Comments on Preble's Meadow Jumping Mouse Delisting Proposal

(Listed in order received. Dates are those on comments.)

Reopened Comment Period

29. 2/6/06 Mark Lusch, Cheyenne, WY

30. 2/18/06 Tom and Mary Ann Cunningham, Green Mountain Falls, CO

31. 2/18/06 Bruce Roberts, Monument CO

32. 2/20/06 Mitchell Baldwin

33. 2/21/06 Oliver A. Richardson

34. 2/22/06 Robert B. Hoff, Colorado Springs, CO (see 1 and 6 above)

35. 2/22/06 Colleen Miller

36. 2/21/06 Linda Samelson, Colorado Springs, CO

37. 2/26/06 Jennifer K. Frey, Frey Biological Research, Radium Springs, NM

38. 2/25/06 Nick Ordon, Falcon, CO

39. 3/1/06 Unsigned, Colorado Springs, CO

40. 3/9/06 Leslie Barstow, Golden, CO

41. 3/9/06 Peter Bray, Portland, OR

42. 3/9/06 Donna Miller, Golden, CO

43. 3/13/06 Daryl E. Mergen, Colorado Springs, CO

44. 3/31/06 Ronald W. Opsahl, Staff Attorney, Mountain States Legal Foundation, Lakewood, CO (See 7 above)

45. 3/31/06 C. J. Rapp, Littleton, CO

46. 4/4/06 Ken Faux, Greenwood Village, CO (see 18 above)

47. 3/31/06 Ken Hamilton, Executive Vice President, Wyoming Farm Bureau Federation, Laramie, WY

48. 3/31/06 Renee C. Taylor, Environmental Coordinator, True Ranches, LLC, Casper, WY (see 12 above)

49. 4/13/06 Robert E. Arlen, Science Faculty, University of Phoenix, Casper, WY

50. 4/17/06 Sandra A. Eddy, Aurora, CO

51. 4/18/06 Kent Holsinger, Hale Friesen, LLP, Denver, CO. On behalf of Colorado Water Conservation and Development

52. 4/28/06 Robert A, Schorr, Zoologist, Colorado Natural Heritage Program, Colorado State University, Fort Collins, CO

53. 4/28/06 Eric Hallerman, Professor, Department of Fisheries and Wildlife Sciences, Virginia Polytechnic Institute and State University, Blacksburg, VA

54. 5/11/06 Sacha Vignieri, Center for Study of Evolution, University of Sussex, Brighton, UK

55. 5/15/06 Jonathan Dowling, Assistant Vice President, Wyoming Contractors Association, Cheyenne, WY

56. 5/1/06 Sallie Clark, Chair, Board of County Commissioners of El Paso County, Colorado Springs, CO

57. 5/16/06 Sylvia M. Fallon, Conservation Genetics Fellow, Natural Resources Defense Council

58. 5/17/06 Don Britton, Manager, Wheatland Irrigation District, Wheatland, WY

59. 5/17/06 Dale Moore

60. 5/18/06 Carron Meaney (Meaney and Co.; Reasearch Associate, DMNS; Curator Adjoint, University of Colorado Museum), Thomas Ryon (Wildlife Biologist and Certified Ecologist), Mark Bakeman (President, Ensight Technical Services Inc.) and Anne Ruggles (Bear Canyon Consulting), CO

61. 5/18/06 Tina Comerford, Wheaton, IL

62. 5/17/06 Niel A. "Mick" McMurry, Shareholder, Sybille Ranch LLC, Cheyenne, WY

63. 5/18/06 Rob Roy Ramey, II, Nederland, CO

64. 5/18/06 Jim Magagna, Executive Vice President, Wyoming Stock Growers Association, Cheyenne, WY

65. 5/18/06 Erin Robertson, Staff Biologist, Center for Native Ecosystems, Denver CO. On behalf of: Jeremy Nichols, Conservation Director, Biodiversity Conservation Alliance, Denver, CO and Nicole Rosario, Conservation Director, Forest Guardians, Santa Fe, NM (See 23 above)

66. 5/18/06 Patrick J. Crank, Attorney General, State of Wyoming, Cheyenne, WY

67. 5/19/06 Cheryl Matthews, Director, Douglas County Division of Open Space and Natural Resources, Castle Rock, CO (See 19 above)

25 Feb 2006

Field Supervisor
Colorado Field Office
Ecological Services
Bx 25486
Denver Federal Center
Denver, CO 80228

I would like to take this opportunity to submit my comments regarding the Preble's meadow jumping mouse. The USGS conclusion should be the reigning opinion and guidance for continuation of the Preble's protection. It is more detailed and uses more research in its finding.

Sincerely,

Nick Ordon

Nick Ordon
11450 Garrett Rd
Falcon, CO 80831

no-reply@erulemaking.net
03/09/2006 12:35 AM

To FW6_PMJM@fws.gov
cc
bcc
Subject Public Submission

Please Do Not Reply This Email.

