about ¾ of the lower end of the 95% confidence interval as the state transitioned from minimum count to POM as the main wolf population estimate. The 2017 minimum count is expected to be the last year this system will be used (MFWP 2018). In recent years the Montana wolf population estimate from POM seems to be hovering at about 900 wolves (MFWP 2018)."

On P. 14 updated counts should be done for Oregon, Washington and California for 2018.

On P. 16, the third paragraph on wolf population estimates for Minnesota can be updated to 2018. The most recent estimate in 2018 was a population of 2,655 wolves in 465 packs ((90% CI of 1972-3387) (Erb et al. 2018)

The third paragraph on p.17 discusses changes in wolf range in Minnesota. In 2018 total wolf range had grown to 111,862 km^2 (43,190 mi^2), or nearly 300% since the 1970s. Occupied range, the estimate of actual area occupied by wolf packs, generally is about 70 to 80% of total range, and has remained between about 68,000 – 74,000 km^2 (26,250 -28,570 mi^2) since 1997-1998 (Erb et al. 2018).

Fourth paragraph on page 17 discuss wolf extirpation and early surveys in Wisconsin. Along with Thiel (1978) and Thiel (1993), the paragraph should cite Thiel and Welch (1981). The following statement should be modified, "….no reproduction was documented in Wisconsin <u>until 1978 (Thiel and Welch, 1981). Most wolves reported were probably dispersing animals from Minnesota."</u>

In the first paragraph on p. 18, along with Wydeven et al. (2006), Wydeven et al. (2009) should be cited on survey methods for Wisconsin.

The third paragraph on p. 18 discusses growth of the Wisconsin wolf population. This should be referred to as "minimum counts", and not "population estimates". Wiedenhoeft et al 2018 should be cited for the 2018 population information. It would be useful in the same paragraph to point out that the minimum count of wolves in Wisconsin at the start of delisting in 2012 was 815 wolves, and after 3 years of public hunting and trapping seasons had been reduced to a minimum count of 746 in 2015, or a reduction of 8.5%. During that same time period verified wolf kills on cattle declined from 71 in 2011 to 30 in 2014, or 57% reduction, and numbers of farms with wolf depredations declined from 40 in 2011 to 22 in 2014, or 45% reduction (USDA-WS reports). These data demonstrate that active management with delisting could drastically reduce wolf depredations on livestock, without the need to drastically reduce the wolf population.

The fourth paragraph on p. 19 discusses wolf population estimates for Michigan. The following statement is confusing, "..were an estimated 687 wolves in the UP (see Table 1 above)." There is no Table 1 above, but this information is presented in Appendix 1.

In the same paragraph, the following is misleading , "…but overall the population grew by 19% over the 8 years." That is only true if the 2010 estimate is included. The population appeared to peak at 687 in 2011, and has slightly declined or remained relatively stable. With a minimum estimate of 662 in 139 packs in 2018 the wolf population in Michigan appears to have stabilized since 2011 (Michigan DNR 2018).

It should be made clear in this section that Michigan DNR assessments of wolf numbers used a minimum counting system of the whole wolf population from 1989 through 2007, but since 2008 has estimated the minimum number of wolves in the state by a partial sampling system.

The return of wolves to Isle Royale is ongoing and readers might be directed to the NPS web site. Four wolves from mainland Minnesota were released on the island in the fall 2018, and in March 2019, 11 Canadian wolves from mainland Ontario (3) and Michipicoten Island (8) were released on the island. During fall one wolf died and in winter one wolf dispersed back to mainland. By mid-April 2019, 15 wolves were on the island including the two wolves from the original population. https://www.nps.gov/isro/learn/news/fall-implementation-of-wolf-translocation-project-ends-due-to-weather.htm

On p. 20 the third and fourth paragraphs discuss possible wolf occurrence in Northeast USA. As discussed earlier, there is some question whether this was originally part of historical gray wolf range. Several studies do suggest that gray wolves and or eastern wolves could potentially disperse into this area (Harrison and Chapin 1998, Wydeven et al. 1998, and Carroll 2003). Thiel and Wydeven (2012) list eight wolves detected in New England, New York and Quebec south of the St Lawrence River. Kays and Feranec (2011) determined that at least three of these wolves were not of wild origin. The specimens detected included three listed as gray wolf killed near Day, New York, December 2001, North Troy, VT, October 2006, and Shelburne, MA, October 2007. A wolf killed near Glover VT, November 1998 was listed as eastern wolf, and Ste. Marguerite-de-Lingwick, QE in January 2002 was listed as eastern wolves/coyote hybrid (Villemure and Jolicouer 2004). A recent wolf found in New Brunswick carried Mitochondrial DNA sequence of gray wolf, but Y-Chromosome of eastern wolf, thus an eastern/gray wolf hybrid (Mc Alpine et al. 2015). In general dispersing wolves moving south of the St. Lawrence River are relatively rare, and appears more likely to occur for gray wolves. Benson et al. (2014) determined that dispersing eastern wolves from protected areas suffer high rates of mortality especially by public harvest, and are more sensitive to areas with high road densities than gray wolves. Thus it will be difficult for eastern wolves to disperse to northeastern US.

The first three paragraphs on p. 21 discuss Mexican wolf status. This should be updated. At the end of 2018 the minimum count in US was 131, and increase of 12% from 2017, including 64 in AZ and 67 in NM (USFWS 2019). A total of 32 packs were detected and at least 16 produced pups. About 30 wolves were also detected in the wild in Mexico (USFWS 2019).

The list of lone wolf records on listed on p. 21 should include IA, NY, & VT. Since 2001, there have been 30 reports of gray wolves in Missouri and six have been killed in the state (Conlee and Johnson 2018, p. 40). Between 2012 and 2017, four wolves have been killed in Iowa, one was observed on a trail camera, and 17 unconfirmed reports of wolf observation were received (Iowa DNR 2018, p. 216-219). In the Northeast lone gray wolves were also found dead in New York and Vermont (Kays and Feranec (2011) verified that wild gray wolves had been killed in New York (1) and Vermont (2) in 1990s and early 2000s.

It is probably important to mention that within gray wolf range west of the Mississippi River, gray wolves have only been undetected in two states, Oklahoma and Texas (gray wolves were not know to occur in Arkansas and Louisiana). Smith et al. (2015) suggested that western portions of Great Plains states, might represent suitable wolf habitat, but despite high rates of dispersers occurring in the Dakotas, no breeding packs have developed in the area. Smith et al. (2015) relied mainly on human population density and road densities, and did not include forest cover as a factor in assessing potential wolf habitat. Forest cover has been found to be an important factor in models of suitable habitat in both the Great Lakes region (Mladenoff et al. 2009) and Rocky Mountains (Oakleaf et al. 2006).

On page 22 the second paragraph discusses current wolf numbers in the Great Lakes area. The current number (2018) is 4200 (specifically 4222), not 4400 as listed. The last sentence includes the following, "The wolves in these three states occupy areas of high quality habitat with abundant prey." Some citations at the end such as DelGuidice et al. (2009), and Mladenoff et al. (2009) would be useful.

On page 23, the third paragraph discusses the two US meta-populations. The following may be useful addition to that paragraph, "Dispersing wolves have been detected in all the states in historical gray wolf range west of the Mississippi River except Oklahoma and Texas. The high rates of wolf dispersal across western states suggests that gray wolves will likely eventually find most large patches of suitable habitat as long as healthy core wolf populations are maintained on the landscape."

Pages 24 illustrates Appendix 1, wolf populations in MN, WI and MI. It would be useful to include 1979-1980 under year to capture the first year of agency wolf counts in Wisconsin (25 in 1979-1980). Under column for Wisconsin, 1976 should be 2+ and 1978-1979 should be 7+. Under the column for Minnesota, 2017-2018 should be 2655.

Additional Literature Citations:

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Review of Federal Register (Mar. 15, 2019), Propose Rule, Removing the Gray Wolf (*Canis lupus*) from the list of Endangered and Threatened Wildlife 84 (51):9648-9687.

1. ***Does the proposed rule provide an adequate review and analysis of the factors relating to the persistence of gray wolf populations currently listed under the ESA in the contiguous 48-States?***

In general the USFWS has done an extensive review and analysis of the factors relating to the persistence of gray wolves (*Canis lupus*) across the contiguous 48 states. The gray wolf populations have obvious grown and expanded extensively across the Great Lakes States of Minnesota, Michigan and Wisconsin, as well across the northern Rocky Mountain States of Idaho, Montana, and Wyoming, and into the Pacific Coastal States of Oregon, Washington, and recently into California. Dispersing wolves from these two core populations have been detected in Nevada, Utah, Colorado, North Dakota, South Dakota, Nebraska, Kansas, Missouri, Iowa, Illinois and Indiana. In Southwestern US, Mexican gray wolf (*Canis lupus baileyi*) populations are growing and expanding in Arizona and New Mexico. Thus west of the Mississippi River in recent times, gray wolves have been detected in all states in the original gray wolf range except Oklahoma and Texas. East of the Mississippi and south and east of the Great Lakes, only limited dispersers from Canada have been detected in Vermont, New York, and possibly Massachusetts, but there remains considerable debate whether gray wolves existed in the region in historical times, and may have been just occupied by red wolves and or eastern wolves.

a. Discussions on impact of human caused mortality

In the discussion on "Human-Caused Mortality" (p. 9659-9662) some important recent literature on wolf population dynamics and mortality factors are missing. I will discuss where additional literature should be cited below.

On p. 9659, column 3, the third paragraph discusses history of bounty systems for each state. The more appropriate citations are Beyer et al. (2009) for Michigan, Wydeven et al. (2009) for Wisconsin, and Erb and Don Carlos (2009) for Minnesota.

The statement at the bottom of column 3 on p. 9659 discussing regulation of human-caused wolf mortality, can be supported by citing the following: Smith et al. (2010), O'Neil et al. (2017), Stenglein et al. (2018).

Several important paper can be cited on page 9660 in reference to "number of illegal killing" paragraph 3 in column 1. In the Northern Rocky Mountains, mean annual mortality for wolves mostly one year and older, averaged about 25% annually, the majority of mortality was due to humans, and about 30% of the mortality or 8% of the population was due to illegal killing (Smith et al. 2010, p. 625). But 12 % of the mortality in Northern Rocky Mountains was due to unknown causes, thus actually illegal kill rate was probably higher (Treves et al 2017b). In large protected areas such as Yellowstone National Park, human-caused mortality factors were minor, and intraspecific strife was the most important form of mortality (Cubaynes et al. 2014, p.1351). In Wisconsin annually mortality averages of24% of wolves one-year or older, and about 9% of wolves were killed illegally (Stenglein et al. 2018, p. 104). Additionally, Stenglein et al. (2015, p.1183) suggested that as many as 400 illegally killed wolves were undetected in

2003-2013. Using some different methods, Treves et al. (2017b, p. 27), also determined that 39-45% of wolf mortality was due to illegal killing in Wisconsin. Olson et al. (2015, p.354-355) in Wisconsin determined that during the period 2003-2012, illegal kill rates were much higher when federally listed, than when the state had some management authority. In Michigan, annual survival averaged 0.75 (i.e. 25 % annual mortality, O'Neil et al. 2017, p.9523). Thus illegal kill continues to be an important factor in recovering wolf populations, with human mortality being the most important form of mortality (except in large protected areas), yet wolves across Northern Rocky Mountains and Great Lakes region have grown to healthy population levels and continue to expand where suitable habitat exists. Illegal kill and human mortality have been adequately reduced to allow recovery to healthy population levels in both locations.

Clarification is needed on p.9660, third column, second paragraph, on authority to use 4 (d) control actions in Wisconsin and Michigan. Lethal control action was authorized under down-listing authority April 13, 2003-January 31, 2005, by special permits (April 1, 2005-September 13, 2005; April 24, 2006-August 10, 2006), and when delisted (March 12, 2017-September 29, 2008, May 4, 2009-July 1, 2009, and January 27, 2012- December 19, 2014).

On page 9660, 3rd column at the bottom of the page discusses "…initial objectives of States may be to lower wolf populations…". This is a misleading statement. State objective may be to "reduce or stabilize" wolf numbers. In Great Lakes, only Wisconsin expressed interest in lowering wolf number to a management goal.

On page 9661, first column, third paragraph, discusses compensatory mortality. This paragraph needs updating. The presence of compensatory mortality in wolf populations varies with habitat quality and other circumstances. While harvest mortality often is additive (Horne et al. 2019, p. 40-41), compensatory mortality has been detected in some wolf populations (Stenglein et al. 2018). In Wisconsin, Stenglein et al. (2018, p.106-108) did not detect compensatory mortality until the wolf population started approaching carrying capacity since 2004. Cubaynes et al. (2014) indicated that in Yellowstone National Park, average annual survival was 0.80 in an area where intraspecific killing among wolves was the most important mortality, only slightly higher than mean survival across the Northern Rocky Mountains (0.75) and Great Lakes region (0.76) where human-caused mortality dominated (Smith et al. 2010, O'Neil et al. 2017, Stenglein et al.2018). In Denali National Park compensatory mechanisms buffered against impacts on the wolf population from packs dissolution (Borg et al. 2014, p. 185). Similarity of survival rates among naturally controlled and human controlled populations does suggest a moderate level of compensatory mortality.

On pages 9661, first column, the 4th paragraph discusses impact of state management on wolf populations. It may be useful to updates with the following; in Idaho the wolf population peaked in 2009 at 856, and with state management including public harvests since 2009, the population declined slightly and has stabilized between 684 to786 wolves during 2010-2015 (Hayden 2017, p. 14). In Montana the minimum count peaked in 2011, and has remained at minimum counts of 500 to 600 wolves during 2012-2017 (Montana Fish Wildlife and Parks, 2018, p. 10). Population estimates by patch occupancy modeling show actual numbers in Montana have stabilized near 900 wolves (MFWP 2018, p.10).

In the paragraph there is a statement about "…instituting fair-chase wolf hunting seasons with the objective of slowing or reversing population growth…" A better description would be "….instituting regulated hunting with the objective of stabilizing or reducing population growth."

On page 9661, the bottom paragraph discusses effects of human-caused mortality on wolf social structure. Some updated references could be added. Ausband et al. (2015, p. 418-420) reports reduced pup survival and presence in wolf packs with harvest, and suggest the proportion of pup mortality was greater than actual harvest impact, perhaps due to smaller pack size. Ausband et al. (2017, 1097-1098) reported reduce pack size, increase polygamy with breeding female turnover, and reduced recruitment of female pups into litters after breeding male turnover.

On the bottom of page 9661, column 3, the last paragraph discusses "Role of Public Attitudes'. A few recent important citations that can be added to those near the bottom, include, Shelley et al. (2011), Browne-Nunez et al. 2015, and Hogberg et al. (2015). Shelly et al. 2011 demonstrated that members of the Ojibwe Tribe generally had more positive attitudes and were more protective of wolves, than non-tribal residents in wolf range. Browne-Nunez et al. (2015) examined changing attitudes before and after wolf hunting occurred, and although found little change in attitudes toward illegal killing of wolves one year after the first harvest, did find more support for wildlife agencies that managed wolves. Hogberg et al. (2015) did determine that 36% of people interviewed in wolf range had better attitudes toward wolves after the first wolf hunt. Browne-Nunez et al. (2015), had used mixed methods to survey people in wolf range, and found through focus groups, that attitude surveys did not often capture the complexity of attitudes, and issues of fear, trust and empowerment were often more important issues. Although major shifts did not occur in attitudes one year after wolf hunts, both surveys found strong support for wolf hunting for residents in wolf range, and such hunts could improve issues of trust, fear, and empowerment over time.

On page 9662, the first column and first paragraph discusses difficulty of determining behaviors toward wolves from attitude surveys. Olson et al. (2014, should be 2015) is cited but not adequately discussed. Olson et al. (2015) demonstrated that rates of illegal killing of wolves declined in Wisconsin during periods when the state had more flexible management authority, and increased when federally listed as endangered during 2003-2011. Although Chapron and Treves (2016) conducted analysis that challenged the findings of reduced illegal kill rates with state management of wolves, others have refuted their analysis (Olson et al. 2017, Pepin et al. 2017, Stein 2017).

On p. 9662, second column, first paragraph discuss potential state impacts on wolf numbers. The following statement should be modified, "…these three States, we can expect to see some reduction in wolf populations in the Great Lakes areas if they are delisted as States implement lethal depredation control and begin to institute wolf hunting seasons with object of slowing or reversing population growth". "we can expect to" should be changed to "we may". Instead of "slowing", a better term would be "stabilizing". In the past only Wisconsin indicated attempts to manage toward a numerical goal, which could change with a new wolf plan for the state. Minnesota and Michigan have not indicated intent to reduce wolf numbers, and it appears wolf population have mostly stabilized in both states, and only Wisconsin continued to have a growing wolf population.

b. Discussions on threats to and availability of wolf habitat

On p. 9662, second column, and last paragraph, discusses that “large portions of gray wolf historical range is no longer suitable habitat”. Smith et al. (2016, p. 559) should probably be included in the list.

On p. 9662, third column, and second paragraph on discussion of wolf habitat in Great Lakes region should also include Smith et al. (2016, p. 559-562).

On p.9662, third columns, last paragraph discusses road density as a factor in habitat suitability for wolves. The discussion should probably include that during early colonization wolves selected some of the lowest density on the landscape, but as the wolf population grew and expanded, wolves were willing to accept areas with higher road densities Mladenoff et al. (2009, p. 129-136). While road density continues to be an important predictor, presence of forest cover and lack of agricultural lands in recent times were also important predictors for potential wolf habitat (Mladenoff et al. 2009). Smith et al. (2016, p.560) relied mainly on road density and human population density to assess potential wolf habitat across the central US, and probably show exaggerated potential for wolf colonization, especially in the western Great Plains that lack forest cover.

On P. 9663, first column, and end of second paragraph discusses different mortality rates across different classes of wolf habitat in Wisconsin. Stenglein (2018) updates this analysis. Stenglein et al. (2018, p.106) demonstrated that in the core of wolf range and high quality habitat, survival rate ranged from 0.78-0.82, similar to Yellowstone National Park (Cubaynes et al. 2014). At the edge of wolf range and into more marginal habitat survival rates declined to 0.49-0.61 (Stenglein et al. 2018). Also there was a shift from prevalence of natural mortality in core habitat, to prominence of human-caused mortality in more marginal habitat (Stenglein et al. 2018, p.107).

On p. 9663, second column, the fifth paragraph discusses wolf habitat modeling in the northern Lower Peninsula of Michigan. Stricker et al. (2019) recently assessed potential den habitat and dispersal corridors in the region. The study determined that 736 mi^2 of high quality den habitat existed in the Lower Peninsula, but the landscape has low permeability for wolf movement (Stricker et al. 2019, p.87-88).

On p. 9664, first column, second paragraph discusses prey available to wolves in Great Lakes. An important publication that should be cited here is DelGiudice, et al. (2009, p. 158-163).

c. Discussion of disease and parasites

On p.9666, first column, second paragraph discusses Lyme disease in wolves. Jara et al. (2016) present updated information on vector-borne disease in wolves. Jara et al. (2016) demonstrate that a large percentage of wolves in Wisconsin test positive for Lyme Disease, and increased over time during wolf recovery in the state, but showed no indication of having population effect on wolves in the state (Jara et al. 2016, pp. 5-9).

On p. 9666, second column, third paragraph summarizes other disease and parasites. Anaplasmosis, and Ehrlichiosis should be added to the list, and Jara et al. (2016, pp. 1-13) should be added to literature at the end of statement.

With a warming climate new diseases may occur in wolf populations. Jara et al. (2016) show that Lyme Disease which may be enhance by mild winters, has grown some in wolf population in Wisconsin, but not showing any impacts on the population. Diseases such as Heartworm spread by mosquitoes, remains at low levels in wolves and has not shown any upward trends despite warming climate (Jara et al. 2016.

Chronic Wasting Disease (CWD) could become more of a future concern if it causes major decline in deer and elk, the most important prey for wolves in the Contiguous States. Thus far CWD has not spread into extensive areas with wolf packs, and wolves may be a factor to reduce spread or prevent establishment of the disease (Wild et al. 2011). In Wisconsin the concentration of Chronic Wasting Disease is in southwest part of the state in mixed agricultural /woodlot landscapes where wolf packs do not occur (Wisconsin DNR, https://dnr.wi.gov/topic/wildlifehabitat/regulations.html).

d. Discussion of state wolf management plans, tribal conservation, and management on federal lands

On P. 9670, first column, fourth paragraph discusses the management goal in the Wisconsin wolf plan. The paragraph begins, "The Wisconsin Plan contains a minimum goal of 350 wolves outside Native American reservations," is not correct. The '350' is a management goal, and '250' the state delisting goal would represent the state minimum goal.

On p. 9672, second column, first paragraph discusses impact of depredation controls on the viability of the Wisconsin wolf population. Several studies can be cited to provide additional support. Stenglein et al. (2015, pp. 17-21) demonstrates that regular removal of 10% of the wolf population for depredation controls has little impact on growth of the wolf population. Olson et al. (2015b, pp 680-681) shows that only a small percentage of packs cause depredation on livestock, and several risk maps show that the potential locations with high risk of wolf depredations on livestock represent a small portion of wolf range in Wisconsin (Treves et al. 2011, Treves and Rabenhorst 2017).

On p.9673, second column, first paragraph discusses Wisconsin Act 169 creating a public wolf hunting season in Wisconsin. With the establishment of the wolf hunting season in 2012, the WDNR modified the 4 zones from the 1999 wolf plan into 6 harvest zones. Much of the original zone 1, northern forest wolf range from the 1999 plan was modified into 4 harvest zones, with zones 1 and 2 representing core wolf areas and zone 3 and 4 representing transitional wolf habitat (WDNR 2014, p.8). Most of Zone 2, central forest core wolf range, from the 1999 plan became harvest zone 5 (WDNR 2014, p.8). The remainder of the state mostly zones 1 and 2 from the 1999 plan, are marginal and unsuitable wolf habitat became wolf harvest zone 6 (WDNR 2014). With change to these harvest zones, WDNR management and survey focus currently rely on the 6 harvest zones.

On p. 9673, second column, second paragraph, discusses Wisconsin wolf harvest in relationship to Tribal lands. The paragraph does not properly represent the issue. The following statement after listing reservations, includes incorrect information, "…and separate quotas were set for these ceded territories." The WDNR did not set any quotas for Indian reservations. Ceded territories are not reservation lands, but are lands outside reservations where Ojibway Tribes/Bands continue to hold fishing, hunting and gathering rights. A large portion of the zones open to wolf hunting in the state included ceded territories. Within ceded territories the Ojibway bands can request up to half of any allowable harvest of wildlife for their members. The ceded territories portions of wolf harvest zones included an allowable harvest of 170 wolves, and one half or 85 wolves was offered to the tribes for harvest in 2012. The Ojibway Tribes choice not to take part in the wolf harvest, and all State Tribes closed tribal lands to wolf hunting. Because the Ojibway Tribes choose not to exercise their wolf hunting authority, the portions of the allowable harvest offered tribes declined in subsequent years to 24 in 2013, and 6 in 2014 (WDNR 2013, WDNR 2014b, MacFarland and Wiedenhoeft 2015).

It would be useful in the same paragraph to point out that the minimum count of wolves in Wisconsin at the start of delisting in 2012 was 815 wolves, and after 3 years of public hunting and trapping seasons had been reduced to a minimum count of 746 in 2015, or a reduction of only 8.5%. During that same time period verified wolf kills on cattle and numbers of farms with depredations declined drastically (Wiedenhoeft et al. 2015), suggesting that active management with public harvests and targeted lethal depredation controls could reduce conflicts without causing major declines in wolf numbers in the state.

Last paragraph on p. 9674 contrasting wolf depredations on dogs being greater in Wisconsin than Michigan, may want to cite Ruid et al. (2009, p. 264) and Bump et al. (2013).

On p. 9678, first column, third paragraph discusses a grant on Ho-Chunk land. The Ho-Chunk do not possess a reservation, but own scattered tribal lands.

On p. 9678, last column, third paragraph discusses Red Cliff Band. The discussion should include that the Red Cliff do have a wolf protection plan for their reservation (Red Cliff Band of Lake Superior Chippewa 2015). This section could also discuss the Bad River Band of Lake Superior Chippewa, who established a wolf management plan for the reservation in 2013 (Bad River Band of Chippewa Indians Natural Resource Department , 2013). The Bad River Band has been involved in wolf monitoring on the reservation since 1997. In period of 2010-2018, from 5 to 17 wolves were counted on the reservation in 2 or 3 packs (Bad River Band Natural Resource Department).

On p. 9679, first column, first paragraph discusses wolves on the Menominee Reservation. This section can include the following: “In 2010-2018 the reservation generally supported 7 to 16 wolves in 3 or 4 packs (Menominee Tribal Conservation Department).

There is no discussion on any tribes in the West Coast region and their relationship to wolves and perspectives on wolf conservation. This seems like an oversight.

On P. 9680, column three, first paragraph wolves on Isle Royale are discussed. This section should be updated. Four wolves were released on the island in fall from Minnesota, and 9 were relocated from Ontario in late winter. One of the Minnesota wolves died later in fall, and during winter one returned to the mainland. As of mid-April 2019, 15 wolves, including two of the original occurred on Isle Royale National Park (https://www.nps.gov/isro/learn/news/presskit.htm).

On p. 9680,column three, the third paragraph discusses the Apostle Island National Lakeshore. An additional citation would be Allen et al. (2018) of a gray wolf being detected on Stockton Island.

2. Has the Service adequately considered the impact of range reduction (lost historical range) on the long-term viability of the gray wolf in its remaining range in the Lower-48 States?

The USFWS has done an extensive effort identifying and characterizing potential wolf habitat in the Great Lakes region and West Coast region. In these areas habitat is fairly secure, and not likely to drastically change in the foreseeable future, and have been demonstrated to support viable wolf populations.

Great Lakes wolves seemed to have occupied much of the area of suitable wolf habitat in the region (Mladenoff et al. 2009, Smith et al. 2016, Erb et al. 2018, Michigan DNR 2018, Wiedenhoeft et al. 2018). Wolf populations seemed to have mostly stabilized in Michigan and Minnesota, and may be approaching stable conditions in Wisconsin. Although this Great Lakes core population continues to send dispersers into surrounding states of North Dakota, South Dakota, Nebraska, Iowa, Illinois, Indiana, and Missouri, and wolves from this core will continue to disperse into these areas in the foreseeable future, but there seems very limited opportunities for these dispersers to find suitable habitat. Smith et al. (2016) do show some suitable wolf habitat in southern Missouri, as well as portions of Arkansas, and Louisiana, but that have generally considered outside historical range of gray wolf, and within historical range of red wolves (Nowak 2003, 2009). Smith (et al. 2016) also shows the presence of potential suitable wolf habitat in western portions of Plains State, but they mostly relied on road density and human population density for predicting potential wolf habitat. Forest cover seems to be a fairly critical component to wolf habitat in most of the Lower 48 States (Carroll et al. 2006,Oakleaf et al. 2006, Mladenoff et al. 2009), and when incorporated into habitat for large carnivores (Smith et al. 2016), little suitable habitat for gray wolves exists in Central US outside of areas already occupied. In Michigan, there is some potential for wolves to spread into the Lower Peninsula, but the ability of wolves to disperse to the region is limited and permeability between habitat patches is low (Stricker et al. 2019).

Thus there is little potential for additional wolf populations to establish in historical range in Central US much beyond the current wolf population in the Western Great Lakes. The Great Lakes wolf population is genetically diverse and highly connected (Treves et al. 2009, Fain et al. 2010,Wheeldon et al. 2010), it performs ecological functions (Bouchard et al. 2013, Callan at al. 2013, Flagel et al. 2016), and shows signs of high levels of viability (Stenglein et al. 2015, O'Neil et al. 2017, Stenglein et al. 2018). As a delisted wolf population, there is no reason to assume genetic, interconnectedness, ecological functioning or viability will be altered. This population is not dependent on any wolves in former historical range and delisting will not alter the continuous flow of wolves into lost historical range, but these wolves probably represent a doomed portion of the population because of lack of suitable habitat unless dispersers return to the core population. At this point lost historical range has little impact on viability of wolves in Western Great Lakes region.

In the northwestern US, wolves continue to spread and grow from the core wolf population in the Northern Rocky Mountains. Despite delisting, the Northern Rocky Mountains continue to serve as a core population sending dispersers into adjacent states, and contributing to growth of wolf populations in Oregon, Washington and California. Although delisting has resulted in some decline and stabilizing of wolf populations in the Northern Rocky Mountain region (Hayden 2017, USFWS 2017, Montana Fish, Wildlife and Parks. 2018), the original introduced wolf population continues to serve as a source for wolves spreading throughout the region. This population will likely continue to spread and expand as long as large blocks of suitable habitat exists within range of dispersing wolves. Wolves in West Coast are not critical to the viability of the delisted Northern Rocky Mountain wolves and have no impact on viability of wolves in Western Great Lakes region. While the West Coast wolves are not needed for the viability of the core wolf population in the Northern Rockies, they do represent a healthy expansion of the same meta-population that will likely continue to expand under state protection until much of the suitable habitat in the region is occupied.

In the Northeast US, wolves continue to be missing, but it remains unclear if the original wolves in the region were gray wolves or eastern/red wolves. While the Northeast does have considerable area of potential wolf habitat, these areas are not easily connected to core wolf habitat in the Great Lakes region, and any wolves dispersing into the region are likely to be from Ontario or Quebec in Canada (Wydeven et al. 1998). Thus because of lack of any connection to other gray wolf populations in the Lower 48 State, and uncertainty of the area being historical gray wolf range, any wolves occurring in the Northeastern U.S. would not impact viability of gray wolves in core areas of the Midwest or Northwest.

3. ***Is it reasonable for the Service to conclude that the approaches of Michigan, Minnesota, and Wisconsin to wolf management will maintain viable wolf populations in the Western Great Lakes region?***

The federal register provides detailed discussion on state management in the region. All state plans provide extensive conservation consideration in all aspect to wolf management. Michigan has been the most aggressive at updating and keeping plans current. Wisconsin is mostly operating from a plan developed in 1999, with some minor updates in 2007. Minnesota developed in plan in 2000. Lack of new plans for those two states are probably due to legalistic and political uncertainty of down-listing/delisting /relisting that have occurred between 2003 and 2014 (Olson et al. 2015). After federal delisting, all states do continue to list gray wolves throughout each state as highly protected wild animals, allowing the killing of wolves only by limited permits during harvest or under specific circumstance of depredation or risk of depredations. During the most recent delisting, 2012-2014, there was no major change in wolf abundance in the region, going from 815 to 746 in Wisconsin, 2,211 to 2,221 in Minnesota, and 687 to 636 in Michigan. All states do intense levels of population monitoring, and use the information to inform population management. In Minnesota, when the population estimation from 4 years earlier, showed a decline after harvest, the MN DNR reduced harvest in 2013, and showed small increase the next year. Similar in Wisconsin, when the population showed an 18% decline after the 2013 harvest, quotas were reduced and the next year the population increased by 12%. Michigan only allowed one wolf hunt in 2013, with attempts to reduce wolf-human conflicts in specific areas. Only in Wisconsin was the public harvest used to reduce wolf numbers. Even with a fairly aggressive wolf harvest imposed by the state legislature (Olson et al. 2015), wildlife managers avoided over harvesting the population, and after three years of public harvest the wolf population had only declined by 8%. The combinations of thorough conservation plans, careful population monitoring, and long term commitment to wolf conservation, provides assurance that states will maintain healthy viable wolf populations.

4. ***Oversights and omissions of data or information in the delisting rule for gray wolves.***

The analysis does leave out some important reports and publication, in part due to publications that have occurred since the main analysis was conducted by the USFWS. Also it appears that a bulk of the assessments were made a while back, and not all aspects have been adequately updated. I have cited missing publications in discussions above, and have listed additional publications below.

5. ***Are there demonstrable errors of fact or interpretation? Has the USFWS delisting rule provided reasonable and scientifically sound interpretations and syntheses from the scientific information presented in the draft biological report and proposed rule, and are there other interpretations that could be made?***

Probably the main area of scientific disagreement in the Federal Register and Biological report are discussions on taxonomy and original gray wolf range. The decision, "we consider eastern wolves to be a member of the species *C. lupus* because there is no clear support for a recognizable and independent evolved eastern wolf." seems rather arbitrary. It ignores the USFWS own efforts to continue to list and recover the red wolf, which is a recognized and independently evolved eastern wolf (Nowak 2002, Kyle 2006, National Academy of Science, Engineering and Medicine 2019). It also ignores genetic and ecological assessments of eastern wolves as distinct species (Rutledge et al. 2010, Rutledge et al. 2010b, Benson et al. 2012, Chambers et al. 2012, Rutledge et al. 2012, Wilson et al. 2012, Benson and Patterson 2013, Benson et al, 2014, Dinets 2015, Rutledge et al. 2015,Hohenlohe et al. 2017, Heppenheimer et al. 2018). While challenges to eastern wolves as a distinct species remain (National Center for Ecological Analysis and Synthesis 2014, von Holdt et al. 2016, Sinding et al. 2018), there is considerable evidence of a distinct eastern wolf that differed from the gray wolf, had originally existed in the eastern US. The USFWS also choose to use older versions of Nowak's distribution of gray and red wolves across North America (Nowak 1995), instead of more updated versions (Nowak 2002, Nowak 2003, Nowak 2009). The more recent versions were updated based on examination of additional specimens, and gathering additional information. Currently there is no solid evidence that gray wolves historically lived in eastern US outside the Great Lakes region, though strong evidence that the area was occupied by wolves. The area is not critical to recovery of gray wolves in eastern US, because recovered populations exist in the Great Lakes, and it remains uncertain whether gray wolves would have extended into eastern states. Because the area was within the historical range of eastern/red wolves, some level of federal protection may be needed for "wolves" in the region to protect any dispersers that may travel through the region, or allow for future reintroductions. The red wolf remains highly endangered with wild populations only in small portions of northeastern North Carolina (NASEM 2019), and eastern wolves are threatened in Canada and only found in and around Algonquin Provincial Park, and nearby areas in Ontario and Quebec (Benson et al. 2014).

The USFWS further confuses the issue by using the term "eastern wolves" for gray wolves in the eastern part of their range. The term eastern wolf should only be used in reference to the genetically distinct smaller wolves living in and around Algonquin Provincial Park, and should not be applied to Great Lakes wolves that broadly accepted as gray wolves by most mammalogists and geneticists.

Minor errors of fact and interpretation are enclosed above under question one that extensively reviews the proposed delisting rule, and additional literature cited are enclosed below.

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Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Clarification Questions and Responses

May 24, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 3 – Adrian P. Wydeven

GRAY WOLF 2019 PROPOSED RULE: PEER REVIEW

Clarification Questions

Peer Reviewer #3

No questions. Please see updated review above.

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

May 24, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 4 – Dr. Adrian Treves

Peer review by A. Treves "of Removing the Gray Wolf (*Canis lupus*) From the List of Endangered and Threatened Wildlife" Federal Register **84**(51): 9648-9687 (2018). Final version 24 May 2019

I appreciate the opportunity given to me to serve as a peer reviewer of the science in the proposed rule and draft biological report. I want to recognize and applaud the Department of Interior and USFWS for enhancing peer review policies and respecting the public interest in its role as trustee of endangered and threatened species of the USA. I also am grateful for the opportunity to present new or overlooked information. This cover page summarizes my responses to the scope of work as a peer reviewer. Throughout, text sent to me by the U.S. Fish & Wildlife Service (USFWS or Service) or its contractor is italicized and my contributions are not italicized.

Response to questions posed to me

Questions posed for me about the Draft Biological Report and Proposed Rule

1*. Does the draft report provide an adequate and concise overview of gray wolf (Canis lupus) taxonomy, biology, and ecology as well as the changes in the biological status (range, distribution, abundance) of the gray wolf in the contiguous 48 United States over the last several decades?*

No, I find the draft biological report adequate on the taxonomy but not on the biology, ecology, or biological status.

2. Please identify any oversights or omissions of data or information, and their relevance to the report. Are there other sources of information or studies that were not included that are relevant to the biological report? What are they and how are they relevant?

In the 48-page review that follows, I detail the missing scientific information on all of the following topics: biology, ecology, or biological status. In summary, I am concerned by missing information on human-caused mortality, human attitudes leading people to kill wolves, and dispersal.

3. Does the proposed rule provide an adequate review and analysis of the factors relating to the persistence of the gray wolf population currently listed under the ESA in the contiguous 48-States (human-caused mortality, habitat and prey availability, disease and predation, and effects of climate change)?

No, the proposed rule does not address human-caused mortality or habitat suitability adequately.

4. Have we (the Service) adequately considered the impacts of range reduction (i.e., lost historical range) on the long-term viability of the gray wolf in its remaining range in the lower-48 states (outside of the northern Rocky Mountains) and, if not, what information is missing and how is it relevant?

Almost. I would have liked more information on dispersal and its related legal definition of discreteness and the causes and consequences of recolonization apparently having stopped.

5. Is it reasonable for the Service to conclude that the approach of Michigan, Wisconsin, and Minnesota to wolf management, as described in their Plans and the proposed rule and in the *context of wolf management in the Western Great Lakes area, are likely to maintain a viable wolf population in the Western Great Lakes area into the future?*

No. In the 48-page review that follows, I detail the many sources of evidence that are missing to draw such conclusions and the many contrary findings that seem to have been overlooked, which undermine the conclusions.

6. Please identify any oversights or omissions of data or information, and their relevance to the assessment. Are there other sources of information or studies that were not included that are relevant to the proposed rule and, if so, what are they and how are they relevant?

In the 48-page review that follows, I detail the missing scientific information on biology, ecology, or biological status. In summary, I am concerned by missing information on all of the following topics: human-caused mortality, human attitudes leading people to kill wolves, the effects of legal killing on illegal killing, and the uncertainties surrounding WGL wolf population census and monitoring.

7. Are there demonstrable errors of fact or interpretation? Have the authors of the proposed delisting rule provided reasonable and scientifically sound interpretations and syntheses from the scientific information presented in the draft biological report and the proposed rule? Are there instances in the proposed rule where a different but equally reasonable and sound interpretation might be reached that differs from that provided by the Service? If any instances are found where this is the case, please provide the specifics regarding those particular concerns.

There are demonstrable errors in the proposed rule and the draft biological report. Several of the Service's documents' interpretations and syntheses are neither reasonable nor scientifically sound. In several instances, a different and equally reasonable (or more) and sound (or more) interpretation has been reached in the scientific peer-reviewed literature. In several cases, results in the best journals (ranked independently on a worldwide scale of impact factors) were ignored or overlooked, in favor of non-peer-reviewed interpretations or results from lower-ranked journals. In a few cases, the stronger evidence was paid for by the USFWS or was co-authored by USFWS staff.

Summary of the scientific evidence in the order presented in my peer review

Following the purpose on p9649 of the proposed rule, I have evaluated "*data, assumptions, and analyses"* and the inferences and conclusions that followed, as scientific statements. For all of the data, assumptions, and analyses to be scientifically sound, I weighed whether I found them accurate, precise, and valid, both on their face, and in light of the literature with which I am familiar. Regarding assumptions specifically, I sought clear statements of assumptions and considered their validity once identified: were they reasonable and were assumptions more likely to be true than false. I flagged unstated assumptions because transparency about assumptions is a key principal of scientific integrity. For data and analyses, I attempted to weigh whether they met the highest standards of evidence, including whether they were generated, interpreted, and reported with high standards of integrity and scientific rigor— to assist the USFWS in considering the "best available" science under the ESA. Overall, I focused on 4 related scientific topics in the proposed rule and biological report. I also focused on the following claim,

"*The metapopulation in the Great Lakes area contains sufficient resiliency, redundancy, and representation to sustain populations within the gray wolf entity over time. Therefore, we conclude that the relatively few wolves that occur outside the Great Lakes area within the gray wolf entity, including those in the west coast States and lone dispersers in other States, are not necessary for the recovered status of the gray wolf entity*." p9683.

1. The first topic was the definition of the entity as it relates to the scientific criteria for recovering or delisting under the ESA (Endangered Species Act of 1973 as amended). I see this as a scientific issue because one must agree on how to group or split subpopulations of wolves that exist on the ground before scientists can evaluate a claim about their status. I found the rationale for the listed entity coherent. By contrast, I found that the handling of evidence for dispersal, and treatment of vacant habitat and different subpopulations of the gray wolf entity were not consistent in a scientific sense. As a result, the conclusions drawn about current range, vacant habitats, and northeastern USA gray wolves were not well substantiated. My scientific judgment is that the gray wolf entity 's current range is not defined well by scientific standards.

2. The second topic was the definition of suitable habitat. I found the definition of suitable habitat did not conform to standard practice in ecology and conservation, and moreover it contained an unstated value judgment in place of scientific observation.

3. The third topic was gray wolf mortality. I found that the presentation of data, review of literature, analysis, and predictions about gray wolf mortality were sometimes incomplete, sometimes imprecise, sometimes inaccurate, or sometimes invalid. Taken altogether, the interpretation of the threat posed by human-caused mortality and its cumulative effects experienced by gray wolves was inaccurate and misleading.

4. The fourth topic was human attitudes to gray wolves. I see this as a scientific issue because the measurement and interpretation of human attitudes can help to predict future human action to conserve or eliminate gray wolves. I found the review of literature and analysis of attitudes was incomplete, inaccurate, and overall misleading.

In sum, I do not find the proposed rule and draft biological report present the best available science and I made numerous suggestions for improving the identification, presentation, and the analysis of evidence in the draft biological report and in the proposed rule.

Addressing process of the peer review as an issue of scientific integrity

I also address scientific process throughout my peer review, because the best available scientific and commercial data depends on scientific integrity, consistent standards of evidence, and strong inference. The USFWS purpose for per review guided me in this direction because it made clear a base of fact precedes the decision,

"The purpose of peer review is to ensure that our decisions are based on scientifically sound data, assumptions, and analyses." p9649

I found an overall problem with the process for coming to the legal determination in the proposed rule. This is a scientific problem. The problem is that the draft biological report was released simultaneous with the proposed rule for the peer review. Logically, the draft biological report which should stick to scientific evidence and scientific inference should be peer reviewed long before a political and legal decision about delisting is made. The nature of a base of evidence is that it is solid before one builds on it.

In other words, the evidence should inform the value judgments that underpin a political and legal decision. Without the sequence I recommend, the proposed rule looks like a predetermined conclusion. Moreover, the proposed rule is full of presentations of evidence (often flawed) rather than the proposed rule referring to inferences that were drawn (and passed peer review) in the draft biological report before being used in the proposed rule.

Science cannot tell us what we should do. I am identifying a scientific problem and recommending an improvement to the scientific integrity of the peer review. The current mix in the proposed rule confuses evidence with policies based on legal and ethical reasoning. In my opinion, a clearer separation between fact and value judgments would make the process more scientific. Indeed, separating the ethical review – along the lines of one conducted by the USFWS for spotted owls recently (Lynn 2018) – from the scientific review would do much to dispel the current muddle between fact and policy in the proposed rule. Facts and evidence established by scientific consensus should be regarded as distinct from the ethical reasoning about what we ought to do.

1. **Definition of the entity and whether that entity met the ESA range criterion for delisting**

2. **Definition of suitable habitat**

3. **Human-caused mortality**

4. **Human attitudes to gray wolves**

5. **Biological Report**

Appendix

1. **Definition of the entity and whether that entity met the ESA range criterion for delisting**

I understand the legal definition of the gray wolf entity in the proposed rule.

"In this proposed rule, we consider the status of the gray wolf within the geographic boundaries of the two currently listed C. lupus entities... These two currently listed entities are: (1) C. lupus in Minnesota, and (2) C. lupus in the lower 48 United States and Mexico outside of Minnesota, the NRM DPS (Montana, Idaho, Wyoming, eastern third of Washington and Oregon, and north-central Utah), and the area covered by the experimental population area for C. l. baileyi…" p9653

I understand how the evidence of dispersal leads to a biological definition of discreteness, and I think I understand how that, in turn, applies to a legal definition for listable entities, starting with the treatment of the Pacific Northwest (PNW) wolves in the proposed rule:

We determined that these wolves are not discrete, under our DPS policy, from wolves in the NRM DPS… and, therefore, are ***not a valid listable entity under the Act****… Therefore, wolves in western Washington, western Oregon, and northern California are not a valid DPS because they are not discrete from the NRM DPS… " p9653 emphasis added.*

The above quotation clearly connects all wolves of Washington, Oregon, and California with the NRM wolves on biological grounds (dispersal in both directions). I believe the biological evidence is consistent with this claim. Also, the proposed rule goes on to link Wisconsin and Michigan wolves to Minnesota wolves through frequent dispersal (quite correctly, in my interpretation of several peer-reviewed papers). By this logic, the WI and MI wolves could not be a DPS either. So the PNW and NRM wolves proposed as one subpopulation, and we have the Minnesota, Wisconsin, and Michigan wolves as another subpopulation (WGL) of the gray wolf entity. So far so good. However, the criterion of discreteness was abandoned when it came to examining evidence of dispersal within the gray wolf entity.

If discreteness only applies to a DPS, then it would seem that the NRM could potentially be an invalid DPS. That might be a legal issue but scientifically, the handling of dispersal and discreteness was inconsistent in the proposed rule.

The facts of dispersal are not analyzed in the same manner to consider discreteness of the NRM DPS and Minnesota's wolves.

"*…a number of lone long- distance dispersing wolves have been documented outside core populations of the Great Lakes area and western United States since the early 2000s. Confirmed records of individual wolves have been reported from North Dakota, South Dakota, Utah, Colorado, Nevada, Missouri, Indiana, Illinois, Nebraska, and Kansas. The total number of confirmed records in each of these States, since the early 2000s, ranges from one in Nevada to* *at least 27 in North Dakota, with the latter also having an additional 45 probable but unverified reports " p9656*

Is Minnesota's entity non-discrete from the NRM DPS or the less-well-understood northeastern USA gray wolves? Are PNW wolves discrete from all the rest? Clearer terminology will strengthen the proposed rule and potentially reduce future disagreements.

Also, clarity about subpopulations within the gray wolf entity was hindered by a proliferation of terms in the proposed rule. Within p9655-9657, the proposed rule uses the terms metapopulation, extension of a metapopulation, subpopulation, "*core population*", and "*biologically part of (although outside the legal boundary of) an already recovered and delisted population*". The draft biological report does not present enough information on dispersal to clarify these terms.

I did not find an answer to my questions above or even a reasoned analysis about dispersers in the proposed rule or the biological report. Consider the scant information about northeastern USA wolves. The proposed rule pooled the northeastern canids with the gray wolf entity to delist, but did so without providing evidence such as data on dispersal, analysis of status, or an analysis of discreteness in either the proposed rule or the draft biological report.

Moreover, the proposed rule seemed to ignore vacant habitat while claiming to consider the status of gray wolves where they occur. It does not do so for the gray wolves in the Northeast. as the following quotations illustrate:

"Our analysis of threat factors below does not consider the potential for effects to C. lupus in areas where the species has been extirpated—rather, effects are considered in the context of the present population." p9659

Apparently, the proposed rule considers the status of wolves ONLY within the geographic boundaries of the two currently listed *C. lupus* entities.

"In this proposed rule, we consider the status of the gray wolf within the geographic boundaries of the two currently listed C. lupus entities to determine whether these wolves should remain on the List in their current status, be reclassified, or be removed from the List." p9653

The approach above seems to contradict the ruling by the 2017 decision of the D.C. Circuit Court of Appeals, which was cited in the proposed rule. While my comment might seem focused on policy, it actually reflects the need for additional evidence – on vacancy, occupied range, and dispersal – before delisting the gray wolf entity.

"[The U.S. Court of Appeals in 2017]… upheld the District Court's vacatur, concluding that the Service failed to reasonably analyze or consider two significant aspects of the rule: The impacts of partial delisting and historical range loss on the remainder of the listed entity." p9650

The D.C. Court of Appeals raises the critical issue of insufficient analysis. Although no specific percentage of range was mandated by the ESA, the claim that one can delist regardless of what portion of the listed entity's range is occupied seems an extreme position, a case of following the letter of the rule while ignoring its goal. Perhaps the USFWS is not obligated to recover a listed entity across all or a significant portion of historical range, but the range of the listed entity and a significant portion of that range both seem relevant for analysis. A facile assertion that "there are no wolves so there is no information" would be flawed. Scientists glean information from allied species when faced with an information gap about a particular place. For example, how are other large carnivores faring in habitat currently lacking gray wolves? More analysis of the vacant and the occupied range of the gray wolf entity is imperative. To improve clarity, the proposed rule should insert the plain terminology of the ESA, e.g. "the significant portion of range" clause.

Here the D.C. Court of Appeals raises the critical issue of thorough analysis. Although no specific percentage of range was mandated by the ESA, the claim that one can delist regardless of what portion of the listed entity's range is occupied seems an extreme position to take; a case of following the letter of the rule, but ignoring its goal. Perhaps the USFWS is not obligated to recover a listed entity across all or a significant portion of historical range, but the range of the listed entity and a significant portion of that range both seem relevant for analysis. The easy retort that 'there are no wolves so there is no information' would be flawed. For one, scientists glean information from allied species when faced with an information gap about a particular place. So for example, how are other large carnivores faring in the habitat currently vacant of gray wolves? Therefore I wanted more analysis of the vacant and the occupied range of the gray wolf entity. To improve clarity, the proposed rule should insert the plain terminology of the ESA, e.g., the significant portion of range' clause.

There is also a scientific question associated with what is a significant portion of range, given the definition of the listed entity's vast range (Figure 2). I would answer the question scientifically, as follows: Congress did not mean significant in its statistical sense (whatever that might mean) but in its common English usage. I also deduce they meant the geographic range of the listed entity as opposed to some other type of range. As a scientist, I doubt the definition of significant in common English would include less than half because we have a perfectly understandable phrase for that ("a minority of the range"). By the same logic, I doubt plain English understanding would mean 51% because they could have written "a majority of the range". I also doubt that plain English interpretation would include 100% because 'all' would be covered by the ESA phrase. So visual inspection of Figure 2 and the proposed rule both confirm that the gray wolf entity is currently not occupying a majority of the listed entity's range. Therefore, my scientific judgment is that the gray wolf entity has not recolonized enough of its range to meet the standard of a significant portion of range.

The scientific basis of the gray wolf entity and its range seems questionable on scientific grounds because I found neither consistent terminology for subpopulations of current wolves, nor consistent handling of data on dispersal, discreteness, range, or status across the entity.

2. Definition of suitable habitat

I agree with the proposed rule assertion, *"gray wolves are habitat generalists*" p.9654. But then to further claims,

"We consider suitable habitat as forested terrain containing adequate wild ungulate populations (elk, white-tailed deer, and mule deer) to support a wolf population. Suitable habitat has minimal roads and human development, as human access to areas inhabited by wolves can result in wolf mortality…" p9662 (citing Oakleaf, Mladenoff, Carroll)

First, habitat suitability is estimated at the scale of individual animals or breeding social units, not populations as in the latter quotation. In brief, for wolves, suitable habitat can be as small as the area needed for one breeding pair to raise pups that survive to adulthood. Second, it is standard practice in ecology to define suitability by observing where reproduction and survival occur to define suitability, not by imposing a human value judgment on it as hinted above and obvious in the following quotation.

"*Thus, the estimated 450 wolves in [Minnesota's] Zone B could be subject to substantial reduction in numbers. …* wolves *should be restored to the rest of Minnesota but not to Zone B (Federal Zone 5) because that area 'is not suitable for wolves' (USFWS 1992, p. 20)." p9669.*

The latter paragraph undermines the proposed rule's definition of suitable habitat because an area containing 450 wolves is suitable by its own definition.

The USFWS in 1992 confused a value judgment (e.g., we don't want wolves in Zone B) with a scientific evaluation of suitable habitat (e.g., wolves survive and reproduce here). The proposed rule endorses the 1992 recovery plan repeatedly, despite multiple unscientific statements about habitat suitability.

Third, defining a human behavior (wolf-killing) as a habitat feature is contrary to long-standing ecological practice. Not all humans kill gray wolves or even want to kill gray wolves (e.g., (Treves et al. 2013). Therefore, human density is a weak correlate of threat to wolves. Stronger correlates of inclination to kill wolves have been identified and they do not always occur where human population density is moderate or high (Smith et al. 2010). Therefore, any claim that the cause of suitability of habitat is the presence or density of humans would be erroneous.

I anticipate the rebuttal that habitat suitability indices often include mortality sinks or habitat patches that hinder reproduction or survival for wildlife. But ecologists do not define habitat as unsuitable because a predator **resides there**. Nor should the proposed rule define a habitat as unsuitable **because people live there**. Only when mortality or failed reproduction are recurrent phenomena in a restricted area might that area be classified as unsuitable. Therefore, the definition of suitable habitat does not seem to accord with standard practice in ecology or conservation science.

Clearly lacking from the proposed rule and draft biological report analysis of suitable habitat is a reasoned spatial model of where illegal killing and legal killing have been concentrated or are more likely in the future. All I can glean from the proposed rule about these topics is that after delisting, legal killing will increase as states and tribes make value judgments about where wolves will be allowed to recolonize. Such value judgments are not scientific evaluations of suitable habitat as explained above., Moreover the role of illegal killing deserves further analysis in light of the ESA admonition to treat overutilization (e.g., high human-caused mortality) as a threat to listed species. Defining human-caused mortality as a threat to be reduced seems to be the appropriate stance, rather than redefining spaces, in which human-caused mortality might occur, as unsuitable. The ESA permits predator control to protect listed species. The USFWS could treat illegal killing as predation and control it, rather than redefine it as an immutable factor in habitats.

Likewise, an unwillingness to curb human-caused mortality (by the agencies responsible) is a value judgment, not a scientific fact or prediction. Unwillingness to curb illegal killing does not make wolves less capable of using habitat. Similar points were made by (Bruskotter et al. 2013), in a document I am confident was shared with the USFWS. The latter seems to have been omitted from the draft biological report and proposed rule.

3. **Human-caused mortality**

    a. **Is human-caused mortality lower now?**

    b. **Illegal killing**

    c. **Wolf mortality in light of government measurements and regulatory mechanisms**

    d. **Legal killing to protect domestic animals**

    e. **Cumulative effects**

Starting on p. 9659, the proposed rule presented its summary of human-caused mortality. I do not find it to be a thorough and comprehensive review of the best available scientific and commercial data. Furthermore, even when the evidence summarized seems to be the best available, I find several key analyses and conclusions drawn from the review are unclear, illogical, or poorly reasoned. There is a serious gap in the draft biological report as pertains to wolf mortality also. Before recommending improvements to the proposed rule and draft biological report, I present findings from 2019 and I summarize a debate about wolf mortality dating from 2015–2018 in Box 1 below.

Box 1. Wisconsin and Michigan wolves appear to have been adversely affected by delisting and other policies that liberalized wolf-killing above and beyond the number of wolves legally killed.

Santiago-Ávila (in review) presented a table of the fates (time to event) for radio-collared wolves in Wisconsin 1980–2012 (Table A). In summary, half of all radio-collared wolves with a recorded fate had disappeared from 1980–2012 (n=243 of 485). Also, he found that periodic increases in the rates of disappearance were significantly associated with periods of permitted lethal control or delisting. Furthermore, the preliminary results about incidence (i.e., the proportion of individuals experiencing an event over time) of disappearances over time (or relative incidence) increased by 11–34% relative to periods with full protection.

The range of values depended on 3 scenarios for missing data and the resulting uncertainty left by gaps in state monitoring data. The state's 2012 population report and mortality data omit 26 radio-collared wolves entirely (5.1% of the sample). That was a majority of the 41 radio-collared wolves monitored from January 1–April 14, 2012. Months of requests for those data were rebuffed. Nevertheless, the observations, simulation and imputation scenarios converge in their results and together lead to a clear inference. The risk that a wolf was lost to monitoring rose 'considerably during periods with legalized wolf-killing. Table A presents the raw data.

It seems unlikely that radio-collars fail more often during such policy periods, or that emigration (but not immigration) changes when federal policy changes.

Table A. The distribution of disappearance events for 243 radio-collared wolves in Wisconsin 1980–2012 where 0 = periods without legal wolf-killing and 1= during briefer periods with legal wolf-killing. Results below correspond to a scenario of 12 out of 26 wolves imputed as Disappeared or lost to follow-up (45%), which is consistent with the overall proportion of wolves that disappeared (243/250 = 50%).

lib_kill	Events observed	Events expected
0	180	194.02
1	63	48.98
Total	243	243.00
	chi2(1) =	6.80
	Pr>chi2 =	0.0091

Note: I declined to present the full text of methods for the information above because I must protect the intellectual property of a junior colleague, however he informed me that he is willing to present the entire set of methods orally to the USFWS if needed. Therefore, this source of evidence is no different than the personal communications cited in the draft biological report and proposed rule, which do not contain full methods or manuscripts.

The above results derive from radio-collared wolves and corroborate initial suggestions from all wolves of Wisconsin (Treves et al.2017b) and population dynamics of wolves from Michigan and Wisconsin, which I summarize next.

In 2016, (Chapron & Treves 2016a, b) estimated the wolf population growth in Michigan and Wisconsin slowed by 5% with a year-long period of legalized wolf-killing. They demonstrated quantitatively that those periods of lethal management had slowed population growth in proportion to the length of time those periods lasted and regardless of the number of wolves killed. They could not find evidence nor a biological mechanism to explain why such a slow-down in growth would occur through reproduction or dispersal, so they inferred poaching had increased during periods of legalized killing. Such an explanation is consistent with Santiago-Ávila's results on radio-collared wolves being lost to follow up (off the air) at higher rates during periods of liberalized wolf-killing above.

The attempt by Pepin et al., 2017 and Stien, 2017 to rebut Chapron and Treves (2016a) failed, principally because neither provided biological evidence for the now dubious view that density-dependent growth had occurred (Chapron & Treves 2017a, b), and see Appendix here.

Also Olson et al. (2017) tried unsuccessfully to rebut the results of Chapron and Treves (2016a). Olson et al.'s prior analysis of poaching (Olson et al. 2015) had major shortcomings (Chapron & Treves 2017b) and Appendix here), their table of negative density-dependent population growth in wolves included studies that did not find such growth (Chapron & Treves 2017b), and Olson et al. again omitted essential information on Wisconsin wolf monitoring (Appendix here and (Chapron & Treves 2017b).

Although not directly addressing the preceding debate, work by Stenglein et al. (Stenglein 2014; Stenglein et al. 2015a; Stenglein et al. 2015b; Stenglein et al. 2015c; Stenglein & Van Deelen 2016; Stenglein et al. 2018) cannot support the rebuttals by Pepin et al. Stien, or Olson et al. because those papers do not accurately account for missing radio-collared wolves or biases in wolf census methods that might affect the detection of dead wolves (Appendix and Box 1). Nor does it contradict Santiago-Ávila (above). In sum, I find no credible rebuttal to Chapron & Treves (2016a,b) and it is now supported by Santiago-Ávila above, using different data and methods.

End of Box 1

a. Is human-caused mortality lower now?

The subtext or implicit assumption throughout the proposed rule are that since bounties on gray wolves no longer exist, all human-caused mortality now is regulated and therefore wolves will not be extirpated. This conclusion mistakenly assumes that bounties are the only cause of extirpation, and that other killings of gray wolves by humans do not match, even cumulatively, prior population losses. Extirpation depends on the relative mortality rate and reproductive rate, regardless of how mortality occurs. Several of my criticisms below, relate to the incomplete or missing quantification of mortality among gray wolves.

The assertion – ***"an active eradication program is the sole reason that wolves were extirpated from their historical range in the United States"*** (p9659 citing Weaver 1978, p.1.) – is both outdated and fails to define a key element, the "eradication program". Because the eradication program was not defined – just as the characteristics of a bounty are not specified in the proposed rule – scientists cannot evaluate the implicit assumption that today's wolf-killing programs plus illegal killing are different from past wolf-killing programs.

If the bounties of 50 years ago resemble the lethal control and public hunting, trapping, and hounding programs of today in rate and risk for gray wolves, then the burden of proof is to show these will be stopped rapidly and effectively before relisting targets are reached.

Furthermore, recall that the eradication of gray wolves in the WGL was due to a combination of factors (prey depletion, habitat loss, and human-caused mortality), not solely bounties (Thiel 1993). Therefore, the notion that gray wolf populations will persist despite the many increasing sources of human-caused mortality after delisting simply because states and tribes do not implement bounty systems seems unwarranted. At very least, we need a more careful analysis than the following effort.

The Stenlund 1955 comparison to Fuller 1989 suggests a possible decrease in mortality rate after bounties ended. The limitations of this one study are minimally acknowledged in the proposed rule. I don't agree with their conclusion that, *"Nonetheless, these figures provide clear support for the contention that human- caused mortality decreased significantly once the wolf became protected under the Act.",* because the studies even if comparable represent a sample size of 1 subpopulation before-and-after multiple changes in policy (bounty, several Acts of Congress, the ESA, and state policies between 1978 and Fuller's study years later). If such weak evidence were accepted in other topics (e.g., human attitudes, road density for suitable habitat), very different conclusions about delisting would have to be reached. A scientifically defensible inference from the Stenlund and Fuller studies if they are comparable would be, "gray wolf mortality declined between the 1950s and the 1980s, an interval in which several policies changed including the repeal of the bounty', habitat regenerated, and prey recolonized.

Likewise, the proposed rule misstated what we know about periods with and without legal wolf-killing (Box 1), when it asserted,

"Regulation of human-caused mortality has significantly reduced the number of wolf mortalities caused by humans, and although illegal and accidental killing of wolves is likely to continue with or without the protections of the Act, at current levels those mortalities have had little impact on wolf populations." p9661

Box 1 above contradicts the latter. Moreover, the proposed rule places the emphasis inappropriately on *"Regulation of human-caused mortality*' rather than on evidence that the WGL state regulations have reduced wolf mortality.

For example,

"*…the regulation of human-caused wolf mortality is the primary reason wolf numbers have significantly increased and their range has expanded since the mid-to-late 1970s.*" p9659.

I encourage the authors of the proposed rule to be more precise when making scientific assertions. Bounties represented up-regulation of human-caused mortality, and the ESA represented down-regulation of human-caused mortality, i.e., regulation up or down should be specified. This might seem trivial but for two implications of the latter quotation.

- **i.** The quotation seems to imply that removal of bounties and replacement by regulated killing solved gray wolf extirpation. A naïve reader might interpret the quotation above to encourage more regulated wolf-killing. Imprecise language leading to inaccurate conservation prescriptions has been discussed recently in similar contexts (Treves et al. 2018).
- **ii.** Regulation has to manifest in behavior to be effective. Regulation by itself is paper protection. That insight applies to the various assurances that currently state regulations are adequate to avoid a repeat of the history of extirpation.
- **iii.** Was gray wolf range expansion facilitated more by recolonization of prey and regeneration of forests or by lifting of bounties? Alternate views include that societal values, habitat and prey restoration, or ESA listing of the gray wolf per se (without the prohibition on take being enforced) were sufficient to permit wolf recolonization (Mladenoff et al. 1997; Naughton-Treves et al. 2003; Schanning 2009; Smith et al. 2010; Treves & Bruskotter 2014). Even if we accept that past bounties caused more mortality than present private action, the proposed rule and biological report should show that current human-caused mortality is lower than past human-caused mortality as a hazard rate (% of wolves dying from that cause in a given time interval). It is not sufficient to point to population increase and range expansion since 1978 to argue *ipso facto* that the major cause of mortality has been removed or lessened. For greater scientific validity, we need efforts to understand mortality and wolf population estimates historically (Appendix and Box 1). That begins by rigorous measurement and interpretation of today's mortality rates and risks.

The proposed rule must present evidence why delisting will not repeat the past and jeopardize the WGL subpopulation. That explanation should include that much has changed in addition to bounties since 1950-1965, including increases in vehicle traffic, access to firearms, and wildlife-killing practices and technologies that did not exist in the 1960s. To my eye, the proposed rule does not adequately consider the modern risks and deregulations of the means to kill wolves.

b. Illegal killing

"Many wolf killings, however, are intentional, illegal, and never reported to authorities." p9660.

The proposed rule rightfully acknowledges this important point because the major cause of U.S. wolf mortality today is illegal killing (Treves et al. 2017a). However it was followed by many paragraphs that I find misleading, starting with a misstatement of the problem in measuring illegal killing.

"The number of illegal killings is… impossible to accurately determine because they generally occur with few witnesses." p9960.

The number of concealed wolf mortalities is very difficult to measure precisely, but it is not difficult to estimate it accurately with defined bounds of uncertainty, as the following two examples demonstrate. Several recent analyses have done so for radio-collared gray wolves in the USA (Schmidt et al. 2015; Treves et al. 2017a). Recall that the USFWS repeatedly miscalculated human-caused mortality for radio-collared wolves with a systematic bias (Treves et al. 2017a). Also, for unregulated mortality in Alaska, scientists estimated >80% of mortality went unreported to the state agency administering a program of legal wolf-killing (Adams et al. 2008). That troubling conclusion cast doubt on the confidence the proposed rule places in well-regulated, legal wolf-killing discussed next.

The best available evidence indicates that the majority of gray wolves died from illegal killing in the NRM and in Wisconsin **during the periods of ESA protection**, not to mention red wolves and Mexican wolves (Treves et al. 2017a; Treves et al. 2017c). Omission of those facts in the proposed rule and draft biological report is noteworthy for other reasons also. Treves et al. presented the raw data and based their conclusions on recalculation of mortalities published by the USFWS and allied scientists in state and federal agencies. In one case (NRM gray wolves), the agencies' miscalculation led to misidentifying the major cause of gray wolf mortality (Treves et al. 2017a). Because illegal killing of gray wolves was the major cause of their mortality despite ESA protection, the proposed rule should analyze the evidence that delisting gray wolves will not lead to dramatic increases in mortality when illegal killing is counted along with legal killing. The proposed rule seems to have ignored a valuable source of inference about the consequences of delisting in not reviewing the scientific debate over the effects of legaizing wolf-killing (also referred to as "tolerance hunting" (Epstein 2017; Epstein & Chapron 2018); Box 1). This is a key gap in both the proposed rule and the draft biological report.

"Liberg et al. (2011, pp. 3–5) suggest more than two- thirds of total poaching may go undetected, and that illegal killing may pose a threat to wolves; however, poaching has not prevented population resurgence in either the Great Lakes area or the northern Rocky Mountains, as evidenced by population growth in those areas... During the times that lethal control of depredating wolves was conducted in Wisconsin and Michigan, there was no evidence of resulting adverse impacts to the maintenance of a viable wolf population in those States." p9660.

The above quotation misses the mark in several ways. First, it is not consistent with evidence in Box 1, and it is not apparent what the proposed rule means by "resurgence." Second, an estimate of unreported, concealed poaching was provided by Treves et al. 2017b (50%), but overlooked in the draft biological report and proposed rule. Third, where are data from 2012–2014, when wolves were delisted in the WGL? I am aware of only one peer-reviewed analysis of how the populations of the WGL were affected by public hunting and trapping seasons in 2013, and that analysis did not answer important questions about missing methods for wolf census or attendant biases associated with incomplete reporting of dead wolves (Appendix and Box 1).

c. Wolf mortality in light of government measurements and regulatory mechanisms

The adequacy of regulatory mechanisms to control human-caused mortality depends heavily on the adequacy of measuring mortality. If one cannot measure wolf mortality without systematic error, the regulation and enforcement of wolf-killing become systematically biased in the scientific meaning of that phrase. What follows is a summation of the inaccuracies, imprecision and misinterpretation of gray wolf mortality data contained in the peer-reviewed literature.

The discussion of wolf mortality in the proposed rule and the draft biological report largely misrepresents the evidence for super-additive mortality from Creel & Rotella (2010)) and (Vucetich 2012); the latter was published in a USFWS document as part of the public record in the Wyoming wolf peer review. I am aware that Creel & Rotella (2010) was disputed by (Gude et al. 2012) but Vucetich then resolved the discrepancies between the two and still concluded that high human-caused wolf mortality was likely to result in a decelerating wolf population growth and an accelerating decline in wolf population abunfances (Vucetich 2012). See the section on the draft biological report below for why the proposed rule presents a misleading picture of sustainable rates of human-caused mortality and also (Creel et al. 2015) for NRM wolves.

In addition to a misleading summary of sustainability research, the proposed rule and draft biological report completely miss key evidence. Regarding the adequacy of mortality data, the proposed rule contains an unstated assumption that we have accurate and precise measurements of mortality from the states' monitoring programs. To the contrary, unchallenged scientific evidence indicates that rates of mortality (% of wolves dying in a given time period) have been under-estimated, and risk of mortality (the proportion of dead wolves dying of a given cause) has been miscalculated, particularly with regard to legal killing and illegal killing as explained below

(Treves et al. 2017a; Treves et al. 2017c) have demonstrated arithmetically that the state and federal wolf monitoring programs produced systematically biased estimates of the risk of mortality because they miscalculated the effect of disappearances in such a way that under-estimated illegal killing by large margins (also see Box 1). That analysis dealt with risk of mortality (the proportion of all dead wolves that died of a given cause), but analyses of mortality rates also raise concerns about the proposed rule.

In two studies, the rate of mortality was under-estimated by current methods of monitoring and analysis (Schmidt et al. 2015; Treves et al. 2017c). Furthermore, a majority of radio-collared wolves in at least one state (Wisconsin) went missing, fate unknown (Treves et al. 2017c) and

Box1. Contrary results by Olson et al. (2015) do not accurately account for missing radio-collared wolves or biases in wolf census methods that might affect the detection of dead wolves (Appendix and Box 1). The proposed rule and draft biological report fail to mention that inferences based on observed mortality of radio-collared wolves are imprecise, and the inaccuracies result in under-estimating illegal killing (Liberg et al. 2012; Treves et al. 2017a; Treves et al. 2017c).

I also found several problems with the proposed rule's handling of legal wolf-killing under state regulatory programs.

"We anticipate the level of mortality due to depredation control that would take place would be similar to what was observed during those times… [with permits or brief delisting 2003–2012]." p9660.

There is an error of extrapolation in this assertion. During recent short periods with permits or delisting (2003–2012), states received federal permits that either stipulated the number of wolves that could legally be killed, or else the period of delisting was brief, a very different scenario from the proposed rule whereby a state would not have to seek federal permission and the time frame for legal killing could be lengthy. For example, after the WGL delisting in January 2012, the state used lethal control to kill an unprecedented number of wolves, invoking catch-up' killing to compensate for not having had such authority the previous year (Treves et al. 2017b), supporting information) with public hunting and trapping allowed as well. Therefore, the expectation cited above seems fanciful and unlikely to come true. The reality is that gray wolf mortality is likely to be higher under the proposed rule than in the brief periods of delisting and issuance of permits for lethal control in the past.

Even if I set aside the scientific questions about whether non-target animals are killed and whether such killing protects human interests at all (Treves & Naughton-Treves 2005; Treves et al. 2016), I found two implicit assumption in the proposed rule's' treatment of wolf-killing to protect livestock.

The first assumption is that killing wolves is effective in protecting human interests. If it is not effective, the USFWS should discourage it, regardless if it has no effect on the viability of wolf populations. The USFWS should discourage it because it is expensive, wasteful, and adds incrementally to wolf mortality. A proposed rule for delisting wolves can and should discourage ineffective management interventions that add to cumulative threats. Indeed, a worldwide review of evidence published by 21 authors from 10 nations (van Eeden et al. 2018) observed the scant evidence and the generally poor quality of evidence used in USA and other government predator-killing programs. Using the most rigorous criteria for evaluating the effectiveness of lethal control, Treves et al. (2016) found more evidence of counter-productive results that would raise the risk for livestock. Research to date provides evidence both for and against the assumption that killing problem wolves protects domestic animals. A longitudinal analysis of wolf pack areas in the NRM suggested that killing entire wolf packs would reduce future livestock losses (Bradley et al. 2015). (A set of three weaker, correlational analyses –

starting with Wielgus et al. - have debated similar data from the NRM, but they merely involved correlations without spatial information.) Although Treves et al. (2016) accepted Bradley et al. (2015) as reliable, that no longer seems appropriate in light of the more recent study by Santiago-Ávila et al. (2018), which points out that Bradley et al. introduced a bias in favor of lethal control by following wolf pack territories rather than wolf packs, because that led them to count intervals in which an area contained no wolves as intervals of effectiveness of lethal control. That approach inflates the apparent effectiveness of whole-pack removal, because the authors counted the period of vacancy as if risk to livestock continued. The sometimes-lengthy period of vacancy would require recolonization and breeding by colonizers before risk resurfaced, which introduces a systematic bias favoring removal of entire wolf packs over other methods. Also Santiago-Ávila et al. (2018) pointed out methodological defects in Bradley et al. (2015) including incomplete data and methods; the latter authors could not provide data and a request for such material was not met. These are critical departures from the scientific requirement of replicability. Relying on Bradley et al. (2015) would be inappropriate unless the methodological problems are fixed, and the data published transparently.

The second implicit assumption was that wolf-killing is self-limited versus the possibility that it accelerates each time wolves are killed. By self-limiting, I mean that wolf-killing stops because the 'problem' wolves are removed and hence the rate of such killing tends to be low and diminish over time. The evidence for and against the assumption is not presented.

Santiago-Ávila et al. (2018) corrected the shortcomings in Bradley et al. (2015) and deployed better methods to the study of lethal control of Michigan wolves. They found corroborating evidence that lethal control did not prevent future livestock losses, and reported that any small reduction in risk for affected farms was outweighed by a subsequent greater risk for farms in neighboring townships. They interpreted patterns of wolf and livestock death to mean that lethal control might cause spill-over effects leading to more losses of domestic animals in subsequent periods. In a state like Michigan that did not monitor the effectiveness of killing wolves legally, and any state that contracts the same federal agency as Michigan did, might erroneously augment wolf-killing after spill-over effects spread to other farms.

Currently, the best available evidence suggests to me that lethal control is risky for domestic animals on farms and is not self-limiting, hence it leads to yet more wolf-killing. Incidentally, social scientific data on human inclinations to kill wolves are consistent with these biological results (See below).

The above concerns relate to the past, observed patterns of legal wolf-killing. Now consider Minnesota's policies and statements about wolf-killing, which are particularly important given Minnesota's essential role in preserving the WGL subpopulation for the entire gray wolf entity.

Without leaps of imagination on my part, I read Minnesota's management plans and the USFWS recovery plan of 1992 as predicting a high likelihood of unprecedented wolf-killing in Minnesota (MN). Consider for example,

"Thus, the estimated 450 wolves in [Minnesota's] Zone B could be subject to substantial reduction in numbers. … At the extreme, wolves could be eliminated from Zone B…(USFWS 1992, p. 20)… there is no need to maintain significant protection for wolves in Zone B in order to maintain a Minnesota wolf population that continues to satisfy the Federal recovery criteria after Federal delisting." p9669.

Moreover, the proposed rule seems to anticipate changes in regulatory mechanisms, which seem to be in the direction of more wolf-killing, in Zone B, when they state,

"Significant changes in wolf depredation control under State management will primarily be restricted to Zone B, which is outside of the area necessary for wolf recovery (USFWS 1992, pp. 20, 28)." p9669.

Note the value judgment above about what is necessary for wolf recovery. Also, wide latitude has been given to private individuals to kill wolves in MN, as p9668-9669 plainly show. Assurances seem hollow without evidence, as in the following,

"We conclude that this action is not likely to result in the killing of many additional wolves, as opportunities to shoot wolves ''in the act'' would likely be few and difficult to successfully accomplish, a conclusion shared by a highly experienced wolf-depredation agent (Paul in litt. 2006, p. 5)." p9669

It would be more scientific to say that this is a prediction. Predictions entail assumptions and the assumptions here are notable. The proposed rule assumes that (a) owners do not bring their domestic animals to sites in which wolves congregate (e.g., dens or rendezvous sites or carcasses) to expose their animals to imminent risk as was apparently done in Washington State recently, and (b) owners do not bring with them means to kill multiple wolves in a short period (e.g., poison bait, automatic weapons). The possibility of a change in human behavior can reasonably be considered without great imagination. Imagine a Zone B wolf-killing contest which is not far-fetched because such contests have become common across the USA and routine in neighboring Wisconsin in the case of coyotes. I recommend the USFWS consider whether the MN DNR rules resemble a bounty and if not, what are the likely cumulative effects of each element of the MN rules, including whether they might represent a continuous drain on neighboring states or Zone A.

d. Cumulative effects

The proposed rule concluded that cumulative effects of delisting will not imperil WGL wolves. The MN wolves seem vital to that claim. On p9657, the proposed rule discusses Minnesota wolves at length and proposes a population goal of 1251-1440 wolves for long-term viability and genetic diversity. I suggest the draft biological report should fortify its evidence for this assertion by relating the effective population size to published recommendations on mammalian population sizes known to be viable and genetically diverse. For example, the proposed rule might use the best available science to present the probability that a population of 313-350 breeding females (assuming a pack size averaging four, which is typical in the WGL) will go extinct in the face of a demographic or environmental event causing massive mortality or cessation of reproduction for 1-2 years. Presenting reasoned evidence from population viability analyses would fortify overall scientific evidence for the MN population goal described on p9657. Without such reasoned evidence, the assurances in the proposed rule sound hollow.

Without such information, it is difficult to evaluate scientifically the USFWS claim that,

"this region contains sufficient wolf numbers and distribution to ensure the long-term survival of the gray wolf entity. " p9658.

To consider cumulative effects fully, the draft biological report should have presented state-by-state estimates of rates of mortality (observed and unobserved for radio-collared wolves, assuming mortality for those that went missing rather than simply omitting them). Regrettably, the proposed rule considers (see its Table 2) mortality only from lethal control in protecting domestic animal losses and from public hunting and trapping , without considering other causes (illegal, vehicular, nonhuman) and without proper attention to unexplained disappearances of radio-collared wolves (Box 1 for example).

Despite such defects, I pooled lethal control with the harvest data and found errors in the proposed rule. Combining the following scattered items – *"The number of wolves killed for depredation control while wolves were under State management for the second time (20121–2014) was slightly higher (203 wolves in 2011, 262 in 2012, 114 in 2013, and 197 in 2014) than during 2007 and 2008, but was still consistent with those killed under section 4(d) in the surrounding years (192 wolves in 2010 and 213 in 2015)."* p9669 with the USFWS estimates of the MN wolf population https://www.fws.gov/midwest/wolf/aboutwolves/mi_wi_nos.htm, and Table 2 – for four of the years covered in the above quotation, I offer Table B as a recalculation to help address the cumulative effects of delisting.

Table B. Legal wolf-killing in Minnesota before and during periods with state authority.

Winter	Population estimate	# killed in depredation control	# harvested	% killed legally
2015-2016	2,278	213 (9.3%)	0	9.3%
2014-2015	2,221	197 (8.8%)	272	21.1%

2013-2014	2,423	114 (4.7%)	238	14.5%
2012-2013	2,211	262 (11.8%)	413	30.5%

Footnote: I find the annual average for 'depredation control' to be 8.7% (4.7–11.8%), which is slightly higher than that presented in the proposed rule's Table 2.

Because the proposed rule itself predicted harvests will be repeated, the cumulative depredation control and harvest (legal kill) percentages should range from 14.5-30.5% (average 22.0%), without factoring in catch-up killing or higher quotas. The years 2012-2014 are most pertinent to that issue, because gray wolves were delisted and subject to hunting in some or all of those years in some or all of the states of the WGL. The range of 14.5-30.5% is not all the human-caused mortality yet it is already near or over the well-measured thresholds of 17% and 29% mortality rates before wolf populations are very likely to decline (Adams et al. 2008; Vucetich 2012).

The range of 14.5-30.5% does not account for vehicular, nonhuman, or the largest source of wolf mortality (poaching). Treves et al. (2017a) calculated that poaching exceeds legal wolf-killing when wolves are federally protected (and often 2 to 3 times higher). Recall also that delisting did not reduce poaching of radio-collared wolves (contra Olson et al. 2015 examining only observed wolf poaching), legal killing appeared to increase poaching (Box 1). illegal killing is virtually certain to add >9% to population declines, with lethal control, (Chapron & Treves 2016a,b) not even counting the possible rise in poaching with legal public hunting, trapping, or hounding. The weight of evidence suggests that poaching will increase and it is already the major source of mortality and is not well measured by state or federal agencies. Therefore, by past trends without any precaution against new ways of killing wolves at higher rates (see above), MN wolf delisting is almost certain to lead to wolf population declines. The size and speed of those declines depends on categories of mortality that the proposed rule and biological report do not carefully quantify or discuss at length. Therefore, a catastrophic decline in the MN wolf population post-delisting is entirely foreseeable.

In light of all WGL state policies legalizing wolf-killing after delisting and in light of under-estimating mortality in Table 2 of the proposed rule and in Box 1 that I present, I have to conclude that the proposed rule and draft biological report provide unwarranted assurances about the safety of wolves in the WGL after delisting.

The proposed rule initially does well to point out that the five-factor analysis mandated by the ESA and regulatory instructions that follow from it, require that all factors be examined individually and in conjunction for their cumulative effect. However by p. 9658 that lesson seems to have been forgotten:

"*However, the mere identification of factors that could affect a species negatively may not be sufficient to compel a finding that the species warrants listing. The information must include evidence sufficient to* ***suggest that the potential threat is likely to materialize and that it has the capacity (i.e., it should be of sufficient magnitude and extent)*** *to affect the species' status such that it meets the definition of an endangered species or threatened species under the Act.*" p9659 emphasis added.

This argument in the proposed rule is unscientific. While each separate threat might not pose an existential threat to a population, an accumulation of threats might well do so. Similarly, different populations within a listed entity might face different threats that pose a cumulative existential risk to each of those populations, such that looking at the entire listed entity may fail to identify a single existential threat even when the listed entity should be viewed as threatened or endangered by the cumulative effects from different threats. Because the proposed rule and draft biological report fail to adequately review the data for best available evidence, I cannot agree with the inference that delisting will not lead to excessive cumulative effects and to wolf population decline and possible collapse too quickly to be averted by relisting.

4. Human attitudes

"It took a considerable length of time for public attitudes and regulations to result in a social climate that promoted and allowed for wolf recovery within the gray wolf entity. The length of time over which this shift occurred, and the ensuing stability in those attitudes, gives us confidence that this social climate will persist." p9659 and

"*It is also possible that illegal killing of wolves in Minnesota will decrease, because the expanded options for legal control of problem wolves may lead to an increase in public tolerance for wolves (Paul in litt. 2006, p. 5).***"** p9669.

These assertions are not evidence-based. The citation to Paul is outdated and does not reflect any measurements of tolerance for predators or understanding of human behavior. The proposed rule and draft biological report neglected to cite research most relevant to the question of how attitudes to wolves change with policy changes (Browne-Nuñez et al. 2015; Hogberg et al. 2015) The former study used focus groups and data analysis (quantitative and qualitative) undertaken both before and after delisting and changes in policy that re-initiated lethal control of wolves. Browne-Nunez et al. (2015) found that attitudes to wolves did not change, and poaching plans appeared to stay the same or increase. Calls to kill more wolves through public hunting and trapping actually increased. Hogberg et al. (2015) used aquantitative mail-back survey to residents living in wolf range, comparing attitudinal measuresfrom the same persons sampled in 2009 and resampled in 2013. Hogberg et al. found that asample of residents of wolf range in Wisconsin had lower tolerance for wolves compared totheir tolerance in 2009. The most significant change in policy was the inauguration of a wolfhunting and trapping season in 2012, after delisting and associated lethal control in two periodsfrom 2009-2012. Therefore, the best available evidence shows that tolerance for wolves and inclination to poach wolves actually **increases** after delisting and during periods of liberalized wolf-killing .

Also, the USFWS misinterprets the review by Treves & Bruskotter (Treves & Bruskotter 2014), which inferred that policies and social norms can rapidly change attitudes to lower tolerance for predators and increase inclinations to poach predators. Missing the former articles and the point of the latter, the USFWS then asserts,

"Thus, it is unclear how delisting and the changes in wolf management subsequent to delisting, such as implementation of wolf harvests, may affect attitudes, human behavior and, ultimately, wolf mortality. " p.9662.

Given two independent empirical studies using different methods and different subjects at a different date, albeit in the same region, and a review cited above, the proposed rule errs in not summarizing these findings. The USFWS funded un-cited studies led by Browne-Nunez (a USFWS employee) and by Hogberg, so the agency cannot fairly claim ignorance about them. Also, Bruskotter et al. 2013 (entire) long ago addressed the weaknesses of the USFWS position on human attitudes to wolves.

The proposed rule's citation to Paul in litt. 2006, above, and the draft biological report's lack of scientific review of individual wolf-killing are troubling insofar as it seems to promote favorable, unqualified opinions over unfavorable evidence rather than addressing uncertainty, fundamental scientific debates or the weight of evidence. It is striking how diligently the proposed rule quantifies minor details of wolf biology (e.g., p9662 has dozens of citations and statistics on wolf use for various road densities) but entire sections on human attitudes and mortality risk and rate remain unquantified, despite the USFWS having access to studies that quantify these phenomena. No scientific justification supports that emphasis. Therefore, I find the proposed rule (and draft biological report next) are unscientific on their face.

Draft biological report

I generally found the biological report to be fairly strong within the limits of what it covers, except for the section on Wisconsin's wolf census and population model (see my Appendix) and on Minnesota's wolf census (see below). I write "within the limits" advisedly because the biological report overlooks essential information. Specifically, it substantially omits evidence relating to human-caused mortality and the cumulative effects of all causes of mortality or reproductive failure of wolf packs. I begin with the substantial gap and end this review of the biological report with suggestions for revision of several misleading paragraphs.

A major problem with the biological report is the entirely absent handling of the biological causes of wolf mortality. Namely, the biological causes of the vast majority of wolves are humans and the process and pattern of (and intervention against) human-caused mortality is completely ignored in the biological report. By analogy, if one avoided discussion of a nonhuman predator and its predation on another endangered species, one's findings would be seen as having a glaring gap. Likewise the ecology of the wolf as presented in the biological report fails to discuss the wolf's primary predator, humans. This gap leads to major errors in the proposed rule. The biological report should be revised substantially, to deal scientifically with the patterns and processes of human-caused mortality in wolves.

Understanding human-caused mortality of wolves requires an understanding of human attitudes, because much of the mortality of wolves is caused by human intentions rather than accidents. The biological report is wholly lacking in pertinent vital information on human attitudes and behavior as they relate to human-caused mortality in wolves. Therefore, the many claims about the survival of wolves after delisting, claims about suitable habitat and consideration of the risk of extirpation after delisting are not informed by a scientific analysis of the peer-reviewed literature on human-caused mortality.

For the biological report to address cumulative effects of mortality and reproductive failure in wolves of the WGL and PNW, it must be revised to meet the scientific mandates of the ESA five-factor analysis. It is impossible to scientifically evaluate the likelihood of a decline of gray wolf populations after delisting without a thorough and comprehensive look at all mortality causes within each subpopulation deemed essential to the gray wolf entity, followed by a thorough examination of cumulative effects across all subpopulations. Scientists need rigorous, peer-reviewed evidence concerning current causes of mortality plus causes anticipated following delisting, along with reasonable estimates of probable catastrophic declines. Absent such evidence, a reasonable scientist cannot assume that states can respond effectively and rapidly to prevent population declines below relisting levels, nor can we evaluate the likelihood of substantial changes in mortality rate for gray wolves in the foreseeable future. Assurances from one or even a handful of self-appointed experts do not constitute valid in scientific conclusions.

It is not adequate to assure us that Canadian wolves will repopulate WGL or PNW subpopulations without offering any supporting data on wolf immigration from Canada.

Likewise, the biological report also lacks any meaningful assessment of standards of evidence. It contains questionable conclusions that do not adequately consider disagreements within the scientific literature, basically treating them as if all sources of evidence were equal in strength of inference.

Instead, different sources should be evaluated in light of each other. Stronger inferences, hence stronger evidence, comes from sources that are more transparent, that consider contrary evidence and provide scientific reasons for why certain sets are weaker, and then present either a better method for drawing inferences or explain contrary results in a more robust manner. A paper that ignores contrary evidence fails on its face. These scientific principles are well illustrated by the recurring problem of citing Gude et al. (2012).

Vucetich (2012) explained why the claim that 48% anthropogenic mortality would be sustainable is in error and misleading. Until Gude et al. (2012) either correct their estimate, or else scientifically rebut the observations of error asserted by Vucetich (2012), Gude et al.

(2012)cannot fairly be listed as best available science. I view the following statements about Gude et al. (2012) as scientifically irresponsible:

"Some studies suggest that the sustainable mortality rate may be lower, and that harvest may have a partially additive or even super-additive effect (harvest increases total mortality beyond the effect of direct killing itself through social disruption or the loss of dependent offspring) on wolf mortality (Murray et al. 2010; Creel and Rotella 2010), but there is substantial debate on this issue (Gude et al. 2012)."

In my view this quotation is inaccurate to the point of being misleading. First, there is no credible evidence to contradict the finding that human-caused mortality has led to more wolf deaths than expected from permitted killing. Second, a plausible mechanism for the super-additive mortality has been proposed (Box 1 and citations to Brainerd et al. and Borg et al. above). Although, super-additive mortality resulting from instability or infanticide in wolf packs is hotly debated at present, the more likely mechanism proposed for super-additive mortality and the accelerating decline of wolf populations modeled by Vucetich (20125) is that legal killing prompted illegal unreported killing of wolves (Chapron & Treves 2016a,b, 2017a,b). And more recent results corroborate that idea with independent data (Box 1).

My last specific point relates to the Minnesota wolf census of 2012–2013 (Erb & Benson 2013). Given the ostensible importance of the Minnesota wolf population for the security of the WGL wolves, it would seem important to the USFWS to validate the Minnesota wolf census if one wishes to persuade scientists that this lynchpin population is secure. That case should be made in an appropriate scientific journal in front of truly independent reviewers if the claim of best available science is to be made. The USFWS has had years to encourage such a move towards the best available science. I am not aware if this census or a subsequent one has been subjected to rigorous, scientific peer review.

If I were asked to peer review Erb & Benson (2013) as it currently stands, I would point out several shortcomings in the methods that need revision. The major shortcomings are a lack of appropriate sensitivity analysis and handling of uncertainty, which together undermine my confidence in the accuracy of the 2013 Minnesota wolf population estimate and its putatively narrow confidence interval. With even a conservative effort at sensitivity analysis, the lower bound might cross the delisting threshold of 1251. Therefore, I describe those shortcomings below for the purpose of recommending that a revision to the draft biological report wait for a more scientific estimate of that population size as I attempt below.

Wolf biologists including myself have long acknowledged that it would be a daunting task to count every wolf pack in Minnesota. I accept that one must extrapolate from a sample. Such extrapolations are tricky and demand sensitivity analysis after transparent and rigorous handling if uncertainty in measurements, which further emphasizes the need for peer review of how extrapolation has been done. Extrapolation is tricky because of the risk of introducing sampling bias (how was the small sample of wolf packs chosen to treat it as representative of the state as a whole?) and measurement uncertainty (was the sample measured precisely and accurately to avoid propagating error by multiplying inaccurate or imprecise variables with each other?), among other issues.

Regarding the uncertainty due to sampling bias, the Minnesota estimate depends on "mean scaled territory size" estimated from a sample of 36 wolf packs (see their Figure 2 for the geographic distribution of those territories, two packs did not provide midwinter pack size counts). These 36 represent 8% of the total number of packs in the state, which on its face raises concerns about representativeness of even the best random sample of packs. The selection of those packs was not random but appears haphazard (without relation to the goal of extrapolating to the state), so we have no information on how representative the 36 wolf packs might have been. The analogy would be if the U.S., Census Bureau did not randomly sample for its long-form census but instead relied on a handful of other studies that had selected their samples by unknown and different criteria. Had the wolf packs been chosen randomly we might extrapolate with confidence. Haphazard sampling is not the same as random sampling because the more convenient sites or wolf packs tend to be sampled. For example, a common practice in carnivore biology is to study animals that are not likely to be killed during your study period, which would waste time and resources.

For example, wolf packs that are vulnerable to annual lethal control as conducted in Minnesota might be poorly represented in the sample of 36. Such packs subject to lethal control also tend to be smaller than average in Wisconsin (Wydeven et al. 2004a) for a variety of reasons. Therefore, extrapolating from larger packs selected for the convenience of other studies might tend to inflate the apparent size of the statewide population. I did not attempt to account for that difference between packs because I chose to be conservative in my sensitivity analysis. But someone else employing the precautionary principle, upheld by the U.S. Supreme Court ruling on the first ESA Case in TVA v Hill 1978, might argue that an endangered species decision demands precautions, so one should assume the majority of wolf packs in Minnesota average smaller size than the 36 wolf packs reported by Erb & Benson (2013).

Regarding measurement uncertainty, consider the following equation used to estimate N, the Minnesota wolf population size estimate: N = ((km2 of occupied range/mean scaled territory size)*mean pack size)/0.85." p.3, (Erb & Benson 2013). This pleasingly straightforward and simple equation raises a few questions about measurement uncertainty.

From left to right, the first variable is the area of occupied range. A precise estimate of the possible error in this value is impossible to gather from(2013), but they report ""total wolf range was estimated to be 95,098 km^2", p.4 and they provide a qualitative sense of uncertainty in the following statement, "Of the total estimated occupied range, 70% was confirmed to be occupied based on pack detection in the township and 30% was presumed to contain packs because of low human and road density..." p.4, (Erb & Benson 2013). Therefore, I treat the lower bound of their estimate of occupied area as 70% of the estimate they ended up using of 70,579 km^2 (i.e., eliminating the 30% of area for which no wolves had been observed). Note that a less conservative approach would require that evidence of breeding wolves be provided for every mapping unit (township) included in the range. The logic behind such a criterion would be further strengthened by the equation's enormous (and unjustified correction for lone wolves, see further below)..

The next variable was the average territory size. My efforts at estimating uncertainty could not unravel how the wolf pack territory sizes were estimated, given that radio-locations were removed from range estimates and two subsets of packs were handled differently – 19 estimates of size of well-studied packs' territories were used without correction but 17 estimates of size were scaled up by 37%, because too few telemetry locations were obtained, to correct for putative under-estimates (Erb & Benson 2013). These procedures might be valid but it is currently impossible to be certain. Nevertheless, we can estimate a minimum level of uncertainty. Had we been given the standard deviation of territory size (Erb & Benson 2013), which is an elementary statistic, we might have calculated the uncertainty around that average. Without that information, I will assume the standard deviation (SD) of pack territory size in Minnesota is the same as in Wisconsin from 2001–2006 (Wydeven et al. 2009), which was ±67 km^2. Hence, I estimate the standard error of the mean (SEM = SD / square root of n) for the 161 km^2 average territory size (Erb & Benson 2013) was 11 km^2. Again, a less conservative approach would have used the SD itself, not the narrower SEM.

Likewise, mean midwinter pack size estimated from 34 wolf packs was 4.3 wolves in Minnesota (Erb & Benson 2013) and probably an over-estimate statewide for the reasons I discussed above. By comparison; in 1997, all Wisconsin wolf packs averaged a size closest to the Minnesota pack size estimate, at 4.1 wolves in midwinter with SD ±2.1 (Wydeven et al. 2009). Therefore, I used that SD to estimate the SEM for the 36 Minnesota wolf packs at SEM 0.4. Again, a less conservative approach would use the SD or even the statewide pack size average from Wisconsin in 2013 (after a wolf hunt like in Minnesota) that was closer to 3 wolves per pack.

The last value in the above equation that has uncertainty is the apparent constant of 0.85, which was used as a correction factor for lone wolves, citing (Fuller et al. 1992). To estimate uncertainty about this value, I use both the estimate of the average and the SD from Wisconsin of 3.2% ± 0.7 SD during the period 1996–2007 (Wydeven et al. 2009). I bracket the 0.85 constant between 0.843 and 0..975 to capture the Minnesota estimate minus the SD from Wisconsin and the Wisconsin average plus its SD. Again, a less conservative approach would use 0.97 and its SEM not the 0.85 value that could substantially inflate the results of the equation and is the least well-substantiated parameter.

What does the above exercise in estimating uncertainty tell us? If we take the margins of error above and apply them to the equation on p.3 in (Erb & Benson 2013), we see the effect of propagating error in a far wider range of possible sizes of the Minnesota wolf population:

Maximum N = ((95098/150)*4.7)/0.843 = 3534 wolves

Minimum N = ((49405/172)*3.9)/0.975 = 1148 wolves

That range of estimates for the Minnesota wolf population size (1148–3534) is a conservative minimum estimate of the true uncertainty in my view, yet admits higher uncertainty than the official estimate of 1662–2640 (Erb & Benson 2013). I am not clear from their report how they claim such certainty. In particular, my lower bound of 1148 is substantially lower than the state's lower range. Either of us could be criticized for preferring our answer over the others answer, but science does not respect authority, only evidence. Given the short time for this peer review and the scarcity of raw data for Minnesota, I do not place great faith in my own calculations or those of Erb & Benson (Erb & Benson 2013). We need a fully transparent, comprehensive review to make a scientific judgment.

The precautionary approach upheld by the U.S. Supreme Court in TVA v Hill 1978 suggests a closer look at the Minnesota wolf population estimate and methods for extrapolating. Indeed, my estimate falls near the lower bound of the federal relisting threshold of 1251 wolves in MN. Given the risk and uncertainty, I recommend all the MN data be presented transparently with basic statistics about standard deviation and with sensitivity analyses applied at every step. There is a possibility that Minnesota contains fewer than 1251 wolves today.

In general, I find the draft biological report ignores a large number of relevant articles published in peer-reviewed journals of the highest rank. Being unaware of them does not seem plausible given the USFWS paid for some of the research in these articles and were sent many of them in previous rounds of delisting. If the draft biological report was written without the need to review past delisting efforts for gray wolves, these sorts of errors will persist, so I recommend a review of the policy for writing draft biological reports as a matter of scientific integrity. Moreover, the articles included appear haphazard. The USFWS shared with peer reviewers an array of non-peer-reviewed sources and lower ranked peer-reviewed journals. From this, I glean the following recommendation on future drafts of biological reports for wolves or other species.

The peer review process ideally provides independent validation of scientific findings. Personal communications, conference proceedings, reports from agencies, journals without editorial policies, etc. do not meet these standards and are therefore less reliable and provide weaker inference no matter how much one likes the conclusions. Of course, the ideal of peer review is sometimes not met even within the peer-reviewed literature. The literature falls short of the ideal when reviewers or editors share the same biases as the authors or when editors or peer reviewers take shortcuts in evaluating the evidence. Therefore, strong inference and progress in science depends on critical evaluation of the evidence itself using accepted standards of evidence and a lengthy process of scientific deliberation and consensus-building. The USFWS has had time for such deliberation and consideration given 20 years of contemplating delisting gray wolves (Refsnider 2009).

Even without consensus among scientists and extensive discussion, one can use long-established standards of evidence to rule out some of the evidence relied on by the draft biological report and proposed rule. For example, controlled comparisons (i.e., those in which a statistical control or experimental control is used) provide stronger inference than anecdote or correlation. Among controlled studies, random-assignment experiments are the gold standard in biomedical research (Ioannidis 2005) as in predator science alike (Treves et al. 2016). Furthermore, within controlled experiments, careful reading of methods can identify systematic bias in sampling, measurement, treatment, or reporting. The USFWS can set an important example for agencies in lower jurisdictions if it restricts itself to drawing conclusions from studies providing the strongest inference, settling for weaker inference when truly nothing else is available. Many examples of such careful discrimination are available in the scientific literature on wolves. Thanks for considering these recommendations for improvement.

End of main text of peer review

Appendix. Reanalysis of wolf population dynamics and monitoring in Wisconsin 1999–2013

Summary statement: The scientific debate about census, monitoring, and the effect of lethal management of wolves in Wisconsin is important to the proposed rule because a foundational claim of the proposed rule was that WGL wolves would be safe after delisting. Ostensibly, WGL wolves would be safe because wolves are being monitored scientifically (when alive and when dead), state management plans are science-based, and therefore that states will swiftly detect if their wolf subpopulations are in jeopardy and respond protectively. The information in this Appendix casts doubt on that assumption. The Appendix below and the following webinar (https://zoom.us/recording/play/unVXR_qjH5QJ0sp3rBbECrNTGR08x1yEmWEocSYSY6SZHihUrQlPQdc-QUk2NrJf?continueMode=true) detail why the state wolf population model and wolf census have been based on incomplete information that requires scientific revision and peer review. The appendix below begins the review and revision including the period 1994–2013 and covers material in the 1999 Wisconsin wolf management plan, its 2007 addendum, and at least half a dozen peer-reviewed papers involving the architects of the state wolf population model and wolf census. The information provided below undermines the scientific rationale for the current state's population goal, state delisting goal, and state harvest models. Although this appendix addresses only Wisconsin, a proper accounting by the USFWS would help to reveal if problems are more widespread.

A. The state wolf management plan of 1999

In 1999, the wildlife agency of the state of Wisconsin (WDNR), published its first wolf management plan (1999 Plan), as the USFWS prepared for delisting wolves in the WGL (Refsnider 2009). I use the federal government's population estimates (USFWS 2018) because they had primary jurisdiction over wolves for most years 1979–2019.The 1999 state wolf management plan included a projection of wolf population growth to 2020 (Figure A1), from the estimated 205 adult and yearling gray wolves counted in winter 1998–1999 (WDNR 1999).

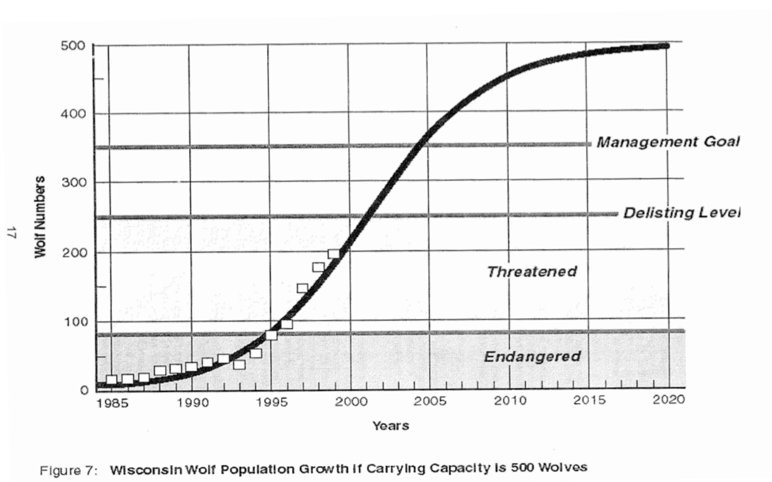

Figure A1. The 1999 Plan's forecasting model of wolf population growth (their Figure 7 (WDNR 1999). The 'Delisting Level' was set at 250, when the legal removal of wolves from the state's list of threatened and endangered species would begin. The 'Management Goal' codified a population target (N_{goal} = 350). N_{goal} is still the state population target today (Stepp 2013). Also note the estimate of carrying capacity (500 wolves) represented one of several estimates in 1999, all of which estimates later proved too low (Appendix).

The 1999 Plan codified two numerical values of population estimates that would alter policy. The first, Delisting Level, was set at 250 (Figure A1) and was apparently set at the midpoint of the range of outputs of a population viability analysis (PVA). The 1999 Plan stated that the PVA, "needs to be cautiously interpreted and should not be used by itself to set management goals… Based on [the Wisconsin PVA], a population between 200 to 300 seemed appropriate for delisting wolves in Wisconsin." p.16, (WDNR 1999). The second value was the Management Goal N_{goal} = 350 (Figure A1) also referred to as the 'population goal'. Ostensibly, N_{goal} was chosen to exceed the USFWS delisting criterion (N = 100), supportable by suitable habitat, compatible with the PVA mentioned above, and "socially tolerated" p.15, (WDNR 1999). Several scientific questions arise from Figure A1 that have not been answered adequately.

The first question relates to the dark logistic growth curve in Figure A1. It appears to be a model fit to the population estimates 1980–1999 and a projection of future growth. Importantly, the 1999 Plan made no mention of that logistic growth curve super-imposed on the population estimates (Figure 1A), or its relationship to an assumption of density-dependent population dynamics and harvest planning (described further below). The assumption of density-dependent population growth was common but far from universal in that a substantial number of wild animal populations did not show such dynamics (Fowler 1987). Apparently, the assumption was made without supporting evidence beyond fitting points to half of such a curve and an estimate of carrying capacity that proved inaccurate (see below). New evidence also suggests density-dependent dynamics are unlikely to characterize Wisconsin wolf population growth from 1980–2012. Subsequently, Brook & Bradshaw (2006) explained many of the reasons why populations like the Wisconsin wolf population might not show density-dependence, including (a) substantial errors or changes in sampling or measurement can mimic or obscure density-dependent dynamics (as we show below); (b) populations growing without spatial bounds and limited mainly by exogenous factors are not expected to show density-dependent dynamics; and (c) minimal changes in density over the sampling period might not produce density-dependent dynamics. Below I review how all of these factors played a role in Wisconsin's wolf population dynamics and the science used to model them.

The second question arose from introducing the criterion of human tolerance (a value judgment). Human tolerance was first introduced by an informal survey about N_{goal} after it had been selected, and the survey was reported without methods in the 1999 Plan. "During the review of the second draft of the [1999 Plan], of persons commenting on the population goal, 38% supported the goal, 38% felt it was too low, and 24% felt it was too high… a reasonable compromise between population capacity, minimum level of viability, and public acceptance." p.16, (WDNR 1999). That small-sample opinion poll probably consisted of individuals in a wolf advisory committee (1999 Plan), not by randomly-sampled, broad public opinion. Later scientific surveys did not support the result, starting in 2001 (Naughton-Treves et al. 2003; Treves et al. 2009a; Treves & Martin 2011). In 2004, the survey with the most representative sample of 1364 respondents sampled randomly from 6 postal areas stratified by region (wolf range or not) and human population density (urban or rural) found that 57% wanted a higher N_{goal} of 500, 1000, or "No cap". N_{goal} itself only garnered 15% support (Naughton-Treves et al. 2003; Treves et al. 2009a; Treves & Martin 2011). Claims to the contrary have been inaccurate or misleading (Groskopf 2014).

The third question that soon arose was about the estimate of carrying capacity in Figure A1. Carrying capacity (K) played an important role in codifying N_{goal}, (p. 15–16, WDNR 1999). Two estimates existed for K (both were exceeded by population growth by 2008–2009): a habitat-based estimate of K = 300–500 wolves or possibly up to 800 if marginal habitat were to be occupied, and a prey-based estimate of K = 262–662 (Mladenoff et al. 1995; Mladenoff et al. 1997). It does not seem a coincidence that the Delisting Level was half of K = 500, and the Management Goal was 53% of K = 662 (Figure A1). Setting population targets at half of K is characteristic of harvest models aiming for maximum sustainable yield (MSY). Such models are designed to kill as many organisms as possible annually, within presumed limits of sustainability. Yet, the 1999 Plan did not explain the model that had been used to set the dark logistic growth curve, the Delisting Level or the Management Goal in Figure A1.

By contrast, the value judgments underlying N_{goal} in the policy side of the 1999 Plan were clearer. The 1999 Plan read, "[N_{goal}] was intended to be the minimum level at which proactive control and public harvest would occur." (p. 16, WDNR 1999). Indeed, the first published draft of the 1999 Plan also included an appendix J, which qualitatively described a future framework for regulation of hunting and trapping wolves. Appendix J was eventually removed from the draft after public opposition (Treves 2008). Yet the science did not change.

Objectivity is a core principle of scientific integrity and therefore important to distinguishing the best available science from biased science (National Academy of Sciences et al. 1992). For the 1999 Plan to have been prepared in a scientifically objective way ("Of a person or his or her judgement: not influenced by personal feelings or opinions in considering and representing facts; impartial, detached." Oxford English Dictionary online 2019), one or more of the following scientific steps should have been taken: presenting alternative hypotheses about population growth (e.g., not density-dependent over the period in question); scrutiny of the underlying assumption of sustainability at N_{goal} or MSY; and exploring alternative scenarios if key parameters were found to differ substantially, e.g., K was exceeded. None of those steps were taken to my knowledge.

B. Changing wolf census methods from 1994–2004

Because wolves are cryptic, territorial, as well as ecologically and socially complex, interpreting wolf population estimates accurately requires a detailed understanding of the census methods used to arrive at population estimates. The source data from unpublished WDNR population reports are no longer easily available to the public, so I present them at http://faculty.nelson.wisc.edu/treves/data_archives/WDNR%20ER%20Bureau%20reports.zip. I use these reports to support the argument that there are four time-series of wolf population estimates in Wisconsin not one time series as has been erroneously depicted (Figure A1 for example). These reports support the following facts:

- From 1979–April 1994, the WDNR deployed a few citizen volunteers to help to census wolves, but in the winter of 1994–1995, the WDNR began a volunteer program for private citizens to track wolves in snow, by driving or on foot, independently of the WDNR (Troxell et al. 2009). In the winter of 1994–1995, the WDNR augmented the number of volunteers from 12 to 40 and those volunteers undertook independent tracking (Wydeven 1994; Wydeven & Megown 1995); see p. 12 and p.13 respectively. The following year, the WDNR deployed 83 volunteers, see p. 16, (Wydeven & Megown 1996). The start date of 1994–1995 when volunteers tracked wolves >526 km has been confirmed several times, most recently in (Wydeven et al. 2009); although a slow start compared with the thousands of km volunteer trackers would later drive (Table 6.1, (Wydeven et al. 2009). Every year thereafter, 50–100 volunteers tracked independently in the snowy months. Census methods changed again approximately 10 years later, but first I examine the effects of the 1994–1995 change.
- The first change in methods strongly affected the population estimate of April 1995, as evidenced by the following: "The 1994-1995 wolf population was 66% above the wolf population present in 1993-1994 (50-57 wolves). This increase probably represents more than just natural reproduction. Some wolves were probably missed in 1993-1994 surveys." p. 10, (Wydeven & Megown 1995).

The above spike and discontinuity in the population estimate can be seen more clearly in Figure A2 than in Figure A1. It represents a break in the first time-series 1980–1994 that created the appearance of a change in shape of the population growth curve. I will argue that this year produced the first (misleading) impression of density-dependent dynamics in this population. My inference is consistent with cautions published later by (Brook & Bradshaw 2006) that sampling effort or measurement methods account for the appearance of a change in slope.

Regrettably, in addition to an illusory pattern of density-dependence in my view, the timing of changes in wolf census methods became confused by an apparent error in the 1999 Plan.

The 1999 Plan stated, "A volunteer carnivore track survey was initiated by the WDNR in fall 1995 [sic] (Wydeven et al. 1996)." p. 20, (WDNR 1999). The reference to fall 1995 is an error that might have come about unintentionally. The 1999 Plan referenced Wydeven et al. 1996 that presented guidelines for those surveys, not the population report by Wydeven & Megown 1995 that presented the first evidence of such a volunteer tracker program (and confirmed in 2009 as summarized above). I presume the one-year discrepancy was an unintentional error in the 1999 Plan, possibly caused by the common confusion that a population report published in the summer of year t+1 was collated from records including the second half of year t, therefore the 1995 population report I cited above for a 66% increase in the number of wolves included late 1994 and is an official government population report in a format of a scanned image which is non-editable (Wydeven & Megown 1995); it precedes the 1996 guidelines on volunteer tracking cited in the 1999 Plan.

Confusion about timing was not restricted to the 1994–1995 change in census methods. For example, the following sentence appeared first in the winter 2003–2004 report, "All volunteers were required to attend weekend wolf ecology courses and day-long track training programs."

p. 5, (Wydeven et al. 2004b). Such training sessions had been implemented by the second half of 1996 and were a regular part of the program every year thereafter.

In all, I found four changes in census methods had been implemented piecemeal: addition of volunteers 1994–1995, training in the second half of 1996, some types of quality control on volunteer tracker data between summer 2000 and winter 2003–2004, and an overhaul of methods and volunteer turn-over in 2012–2013 described in section E below.

Changes in census methods affected the accuracy of wolf census. Importantly, wolf census by volunteer trackers improved the population estimate and routinely differed from those by WDNR staff members surveying the same census blocks at different times. The following quotations substantiate both assertions. The 2003 conference presentation entitled "Counting wolves--integrating data from volunteers", asserted that volunteer trackers improved the population census:

> "Wolves recolonized the state of Wisconsin in the mid-1970s after being extirpated for about 15 years. Between 1979 and 2002, the Wisconsin Department of Natural Resources (WDNR) maintained constant monitoring of the state wolf population through live-trapping and radio tracking, winter track surveys, summer wolf howls, and public reports. The wolf population in Wisconsin ranged from a low of 15 (1985) to 327+ in 2002 and ranged from 4 to 83 packs. Mean estimated pup survival from late gestation to about 9 to 11 months was 0.30 (range 0.16 to 0.57). During periods of disease outbreak of Parvovirus (mid 1980's), and initial outbreak of Sarcoptic mange (early 1990s), pup survival declined to < 3.0 during periods of severe disease outbreak. A mean of 36% (SD + 16.4%) of packs had no surviving pups in late winter. Survival of older wolves (1+yr) was 0.61 during the early 1980s when wolves declined but increased to 0.82 in the late 1980s and early 1990s and has remained high in recent years. Since 1985 the wolf population increased an average of 20% annually and experienced a slight decline during only one year. Areas occupied by wolf packs expanded from about 1500km 2 in the early 1980s to about 13,000km 2 in 2002. The wolf population continues to increase, but growth may decline or stabilize soon as most suitable habitat is occupied, and more liberal lethal controls are enacted." abstract, (Wiedenhoeft et al. 2003).

I do not have the full transcript of that presentation, so I do not know the period of censuses it covered. But the differences between volunteer trackers and WDNR census-takers began to be quantified in population reports by the winter of 2003–2004:

> "Forty-nine blocks were surveyed by both [W]DNR and volunteer trackers. [W]DNR detected 159–180 wolves, and volunteer trackers detected 130-143 wolves in these blocks. [W]DNR detected more wolves in 17 blocks, less [sic] wolves in 17 blocks, and the same in 15 blocks. In 19 of the 34 blocks where counts differed, the group with the higher count had tracked considerably more miles than the group with the lower count. In 5 of the 15 blocks where the counts were the same, both detected 0 wolves." p.9, (Wydeven et al. 2004b).

By contrast, in 2006, the state reported that volunteer trackers found more wolves in the same censes blocks counted by WDNR staff:

> "Both DNR and volunteer trackers surveyed wolves in 41 survey blocks. Overall rates of wolf detection were similar with 147 - 159 wolves detected by DNR trackers and 159- 173 wolves detected by volunteers. DNR detected more wolves in 13 blocks, volunteers detected more wolves in 19 blocks, counts were the same in 7 blocks, and no wolves were detected in 2 blocks. Overall rates of wolf detection indicate volunteers are providing suitable counts of wolves." (p.9, (Wydeven et al. 2006)

The differences between volunteer tracker and WDNR counts varied annually and the differences seemed to relate to search effort. For example, in winter 2003–2004, 55% of census blocks showed different wolf counts by volunteer trackers and WDNR staff, when calculated by the number of census blocks that differed divided by the total number of census blocks that both groups counted; or alternately, the wolf count summed across census blocks that both groups had counted differed -18% to -21% when calculated by the number of wolves counted by volunteer trackers divided by the number that WDNR staff counted in the same census blocks (Wydeven et al. 2004b). By contrast, the winter 2005–2006 report allowed calculations as above of 78% and +8% to +9% respectively (Wydeven et al. 2006). Although the reports from 2004 to 2012 suggest WDNR staff more often counted more wolves than volunteers counted in the same census blocks, the differences were clearly large and variable to the point where they sometimes exceeded the annual average for population growth (see below).

The unpublished population model and its revealing logistic growth curve (Figure A1) in the 1999 Plan might have been materially influenced by the addition of independent volunteer trackers in winter 1994–1995 and beyond. I expect the model would have changed because the average and variance of population estimates changed. However, the architects of the state wolf population model[1] did not transparently describe the above changes or its implications for the 1999 Plan or underlying, unpublished model.

I present a revised figure for Wisconsin's wolf population growth that reflects three time-series rather than one, as I can best recreate the appropriate start and end dates from the above information for 1980–2012. But I retain the official population estimates because providing alternative estimates is beyond my present scope (Figure A2).

[1] In the 2004 population report, the architect of the wolf census introduced the public to the architect of the wolf population model as WDNR staff, until October 2005–March 2006 when the University of Wisconsin–Madison employed the latter. "Between October 2003 and March 2004,… agency personnel were asked to report wolf observations to Tim Van Deelen with DNR Science Bureau." p.8, (Wydeven et al. 2004b) and then, "New research by graduate student Elizabeth (Lizzy) Berkley and Dr. Tim Van Deelen will be conducted, examining fatty acids to determine diet of wolves" p.11, (Wydeven et al. 2006).

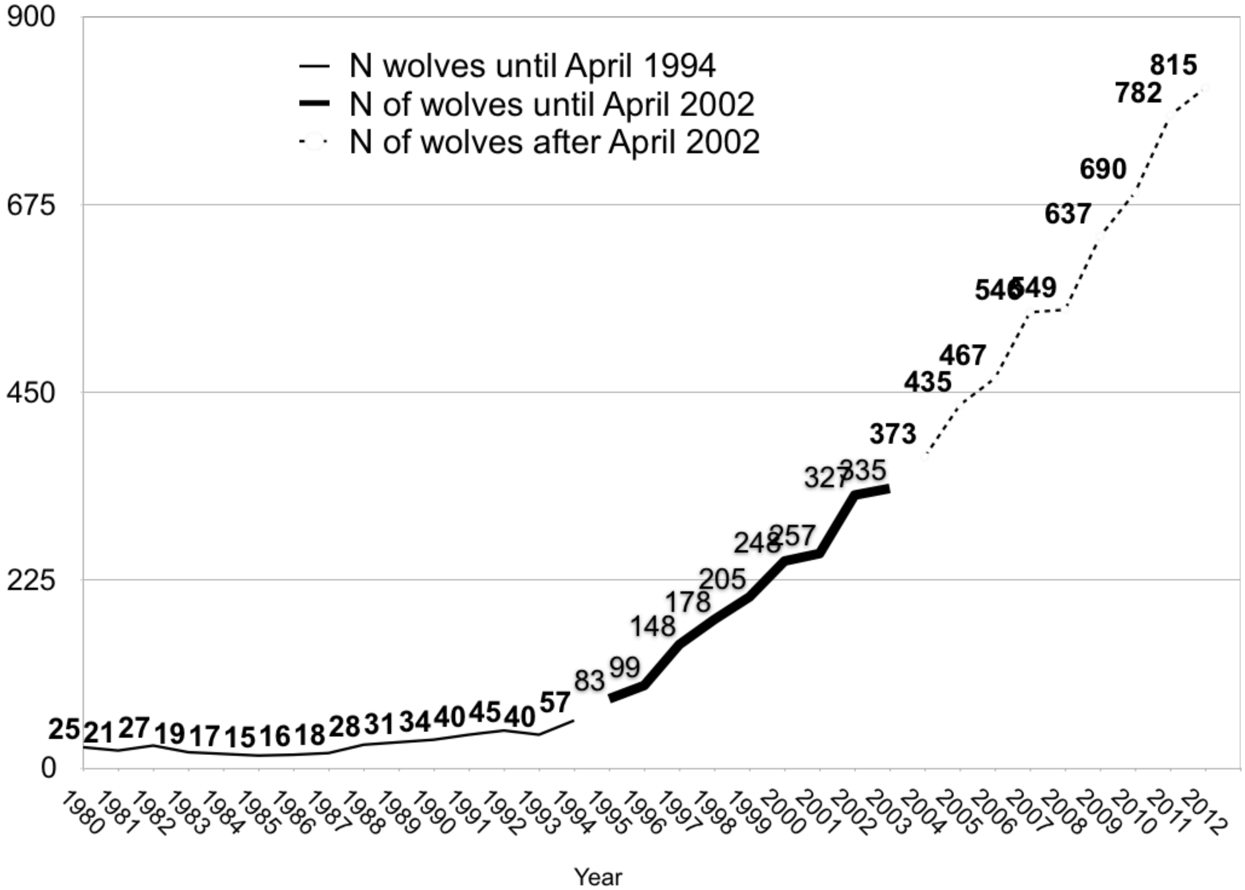

Figure A2. Wisconsin's wolf population estimates in three time-series, 1980–1994, 1995–2003, 2004–2012. The census population size of adults and yearlings in late winter depicted in three consecutive time series: the first (thin, solid line) before volunteer trackers counted wolves independently for the WDNR; the second (thick solid line) marks the major increase in deployment of independent volunteer trackers, until mid-2003 when the WDNR announced the quality controls over volunteer tracker data and search effort. The transition from the second period to the third period might have occurred as early as winter 2000–2001 or piecemeal during the interval 2000–2003. A fourth time-series also seems to have started after winter 2012–2013 (Appendix section E).

When I analyzed the population estimates in three consecutive time series (Figure A2), I found significant differences in annual growth rate averages or standard deviations between time series. Using $(N_{t+1} - N_t) / N_t$), the annual average increase in the first period was 8% (sd 23%), then from April 1995 to April 2004, it was 22% (sd 15%, two-tailed test with unequal variances $X^2=26.9$, p=0.025). Instead, if one ends the second period after winter 1999–2000 (Wiedenhoeft et al. 2003), the average was even higher (29%, sd 15%). In the third period 2005-2012, growth averaged 10% (sd 6%, unequal variances $X^2=62.9$, p=0.001). Even if one demarcates the third period after April 2001, the average growth fell by >50% to 11%. The higher sd in period two than period three suggests that volunteers and WDNR staff might have compensated for undercounting in year *t* with greater effort or redirected effort in year *t+1*. Also the first two winters of volunteer tracking (1994–1996) seem to account for the high average growth rate in time series 2 and the appearance of a slow-down by time series three. The latter appearance of a slow-down might have created the illusion of density-dependent dynamics.

Also, one should consider that in April 2003 the WDNR began issuing permits for lethal control of wolves, adding an exogenous limiting factor, which is not evidence for an endogenous density-dependent factor limiting wolf population growth. Regardless of how one interprets the population estimates and their changes over time, scientific integrity demands disclosure of the changes in methods (National Academy of Sciences et al. 1992) associated with changes in population estimates and average and standard deviations of growth rates.

I conclude that a significant methodological artifact permeates the population estimates for Wisconsin's wolves from 1980–2012. Moreover, the initiation of lethal management of wolves on 1 April 2003 – after quality control was imposed on volunteer trackers' data between 2000 and 2003 – and its cessation or re-initiation in five more periods until 15 April 2012 (Chapron & Treves 2016a), would complicate any simple inference that population growth showed density-dependence.

Because the effect of the volunteers described above was only detected after the 1999 Plan and perhaps only fully grasped in mid-2004, the ensuing years would have been critical for remedying the problems with the unpublished population model, improving the descriptions of methods, and revealing the potential effects of those changes to the public. After 2004, an opportunity arose to improve transparency of wolf population science; explicitly and objectively discuss assumptions, scenarios, and alternative hypotheses; subject census methods and population models to independent peer review; and encourage replication. Little of this was attempted to my knowledge. In sum, the science in the 1999 Plan had gaps in both transparency and objectivity. Those gaps could have been easily remedied scientifically between 1999–2006, but they were not when the WDNR published an addendum (WDNR 2007).

C. Addendum to the 1999 Plan

Collaborations between the architects of the wolf census and population model soon led to the second wolf population model. However, again the WDNR and the architects themselves did not publish their work in a way that could be reviewed by peers or the public. In the 2007 addendum to the 1999 Plan, the architects of the state wolf census and population model wrote,

> "Van Deelen (unpublished) fit simple growth models to a XX [sic] year time series of wolf population estimates. Models fit were the discrete logistic model (CITATION) [sic] and the discrete Ricker model (1975) of the general form Nt+1 = f(Nt) where N = population size. Model fitting was based on a least squares algorithm and jackknife procedures were used to generate variance estimates because of the inherent temporal autocorrelation (Dennis and Taper 1994). The best fit logistic model estimated an equilibrium (or carrying capacity) of 505 (95% C.I. = 501 - 518, P <0.0001, R^2 = 0.99) whereas the best fit Ricker model estimated an equilibrium of **522 (95% C.I. = 295 - 635, P <0.0001 0.** [sic] R^2 = 0.99). Model selection criteria (Burnham and Anderson 1998) suggested that

> these 2 models were nearly equivalent given the data. Nonetheless, a Ricker model is probably more useful because of less restrictive assumptions about the shape of the growth curve. Despite wide use to characterize the growth in a time series of population growth estimates (Lotts et al. 2004) this model fitting approach has recently been criticized in favor of a risk analysis (Population Viability Analysis) that can be generated from the same data (Lotts et al. 2004). Still this exercise demonstrates that the original estimates of 300-800 wolves (depending on the extent to which marginal habitat was used) were reasonable and probably quite accurate." emphasis added, p.7, (WDNR 2007).

The long quotation above serves several purposes in this context. First, the 2007 addendum never mentioned the substantial differences between volunteer trackers and WDNR staff, nor the significant differences in averages or standard deviations of wolf counts in the different census periods I identified in Figure A2. Second, the time series used was omitted as was the citation to the method of fitting a discrete logistic model (see [sic] above), which perpetuated the 1999 Plan's lack of transparency.

I surmise the writing was rushed to meet the publication date: "The addendum to the wolf plan was presented and approved by the Natural Resources Board at their meeting on June 28, 2006 and updated on August 15, 2007." p.1, (WDNR 2007). The date is important because the architects reaffirmed their model outputs estimating K at 522 with confidence intervals of 295–635 (boldface phrase above) when the population had been estimated at 546 wolves in April 2007 (Figure A2) – yet also reaffirming estimates of K at 500–800 wolves. It is unclear why the models described in the quotation above were trusted when the predicted mean values were already surpassed. The architects seemed themselves to grant credence to the habitat suitability models that provided an upper bound of 800 wolves originally credited in the 1999 Plan.

Further details of wolf census and population model only surfaced in an incomplete fashion in 2009. In 2009, the architects co-edited a book on wolf populations in Wisconsin and surrounding states. The relevant chapters were entitled, "History, population growth, and management of wolves in Wisconsin" (Wydeven et al. 2009), and "Growth Rate and Equilibrium Size of a Recolonizing Wolf Population in the Southern Lake Superior Region" (Van Deelen 2009). Neither chapter cites their own findings of differences between volunteers and WDNR staff – p. 91-94 and Table 6.1 in (Wydeven et al. 2009).

The chapter on population growth and modeling presented only one scenario for killing wolves: "For example, given a growth rate of 1.31… an additive maximum sustained yield of 92 wolves (7%) would maintain the [Southern Lake Superior] population at 60% of estimated carrying capacity (770 wolves)." p. 150, (Van Deelen 2009). That author did not specify the model fully nor mention the changes in census methods, the apparent spike in population estimate in April 1995, why summing Michigan and Wisconsin's wolf populations tends to obscure the effects of volunteer trackers, or even the existence of volunteer trackers as shown by the following quotation:

> "These counts were supplemented by howling surveys and winter track surveys." p. 139, (Van Deelen 2009). "Biologists began radio-collaring adult wolves in

> Wisconsin in 1979 (Wydeven et al. 1995) and in Michigan in 1992 (Potvin et al. 2005) to facilitate the identity [sic] and location of wolf packs for aerial counts. **These counts were supplemented by howling surveys and winter track surveys**, but radio tracking has remained a centerpiece of population monitoring (Wydeven et al. 1995). Taken together, these efforts have provided rigorous annual counts of wolves." (p. 139, (Van Deelen 2009).

D. Independent science and an unresolved debate about Wisconsin's wolf population dynamics

Chapron and Treves independently published an alternative model starting with wolf population estimates from 1995 to account for the changes produced by volunteer trackers (Chapron & Treves 2016a, b, 2017b). They could not replicate the findings of the architects and their junior colleagues about population dynamics, but instead concluded that negative density-dependence on population growth was not apparent.

A lack of density-dependence in the period 1980–2012 need not be surprising. the fundamental assumption of density-dependence of Wisconsin's wolf population growth 1980–2013 has not yet been supported transparently or reproducibly. No density-dependence on adult mortality has been reported in any peer-reviewed report. Density-dependence on juvenile recruitment to adulthood is currently contested and vulnerable to problems of statistical dependence. Lack of a density-dependent change in life history parameters need not be surprising for wolves. For example, wolves in Western Poland grew substantially with no sign of density dependence (Nowak & Mysłajek 2016). Wolves may benefit from helpers (hence a higher local density) to raise a litter of pups, and for defending a territory against neighbors. Moreover, the very slight increase in density over time – 0.9% annually from 2000–2011 (Chapron & Treves 2017b), or even the 3% annually from 1995–2007 claimed by (Wydeven et al. 2009) – might be artefacts of measurement uncertainties about the geographic area occupied. Reproduction or mortality need not necessarily change with increasing population size or density. After all, population size may rise by geographic expansion without changing densities in the core areas. Or increasing densities within a pack territory might lead to more successful reproduction (positive density-dependence) if supernumerary adults help breeders reproduce and resources are not scarce at critical periods (Moehlman 1989).

Chapron & Treves (2016a, b) analyzed the time series of wolf population dynamics from 1995–2012 as a single time series, aware of the first change in census methods but unaware of the second (Figure A2). The architects and their junior colleagues responded, "[Chapron & Treves] selectively chose to analyse a subset of wolf population and life-history data (1995 –2012), yet these datasets extend to 1980 and 1989 for Wisconsin and Michigan, respectively (Beyer et al. 2009; Wydeven et al. 2009). Inclusion of the full range of wolf population and life-history data would probably have produced contrary results…" p.1–2, (Olson et al. 2017). Chapron & Treves rebutted, "[Olson et al.] insinuate that we chose to start our analysis in 1995 because it somehow supported our hypothesis. Our choice is justified by two of [Olson et al.] co-authors writing how monitoring substantially improved after 1995 [Wydeven et al. 2009]. The papers they cite [Stenglein et al. 2015, Stenglein and van Deelen 2016, van Deelen 2009] that begin analyses earlier do not seem to account for that change in census methods, which may affect their results." p. 2, (Chapron & Treves 2017b).

The preceding debate in 2017 reflected controversy about a series of papers published by the architects and their junior colleagues, which omitted the changes in census methods (sections B and C above). The first peer-reviewed population model introduced misleading information and apparent errors, when it stated,

> "The population grew slowly from 1980 to 1995 at which point the winter count surpassed **the endangered status of 80 wolves [sic, a]** (Wydeven et al. 2009). Since 1995, the wolf population increased dramatically, and management policy **changed with respect to the degree to which managers may kill wolves to address depredation problems [sic, b]**. Hence, policy changes and population growth interacted to define three recovery periods… **During 1996–2002, wolves were listed as endangered under the US Endangered Species Act [sic, c]** and protected from all hunting and trapping. In 2003, wolves were downgraded to threatened status and lethal control actions [followed]…. The period 2003–2012 was dominated by this on-again and off-again lethal control management..." (internal citations relating to lethal control omitted, emphasis added, p. 371, (Stenglein et al. 2015c).

I perceive errors or misleading text where I inserted **[sic a–c]** above: **(sic a)** Reclassification is a legal designation not a biological one and no change in state or federal policy was made before 2003 as summarized in this Appendix (nor was the claim of policy change substantiated (Stenglein et al. 2015c). Moreover, the 1995 spike in the population estimate that was associated with a change in census methods was detected in the winter of 1994-1995 before the observation that wolves had exceeded 80 individuals in April 1995. Therefore, any ostensible change in state or federal management policies (for which there is no record) follows the change in methods, not the other way around; **(sic b)** The history of lethal management policy is not accurately presented in the quotation above. Authority for killing wolves was not granted to the state of Wisconsin until 1 April 2003 (Chapron & Treves 2016a); **(sic c)** Similarly, wolves were federally listed as endangered since the late 1970s, so identifying a break in policy relating to lethal control in 1995 or 1996 appears misleading.

The errors or inaccuracies noted above were not insignificant given the population modeling used the three recovery periods as parameters, "…we fit a model with three correction factors that were constant within each recovery period (1980–1995, 1996–2002, and 2003–2011)." p.372, (Stenglein et al. 2015c). The periods they chose replicate the first errors in timing, which I detailed above. The first period should end April 1994 before volunteer trackers data led to a methodological artifact. The second period should end sometime between summer 2000 and winter 2003–2004 as I detailed above, when the WDNR first analyzed the substantial differences between volunteer trackers and WDNR staff and undertook changes to control the quality of data. Moreover, lethal methods were only permitted on 1 April 2003, which means 95.9% of that wolf-year (15 April 2002–14 April 2003) should be assigned to the prior policy period without lethal management (Chapron & Treves 2016a). Furthermore, the latter authors showed that lethal management changed 5 more times in the period under question, so I cannot understand why Stenglein et al. (2015c) did not define more policy periods. In sum, I perceive their policy periods obscured the real changes in methods and policy for killing wolves.

The problems with Stenglein et al. (2015c) did not end with their designation of illusory policy periods. Although I agree that adult survival did not seem to vary with density from 1995–2012 (Stenglein et al. 2015c; Chapron & Treves 2016a), I do not agree with their claim of negative density-dependence on recruitment (Stenglein et al. 2015c). Chapron & Treves (2017b) found problems with that claim, which seem to require reanalysis. Chapron and Treves (2017b) could not find Stenglein et al.'s (2015c) quantitative estimates for how recruitment changed from 1995–2012. Instead, the conclusion seems to be based on low-resolution, line graphics in Figures. Moreover, Stenglein et al. (2015c) found weak effects as shown here, "The evidence for a negative slope of the line for t > 18 was 69.0% (proportion of posterior that was <0)" (p. 372, Stenglein et al. 2015c). (Note that t > 18 refers to their policy periods found to be erroneous above.) and here, "48.4% of the time, the estimated population sizes in Wisconsin from 1981 to 2011 were within the 95% posterior intervals of μ_t" (p. 372, Stenglein et al. 2015c), implying that more than "half the time the estimates failed this relatively undemanding test" (p. 1, Chapron & Treves 2017b).

The new problem I report here is that the raw data on reproduction were not based on the few, rare, direct observation of pups around dens or pup survival to late fall, as done in other studies (Fuller 1989; Fuller et al. 2003). Rather, the estimates of recruitment relied on (a) indirect estimates for a minority of packs via summer howl surveys, which were recently shown to be widely variable between observers (Palacios et al. 2017); and (b) retrospective inference about pup production in a majority of packs, based on censuses taken 6–9 months later (Wydeven et al. 2004a; Wydeven et al. 2009). In the former 2004 article, we wrote, "The pup count… [is] based on a combination of direct and indirect evidence collected in both the summer and winter. As a result. pup count is statistically related to total pack size because DNR biologists estimated past pup production from current- and previous-year counts of adults and yearlings." p.35, (Wydeven et al. 2004a). In short, population growth necessarily reflected the observation of more wolves, which led to a circular inference that reproduction was responsible for packs that had grown in size. Migrants, and individuals undertaking long-distance movements, were not uncommon in this population (Treves et al. 2009b), which can lead to changes in pack size when census is based on brief encounters every few weeks (e.g., by volunteer trackers conducting snow surveys or pilots counting packs from the air). By contrast, Chapron & Treves (Chapron & Treves 2016a) treated reproduction as a binary variable (reproduction or no reproduction), which is more robust to uncertainty about the number of pups (Palacios et al. 2017), compared to a continuous measure of pup survival. Therefore, the evidence for density-dependence on reproduction seems so weak as to require new data not skimpy reanalysis.

In 2016, Stenglein and Van Deelen attributed population dynamics to biological mechanisms explicitly, although they addressed (and dismissed) methodological artifacts,

> "We did not find reduced fecundity in pups per pack or in the proportion of breeding females in the population pre-1995 compared to 1995–2007 (Stenglein unpublished). However, the proportion of lone wolves prior to 1995 (roughly 10% of the population) was higher compared to 1995–2007 when only 4% were

> lone wolves (Wydeven et al. 2009). **The difference in proportion of lone wolves could be due to sampling and detection issues;** however a real difference provides support for a mate-finding component Allee effect in early recovery because it suggests that wolves had difficulty finding mates at low densities, resulting in more lone wolves… When the wolf population was small, it may have been more difficult to count wolves, packs and occupied territory. A**t small population sizes, failing to count just one pack and then finding and counting it in the next year could lead to the appearance of substantial population growth which would be due to observer error rather than real growth**." (emphasis added, p. 11-12, (Stenglein & Van Deelen 2016).

All the most recent peer-reviewed articles of which I am aware ignored the debate about census methods entirely and continued to attribute biological causes to the methodological artifact that Treves & Chapron (2016a, 2017b) had made them aware.

The most recent by Stenglein, Wydeven and Van Deelen (2018) described their methods as follows, "The dataset consisted of > 42,000 weekly locations of 501 wolves captured and tracked (November 1979–December 2013)… [documenting] (2) known collar failure or lost to follow-up during the study and then found dead sometime after its endpoint (known censoring)." p.102, (Stenglein et al. 2018). Finding dead wolves whose radio-collars had stopped transmitting or were otherwise 'lost to follow up' requires observers on the ground, yet the methods and changes in those methods over time for volunteer tracking were not presented. See Box 1 main text for the large number of wolves lost to follow up. The same criticisms apply to (Olson et al. 2015; Olson et al. 2017). And because the latter article followed the debate with the architects and close collaborators (Olson et al. 2015; Olson et al. 2017) and similar debates about detecting dead wolves (Treves et al. 2017a; Treves et al. 2017c), the latest publication in 2018 seems to perpetuate misleading methods and analyses (Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018)(Stenglein et al. 2018).

In sum, articles from 2015–2018 modeled wolf population dynamics, birth, or deaths with essential methods having been omitted (Stenglein 2014; Olson et al. 2015; Stenglein et al. 2015a; Stenglein et al. 2015b; Stenglein & Van Deelen 2016; Olson et al. 2017; Stenglein et al. 2018). When specifying methods, they cited only these articles (Wydeven et al. 1995; Van Deelen 2009; Wydeven et al. 2009), which treated the wolf population estimates as a single time series (erroneously in my view), treated the time series as an outcome of density-dependent dynamics, contrary to warnings from scientific reviews of the topic (Brook & Bradshaw 2006), and treated the time series as the statistically independent from estimates of reproduction and mortality despite their own acknowledgments of the shortcomings of monitoring methods. Therefore, I do not consider the work of the architects of the state wolf census and population model to be accurate reflections of the Wisconsin wolf population status or dynamics, nor of the effects of lethal management on illegal killing or population dynamics (Box 1).

E. After delisting in 2012

WDNR plans to allow extraction of wolves in a regulated season with hunting, trapping, and hounding went into effect on 15 October 2012 (Natural Resources Board 2012; Treves et al. 2017b). The next year, the Secretary of the WDNR reaffirmed N_{goal} and the pivotal role of one of the state architects and their junior colleagues,

> "…several scientists, including Timothy Van Deelen and his research associate Jennifer Stenglein, have presented to the [WDNR] wolf committee on multiple occasions.[The newspaper article] suggests that because Van Deelen isn't a member of the committee that his inputs are not considered. To the contrary, their model has been utilized for two consecutive years to help model and project how the state's wolf population will respond to harvest.… Next the assertion that we will get to our goal of 350 wolves in one year ignores the science. The UW's own population model indicates this year's harvest could result in a 13 percent reduction in the state's wolf population.… we have been clear that we will honor the established population goal…." no pagination in original, (Stepp 2013).

The above quote supports my contention that the 1999 Plan and 2007 addendum continued to model wolf populations with an implicit assumption about public hunting of wolves. However, state implementation and regulations for killing wolves did not match the model assumptions as far as I can tell, for the following reasons.

Harvest models based on maximum sustainable yields assume that 'surplus' individuals are 'harvested' or killed such that future reproduction is unimpeded. Wisconsin's eventual wolf hunting regulations made no distinction between killing breeders, supernumerary adult helpers, or young (2012 Wisconsin Act 169) and (Natural Resources Board 2012, 2013, 2014). Evidence suggests those regulations would not protect the reproductive capacity of the wolf population and left it to stochasticity. The total number of breeding females legally killed in Wisconsin in 2012 has not been published. But neighboring Minnesota held a similar public hunting season in 2012 and presented the following estimates: 51% of wolves killed were females, of which 22% appeared to have once been breeders (Figure 9b, Stark & Erb 2013). Without considering the effect of deaths of breeding males on pack reproduction (Brainerd et al. 2008; Borg et al. 2015), and assuming one breeding female per pack, I use the Minnesota estimate to predict wolf reproductive potential diminished by 22% in the late winter after Wisconsin legalized public hunting and trapping for wolves. Diminished reproductive potential following loss of breeders can persist for more than one breeding season (Brainerd et al. 2008; Borg et al. 2015). Also, the 22% estimate from Minnesota did not consider hounding that was not allowed in Minnesota. One might also expect the 22% might be additive to other causes of breeder loss. Without better data on reproduction, the effects of Wisconsin's wolf-hunting seasons on population size and growth are uncertain.

At no time did the scientists assisting the state explain to the public whether or how the risk of population crash had changed (Natural Resources Board 2012, 2013, 2014).

I anticipate one rebuttal that Wisconsin's wolf population is thriving at an estimated census of 866 in April 2016 (USFWS 2018). But questions remain about the censuses from 2013–2016, especially after the change in census methods after winter 2012–2013. In July 2012, 28 veteran, volunteer census-takers opposed wolf policy publicly and many later resigned (Ericksen-Pilch et al. 2012); they represented approximately 42% of the previous year's number of volunteers (Wydeven et al. 2012). In response to a need for wolf monitors, the state seems to have relaxed the criteria in period 3 (dashed line in Figure A2) for effort, quality, and training. Then, the state reduced the former transparency of the tally of the wolf census, "That the DNR sort of has to come up with a count **in less open system** because giving the exact location of every wolf and every pack is occurring in the state is no longer appropriate when they're a hunted species." (emphasis added, transcribed from radio interview at 38:08 min:sec, Wydeven 2016). I do not understand the logic behind that statement given that poaching has long been the major cause of wolf mortality when the more transparent census was conducted (Treves et al. 2017a; Treves et al. 2017c) and the state revealed all wolf pack locations after 2012, with high spatial resolution (Wisconsin Department of Natural Resources 2018). The census policy and reporting would seem to obscure the wolf population estimate while making wolf packs easier not harder to locate.

F. Conclusion of Appendix

The original scientific justifications for Wisconsin's wolf population model (K, density-dependence, a single time series for census estimates) have been discredited or questioned with equivalent or stronger evidence. To uphold scientific integrity and identify the best available scientific and commercial data, we need an open and thorough peer-reviewed test between alternative hypotheses for the pattern of wolf population growth in Wisconsin. Without that, the state's wolf policy should not be considered the best available science.

Gaps in scientific integrity relating to objectivity (section A), transparency (Sections A-C), and replicability of results (Sections C and D) are all considered problematic by the National Academies of Science (National Academy of Sciences et al. 1992). Currently, the primary defense – against gaps in scientific integrity and wore transgressions of scientific ethics by a government agency – is academic freedom fortified by tenure or other protections for the independence of scientists. The U.S. Fish & Wildlife Service can and should contribute to finding and promoting the best available science by holding lower jurisdictions to higher standards of scientific integrity and academic freedom.

End of Appendix

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Text of the peer review ends here

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Clarification Questions and Responses

May 24, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 4 – Dr. Adrian Treves

GRAY WOLF 2019 PROPOSED RULE: PEER REVIEW

Clarification Questions

Peer Reviewer #4

- On the 8th page of the review, the peer reviewer provides information from an unpublished, in review, study by F.J. Santiago-Avila and references this data multiple times in the review. Please provide a copy of this document so that we may further consider this information.

Reviewer’s response was incorporated into an updated review. Please see above.

Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

May 24, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 5 – Dr. Daniel R. MacNulty

Peer Review

U.S. Fish and Wildlife Service
Removing the Gray Wolf (*Canis lupus*)
From the List of Endangered and Threatened Species

Proposed Delisting Rule & Draft Gray Wolf Biological Report

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Introduction

I was asked to perform an independent scientific review of the U.S. Fish and Wildlife Service's ('Service') Proposed Delisting Rule (84 FR 9648, March 15, 2019) and accompanying Draft Biological Report (USFWS 2018), with special emphasis on addressing a series of key objectives and questions listed below. I was free to comment on any aspects of the Proposed Rule and Draft Biological Report. The statement of work included the following key objectives:

1. Assess the logic the Service's assumptions, arguments, and conclusions.

2. Assess how well the Service's conclusions are supported by the data and analyses.

3. Determine whether the Service used the best available information.

4. Assess the quality of the Service's scientific information.

Key questions about the Proposed Rule:

1. Does the proposed rule provide an adequate review and analysis of the factors relating to the persistence of the gray wolf population currently listed under the ESA in the contiguous 48-States (human-caused mortality, habitat and prey availability, disease and predation, and effects of climate change)?

2. Have we (the Service) adequately considered the impacts of range reduction (i.e., lost historical range) on the long-term viability of the gray wolf in its remaining range in the lower-48 states (outside of the northern Rocky Mountains)? If not, what information is missing and how is it relevant?

3. Is it reasonable for the Service to conclude that the approach of Michigan, Wisconsin, and Minnesota to wolf management, as described in their Plans and the proposed rule and in the context of wolf management in the Western Great Lakes area, are likely to maintain a viable wolf population in the Western Great Lakes area into the future?

4. Please identify any oversights or omissions of data or information, and their relevance to the assessment. Are there other sources of information or studies that were not included that are relevant to the proposed rule? If so, what are they and how are they relevant?

5. Are there demonstrable errors of fact or interpretation? Have the authors of the proposed delisting rule provided reasonable and scientifically sound interpretations and syntheses from the scientific information presented in the draft biological report and the proposed rule? Are there instances in the proposed rule where a different but equally reasonable and sound interpretation might be reached that differs from that

provided by the Service? If any instances are found where this is the case, please provide the specifics regarding those particular concerns.

Key questions about the Draft Biological Report:

1. Does the draft report provide an adequate and concise overview of Gray wolf (*Canis lupus*) taxonomy, biology, and ecology? The changes in the biological status (range, distribution, abundance) of the gray wolf in the contiguous 48 United States over the last several decades?

2. Please identify any oversights or omissions of data or information, and their relevance to the report. Are there other sources of information or studies that were not included that are relevant to the biological report? What are they and how are they relevant?

I focus my review on four overarching questions that address the key objectives/questions and that are central to the scientific validity of Proposed Rule and Draft Biological Rule:

1. Is the "the gray wolf entity" logical?

2. Are western listed wolves discrete from eastern wolves?

3. What is "current range"?

4. Is the review and analysis of human-caused mortality adequate?

Finally, I provide specific responses to the key questions. This includes references to my responses to the overarching questions as well as additional comments and information.

1. Is the "the gray wolf entity" logical?

The Proposed Rule considers the status of two currently listed entities: (1) *C. lupus* in Minnesota and (2) *C. lupus* in the lower 48 United States and Mexico outside of Minnesota, the northern Rocky Mountains distinct population segment (NRM DPS; Montana, Idaho, Wyoming, eastern third of Washington and Oregon, north-central Utah), and the area covered by the experimental areas for *C. lupus baileyi*. Entity (1) is listed as threatened, and entity (2) is listed as endangered. Entity (2) includes wolves in Michigan, Wisconsin, western Washington, western Oregon, and northern California. For the purposes of this review, I refer to wolves in western Washington, western Oregon, and northern California as "western listed" wolves.

The Proposed Rule considers "the conservation status of the two listed wolf entities as one combined entity" which it terms the "the gray wolf entity" (84 FR 9648, p. 9653). The details of this combined consideration are the basis for the U.S. Fish and Wildlife Service's (Service) conclusion that "the combined entities no longer meet the [Endangered Species] Act's definition of 'threatened species' or 'endangered species' (84 FR 9648, p. 9686).

The logic of "the gray wolf entity" is problematic because it is based on three assertions that cannot all be true at the same time. The first assertion is that the two listed wolf entities "are not discrete from one another under our current policy on vertebrate distinct population segments (DPSs)" (84 FR 9648, p. 9653). This implies that wolves in Minnesota are not discrete from wolves in western Washington, western Oregon, and northern California. According to the Proposed Rule, which quotes from the Service's 1996 Distinct Population Segment policy, a "discrete population" is one that is "markedly separated from other populations of the same taxon as a consequence of physical, physiological, ecological, or behavioral factors (61 FR 4725)" (84 FR 9648, p. 9653).

The second assertion is that "wolves currently listed in the western United States are not discrete from the recovered Northern Rocky Mountain (NRM) population, which we removed from the [Endangered Species] List in 2009." (84 FR 9648, p. 9653). In other words, "wolves in western Washington, western Oregon, and northern California…are not discrete from the NRM DPS" (84 FR 9648, p. 9653). The third assertion is that NRM wolves represent a distinct population segment (84 FR 9648, p. 9651 & 9653), which implies that NRM wolves are discrete from Minnesota wolves (Entity 1).

It is not logical for western listed wolves to be "not discrete from" NRM wolves AND Minnesota wolves if NRM wolves are distinct/discrete from Minnesota wolves. If the latter is true, western listed wolves are either discrete from NRM wolves OR Minnesota wolves. Information in the Proposed Rule and Draft Gray Wolf Biological Report (USFWS 2018) indicate that western listed wolves are biologically more similar to NRM wolves than they are to Minnesota wolves. The following section considers this issue in more detail.

2. Are western listed wolves discrete from eastern wolves?

The Service's determination in the Proposed Rule that western listed wolves are not discrete from wolves in Minnesota, Wisconsin, and Michigan is not consistent with scientific information in the Proposed Rule and Draft Gray Wolf Biological Report (USFWS 2018). The Proposed Rule recognizes Minnesota wolves (Entity 1) as "eastern wolves" and the Draft Biological Report states that "[m]orphologically, eastern wolves have a long history of being considered distinct from western gray wolves" (USFWS 2018, p. 1), which includes western listed wolves (Entity 2).

Summarizing scientific evidence about the distinctiveness of eastern wolves, the Proposed Rule states that "the 'eastern wolf' has been variously described as a species, a subspecies of gray wolf, an ecotype of gray wolf, or the product of hybridization between gray wolves and coyotes…Minnesota appears to be the western edge of a hybrid zone between western gray wolves and eastern wolves – wolves in western Minnesota appear to be gray wolves both morphologically and genetically while wolves in eastern Minnesota and much of the Great Lakes areas appear to be 'eastern wolf,' introgressed with western gray wolf to varying degrees" (p. 9655). This suggests that the degree of distinction between western listed wolves and eastern wolves possibly increases across the west-east axis of eastern wolf range, such that eastern wolves outside Minnesota (Entity 1) in Wisconsin and Michigan (Entity 2) are even more distinct from western listed wolves than are Minnesota wolves. Results of a recent genetic study of eastern wolves (Heppenheimer et al. 2018) are consistent with this possibility. Note that this study was not among those cited in the Proposed Rule or Draft Biological Report.

Similarly, the Draft Biological Report suggests that the degree of distinction between western listed wolves and eastern wolves may increase across the west-east axis of western wolf range, such that coastal wolves are "genetically and morphologically distinct" from NRM wolves (USFWS 2018, p. 3). Although it seems the distinct coastal ecotype occurs in extant wolves mainly outside the western United States (Entity 2) in British Columbia, the Draft Biological Reports cites a genetic study of western wolves indicating that some western listed wolves in Washington may exhibit the coastal ecotype (Hendricks et al. 2018).

I found no scientific information in the Proposed Rule or Draft Biological Report supportive of the Service's interpretation that western listed wolves are not discrete from wolves in Minnesota, Wisconsin, and Michigan. Rather, the Proposed Rule and the Draft Biological Report supply scientific information that supports the opposite interpretation: that western listed wolves are discrete from wolves in Minnesota, Wisconsin, and Michigan. Note too that the section of the Proposed Rule entitled "Lack of Discreteness of Western Wolves within and Outside the Gray Wolf Entity" (84 FR 9648, p. 9653) does not explain the scientific basis for the Service's determination of a lack of discreteness within the gray wolf entity, including a lack of discreteness between western listed wolves and wolves in Minnesota, Wisconsin, and Michigan. Both gaps weaken the logic for: (a) combining western listed wolves with Wisconsin and Michigan wolves as a "listed entity" (Entity 2), and (b) combining western listed, Wisconsin and Michigan wolves (Entity 2) with Minnesota wolves (Entity 1) as "the gray wolf entity".

3. What is "current range"?

According to the Endangered Species Act and Service regulations, "a species may warrant listing if it is in danger of extinction or likely to become so throughout all or a significant portion of its range" (84 FR 9648, p. 9682). The Proposed Rule explains that "[t]he word 'range' refers to the range in which the species currently exists" (84 FR 9648, p. 9682). This is not a logical definition because it is circular: the term being defined is part of the definition. Confusing matters further, the Proposed Rule states that "[t]he range of a species can theoretically be divided into portions in an infinite number of ways" (84 FR 9648, p. 9684).

A vague definition of "current range" is problematic because the Service focused its analysis of factors that may threaten the gray wolf entity to "the range it currently occupies" (84 FR 9648, p. 9684). Specifically, the Service's "analysis of threat factors…does not consider the potential for effects to *C. lupus* in areas where the species has been extirpated – rather, effects are considered in the context of the present population" (84 FR 9648, p. 9659). It is difficult to judge the validity of this analysis in the absence of a logical definition of "current range" that includes clear criteria that distinguishes "current range" from "areas where the species has been extirpated". Such criteria are necessary to understand which areas the Service included in its analysis of threat factors, and why it included those areas and not others.

Maps in the Proposed Rule (Figure 2, 84 FR 9648, p. 9656) and Draft Biological Report (Figure 1, USFWS 2018, p. 8) show areas that the Service identifies as the "current distribution" of all *C. lupus* in the lower 48 United States. These are identified as gray-colored polygons in Figure 2 of the Proposed Rule, and as yellow-colored polygons in Figure 1 of the Draft Biological Report. Each map also separately illustrates the "approximate distribution at the time or listing (1978)" as a cross-hatched gray polygon in Figure 2 of the Proposed Rule, and as a green polygon in Figure 1 of the Draft Biological Report. Each map implies that the "current distribution" includes the "approximate distribution at the time or listing (1978)".

The Service does not explain the scientific basis for its determination of "current distribution" in Figure 2 of the Proposed Rule or in Figure 1 of the Draft Biological Report. Footnotes accompanying each figure do not indicate the source of information that is the basis for the illustration of current distribution. Nor is this source indicated in the main text of the Proposed Rule or Draft Biological Report. Lack of such documentation leaves open the question of what the "current distribution" actually depicts. For example, does it represent the locations of individual wolves, adults with pups, adults without pups, radio-collared wolves, uncollared wolves, or all of the above? How exactly is the boundary of the current distribution determined? Is it simply a 95% minimum convex polygon around locations or does it reflect other criteria? What time period do these locations (or other criteria) correspond to? Do they correspond to the time when the Proposed Rule was published (i.e., March 15, 2019) or are they averaged across a longer period?

Answers to these questions could help clarify how the current distribution in Figure 2 of the Proposed Rule and in Figure 1 of the Draft Biological Report relates to information presented in the main text of these documents. For example, the main text of the Proposed Rule explains that “as of 2017, three breeding pairs and four packs with no documented reproduction occur within the gray wolf entity in Oregon, Washington, and California” (84 FR 9648, p. 9656). Do each of these seven groups of wolves correspond to one of the seven “current distribution” polygons within the gray wolf entity and distributed across the Pacific coasts States? And if so, why are there three such polygons in Oregon given information in the Draft Biological Report that no more than two separate breeding pairs with pups currently occur in the state (see USFWS 2018, p. 14)? Is the third polygon a group or adults without pups, or a solitary wolf?

A science-based explanation of “current distribution” is especially important if it is synonymous with the “current range” that is the focus of the Service’s analysis of threat factors to the gray wolf entity. The Proposed Rule and Draft Biological Report do not explain whether “current distribution” is synonymous with “current range”. If they are not synonymous, more information is obviously necessary to understand the spatial extent of the threats analysis. Less obvious is that more information is necessary even if they are synonymous. This is because neither document explains if the threat analysis was limited strictly to the areas of “current distribution” within the gray wolf entity, or whether the threat analysis also included areas of “historical range” within the gray wolf entity.

It is reasonable to assume that the threat analysis with respect to wolves in Minnesota, Wisconsin, and Michigan was confined strictly to the area of “current distribution” within the gray wolf entity. This is because this is a single large area that contains all the habitat necessary to support more than 3000 wolves (USFWS 2018, p. 12). By contrast, the “current distribution” of western listed wolves is composed of several small areas except for one area in northern Washington that is contiguous with the current distribution of NRM wolves (see Fig. 2. 84 FR 9648, p. 9656). Was the threats analysis for western listed wolves confined strictly to these small areas of “current distribution”? Or did the threats analysis also include the matrix of “historical range” that surrounds and interconnects these areas with one another and with the current distribution of NRM wolves?

The Proposed Rule states that “wolves in the Pacific coast States”, which apparently includes western listed wolves, “are an extension of the metapopulation of wolves in western Canada and the northern Rocky Mountains” (84 FR 9648, p. 9656). The Draft Biological Report explains:

“A meta-population is a concept whereby the spatial distribution of a population has a major influence on its viability. In nature, many populations exist as partially isolated sets of subpopulations termed ‘‘meta-populations.’ A meta-population is widely recognized as being more secure over the long-term than are several isolated populations that contain the same total number of packs and individuals (USFWS 1994, Appendix 9). This is because adverse effects experienced by one of its subpopulations resulting from genetic drift, demographic shifts, and local environmental fluctuations can be countered by occasional influxes of individuals and their genetic diversity from the other components of the meta-population” (USFWS 2018, p. 22).

A threats analysis of western listed wolves limited to each of the small areas of "current distribution" would not be consistent with the Service's interpretation of these small areas as members of a "meta-population". On the other hand, a threats analysis that considers the matrix of "historical range" that surrounds and interconnects these areas would align with the Service's meta-population understanding of western listed wolves. However, the Service's emphasis on analyzing threats "to a species in the range it currently occupies" combined with questions about whether it considered "current range" as synonymous with "current distribution" leaves open the possibility that the analysis of threat factors considered each area of western listed wolves in isolation and not part of a larger meta-population. This possibility is supported by an absence of analysis or discussion about the potential for high levels of human-caused mortality in one western listed area to affect the viability of other western listed areas. Similarly, the analysis of threat factors did not consider how human-caused mortality in the core of the western meta-population (Idaho, Montana, and Wyoming) could affect the viability of outlying western listed wolves.

If current range is synonymous with current distribution, it is difficult to reconcile the Service's analysis of threat factors to the gray wolf entity, which focuses strictly on current range, with the Service's emphasis on meta-population structure, which values connectivity across "historical range" between seemingly isolated current ranges. Recognition of meta-population structure suggests that it is more logical to classify the interconnecting "historical range" as "current range" given that these interconnections reflect contemporary corridors of regular movement and occurrence, which are themselves subject to potential pack establishment. And because the sink areas of a meta-population can contribute to the viability of the meta-population (Howe et al. 1991; Heinrichs et al. 2015), some sinks may also logically qualify as "current range". Potential examples include the area in northern California where a pack with 2 adults and 5 pups (Shasta Pack) established in 2015 and apparently dissolved in 2016 (USFWS 2018, p. 15), as well as areas that are destinations for dispersing wolves including "North Dakota, South Dakota, Utah, Colorado, Nevada, Missouri, Indiana, Illinois, Nebraska, and Kansas" (84 FR 9648, p. 9656).

A final point of confusion related to the spatial extent of the Service's threats analysis are statements in the Proposed Rule that suggest the Service in some cases did analyze threats outside the current range/distribution of wolves within the gray wolf entity:

"Overall, public lands on the west coast have the ability to support the continued expansion of gray wolves as they disperse from resident packs and surrounding States and provinces to establish new packs in the west coast States. Because these areas are in public ownership and we do not foresee habitat-related threats, we conclude that they will continue to provide secure, optimal habitat for a resident wolf population" (84 FR 9648, p. 9681).

Do the areas of "public ownership" referenced in the above statement occur outside "current range" or outside the "current distribution" depicted in Figure 2 of the Proposed Rule and in Figure 1 of the Draft Biological Report? If so, why were these particular public lands the focus of a threats analysis, and why were other public lands excluded from consideration?

4. Is the review and analysis of human-caused mortality adequate?

The review and analysis of human-caused mortality were incomplete in several respects:

(A) The review overlooked many rigorous, peer-reviewed studies that are directly relevant to understanding and predicting rates of human-caused mortality among wolves inhabiting the current range of the gray wolf entity. These include studies by Maletzke et al. (2018), O'Neil et al. (2017), Stenglein and Van Deelen (2016), and Stenglein et al. (2015a, 2015b, 2018). This list is not exhaustive and further literature search may identify additional relevant studies.

(B) None of the data or analysis cited in the Proposed Rule or Draft Biological Report related specifically to western listed wolves. I found no information in either document that reviewed or analyzed human-caused mortality with respect to western listed wolves. The review and analysis focused on wolves in Minnesota, Wisconsin, Michigan, the northern Rocky Mountains (NRM), and Alaska. The Draft Biological Report also cited a study about human-caused mortality in the red wolf (*Canis rufus*) (Sparkman et al. 2011) that was not directly relevant.

(C) The best available data were not always used. The Proposed Rule includes a review and analysis of human-caused mortality in Minnesota that is based on radio telemetry data collected in a relatively small area (839-km^2) approximately 30 years ago (Fuller et al. 1989) when the number and distribution of Minnesota wolves were smaller than they are today (USFWS 2018, Appendix 1, p. 24). The Draft Biological Report highlights more recent "[d]ata from 5 concurrent radio telemetry studies tracking 36 packs, representative of the entire Minnesota wolf range" (USFWS 2018, p. 16) that could provide more current information on human-caused mortality over a wider area than that provided by Fuller et al. (1989).

(D) Data were sometimes attributed to the wrong study. The Proposed Rule cites Liberg et al. (2011) in support of its statement that "illegal killing was estimated to make up 70 percent of the total mortality rate in a north-central Minnesota wolf population and 24 percent in the northern Rocky Mountains population (Liberg *et al*, pp. 3-5)" (84 FR 9648, p. 9660). However, Liberg et al. (2011) studied wolves in Scandinavia and the data they cite about Minnesota and NRM wolves was attributed to separate studies by Fuller et al. (1989) and Smith et al. (2010).

(E) The analysis of human-caused mortality was mostly qualitative, retrospective, and ad hoc. For example, the Proposed Rule provides a qualitative comparison of annual rates of lethal depredation control with annual rates of population increase from 2003 to 2010 to argue that "depredation control would not adversely affect the viability of the Wisconsin wolf population" (84 FR 9648, p. 9672). I think the Service's overall argument that human-caused mortality no longer threatens the current and future viability of the gray wolf entity could be strengthen significantly if it was based on an integrated and predictive quantitative analysis of wolf demographic rates similar to those described by Maletzke et al. (2018), O'Neil et al. (2017), and Stenglein et al. (2015a, 2015b, 2018). The meta-population analysis described by Maletzke et al. (2018) seems especially well-suited to assessing the future viability of western listed wolves.

These quantitative approaches would allow the Service to convey their general expressions of confidence about the viability of the gray wolf entity in precise, measurable terms. These approaches also provide a rigorous framework for evaluating the possibility that the future may not behave like the past due to planned or unplanned circumstances. This could prove useful for predicting the combined effects of varying levels of depredation control and regulated harvest in populations that have been mainly exposed to only depredation control in the past.

Questions on the Proposed Rule for Peer Review:

1. **Does the proposed rule provide an adequate review and analysis of the factors relating to the persistence of the gray wolf population currently listed under the ESA in the contiguous 48-States (human-caused mortality, habitat and prey availability, disease and predation, and effects of climate change)?**

   No. Please see main comments in Sections 3 and 4 above.

   Additional Comments:

   (A) Disease

   The Proposed Rule states that "distemper is not likely a significant cause of mortality (Brand et al. 1995, p. 421)". Note that Brand et al. (1995) is an outdated reference, and that more recent work demonstrates that outbreaks of canine distemper virus are associated with significant increases in pup mortality (Almberg et al. 2009; Stahler et al. 2013).

   (B) Habitat

   The Proposed Rule that "[o]verall public lands on the west coast have the ability to support the continued expansion of gray wolves as they disperse from resident packs and surrounding States and provinces to establish new packs in the west coast States. Because these areas are in public ownership and we do not foresee habitat-related threats, we conclude that they will continue to provide secure, optimal habitat for a resident wolf population" (84 FR 9648, p. 9681). This assessment seems to overlook the well-documented risk of conflict between wolves and livestock on public lands in the West Coast States. For details see Hanley et al. (2018a, 2018b)

2. **Have we (the Service) adequately considered the impacts of range reduction (i.e., lost historical range) on the long-term viability of the gray wolf in its remaining range in the lower-48 states (outside of the northern Rocky Mountains)? If not, what information is missing and how is it relevant?**

   No. Please see main comments in Sections 3-4 above.

3. **Is it reasonable for the Service to conclude that the approach of Michigan, Wisconsin, and Minnesota to wolf management, as described in their Plans and the proposed rule and in the context of wolf management in the Western Great Lakes area, are likely to maintain a viable wolf population in the Western Great Lakes area into the future?**

Yes, it is reasonable for the Service to conclude that the approach of Michigan, Wisconsin, and Minnesota to wolf management is likely to maintain a viable wolf population in the Western Great Lakes area into the future.

4. **Please identify any oversights or omissions of data or information, and their relevance to the assessment. Are there other sources of information or studies that were not included that are relevant to the proposed rule? If so, what are they and how are they relevant?**

   Yes, there are other sources of information or studies that were not included that are relevant to the Proposed Rule. Please see main comments in Sections 1-4 above.

5. **Are there demonstrable errors of fact or interpretation? Have the authors of the proposed delisting rule provided reasonable and scientifically sound interpretations and syntheses from the scientific information presented in the draft biological report and the proposed rule? Are there instances in the proposed rule where a different but equally reasonable and sound interpretation might be reached that differs from that provided by the Service? If any instances are found where this is the case, please provide the specifics regarding those particular concerns.**

   Yes, there are demonstrable errors of fact, interpretation, and logic. Some interpretations of scientific information are not sound. There are several instances where a different but equally reasonable and sound interpretation might be reached that differs from that provided by the service. For details, please see main comments in Sections 1-4 above.

<u>Questions on the Draft Biological Report for Peer Review:</u>

1. **Does the draft report provide an adequate and concise overview of Gray wolf (*Canis lupus*) taxonomy, biology, and ecology? The changes in the biological status (range, distribution, abundance) of the gray wolf in the contiguous 48 United States over the last several decades?**

   No. Please see main comments in Sections 1-4 above.

2. **Please identify any oversights or omissions of data or information, and their relevance to the report. Are there other sources of information or studies that were not included that are relevant to the biological report? What are they and how are they relevant?**

   Yes, there are other sources of information or studies that were not included that are relevant to the Draft Biological Report. Please see main comments in Sections 1-4 above.

   <u>Additional Comments</u>

   (A) Figure 2, p. 12. I do not understand the rationale for combining counts of western listed and western delisted wolves when the Proposed Rule is focused on the status of western delisted wolves. This also seems to directly contradict a statement in the Proposed Rule that "we do not combine wolves in the west coast States with those in the NRM DPS and British Columbia, Canada, for the purpose of our analysis" (84 FR 9648, p. 9654).

   (B) The section entitled "Washington and Oregon" (p. 14) needs to more clearly distinguish between information about listed and delisted western wolves. As written, this information is conflated. For example, what are the population growth rates of listed wolves and are these rates influenced by management of delisted wolves?

   (C) The section entitle "Current Distribution and Abundance" (p. 22) describes the status of the Mexican wolf population as "growing" which is inconsistent with text on the preceding page that describes the population growth of Mexican wolves in 2017 as "relatively flat" (p. 21).

   (D) The url link on page 22 is broken.

   (E) Appendix 1: Why are annual count data only provided for wolves in Minnesota, Wisconsin, and Michigan? Similar data should be tabulated for western listed wolves in Washington, Oregon, and California.

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Peer Review of USFWS's Draft Biological Report and Proposed Delisting Rule

Clarification Questions and Responses

May 24, 2019

Project Name: Gray Wolf Removal Peer Review
Atkins Project #: 100062975

Reviewer 5 – Dr. Daniel R. MacNulty

GRAY WOLF 2019 PROPOSED RULE: PEER REVIEW

Clarification Questions

Peer Reviewer #5

No questions. Please see updated review above.