Public Comments on Endangered and Threatened Wildlife and Plants; Notice of Six- Month Extension and Reopening of Comment Period for the Proposed Delisting of the Prebles Meadow Jumping Mouse (Zapus hudsonius preblei):========

Title: Endangered and Threatened Wildlife and Plants; Notice of Six- Month Extension and Reopening of Comment Period for the Proposed Delisting of the Prebles Meadow Jumping Mouse (Zapus hudsonius preblei)
FR Document Number: E6-02286
Legacy Document ID:
RIN: 1018-AU12
Publish Date: 02/17/2006 00:00:00
Submitter Info:

First Name: Leslie
Last Name: Barstow
Mailing Address:
City: Golden
Country: United States
State or Province: CO
Postal Code: 80403
Organization Name:

Comment Info: ==========

General Comment:Per the summary text, re-consideration of the delisting of the Preble's Jumping
Mouse is in order based on the conclusions of the new study.

no-reply@erulemaking.net
03/09/2006 12:53 AM

To FW6_PMJM@fws.gov
cc
bcc
Subject Public Submission

Please Do Not Reply This Email.

Public Comments on Endangered and Threatened Wildlife and Plants; Notice of Six- Month Extension and Reopening of Comment Period for the Proposed Delisting of the Prebles Meadow Jumping Mouse (Zapus hudsonius preblei):========

Title: Endangered and Threatened Wildlife and Plants; Notice of Six- Month Extension and Reopening of Comment Period for the Proposed Delisting of the Prebles Meadow Jumping Mouse (Zapus hudsonius preblei)
FR Document Number: E6-02286
Legacy Document ID:
RIN: 1018-AU12
Publish Date: 02/17/2006 00:00:00
Submitter Info:

First Name: Peter
Last Name: Bray
Mailing Address: 3169 NE Irving St
City: Portland
Country: United States
State or Province: OR
Postal Code: 97232
Organization Name:

Comment Info: ==========

General Comment:I OPPOSE delisting of the Preble's Jumping Mouse. As you well know, recent and
definitive studies indicate that this species is genetically distinct from the more common species. The position taken by your department is clearly political.

no-reply@erulemaking.net
03/09/2006 01:04 PM

To FW6_PMJM@fws.gov
cc
bcc
Subject Public Submission

Please Do Not Reply This Email.

Public Comments on Endangered and Threatened Wildlife and Plants; Notice of Six- Month Extension and Reopening of Comment Period for the Proposed Delisting of the Prebles Meadow Jumping Mouse (Zapus hudsonius preblei):========

Title: Endangered and Threatened Wildlife and Plants; Notice of Six- Month Extension and Reopening of Comment Period for the Proposed Delisting of the Prebles Meadow Jumping Mouse (Zapus hudsonius preblei)
FR Document Number: E6-02286
Legacy Document ID:
RIN: 1018-AU12
Publish Date: 02/17/2006 00:00:00
Submitter Info:

First Name: Donna
Last Name: Miller
Mailing Address: 84 Chalet Drive
City: Golden
Country: United States
State or Province: CO
Postal Code: 80403
Organization Name:

Comment Info: ==========

General Comment:Please keep the comments open for the Prebles Jumping Mouse. There is great
confusion as to whether it is a distinct species or not. More information is needed.

RECEIVED
MAR 14 2006

March 13, 2006

1835 Parkview Boulevard
Colorado Springs, CO 80906

Field Supervisor, Colorado Field Office
Ecological Service
P.O. Box 25486
Denver Federal Center
Denver, Colorado 80225

Subject: Comments on Preble's meadow jumping mouse

Dear Field Supervisor,

While the sub-species designation remains in the state of uncertainty, the logical method the USFWS should pursue would be to retain the Preble's meadow jumping mouse designated as threatened. Only after further study and certainty of the sub-species designation should this species be considered for removal from the threaten status.

The habitat, regardless of the mouse sub-species is what is truly threatened. The cumulative effect is great, as each acre of the riparian habitat where Preble's meadow jumping mouse is removed from the environment every species becomes more vulnerable for extirpation. In addition, as each acre of this riparian habitat is lost due to development, the cost of water treatment, flood containment, and sediment removal will increase. As a taxpayer, I pay for these hidden costs with increases in taxes and fees.

I recognize that the Threatened and Endangered Species Act is one of the few laws that can be used to combat uncontrolled development and used to protect certain areas. My only other comment is the USFWS could better explain how the loss of this part of the environment (the riparian habitat for the Preble's meadow jumping mouse) is impacting human health and costing everybody more money, regardless of the impact to a mouse.

It is easy for opponents of the Threatened and Endangered Species Act to portray "mouse huggers" (in this case) as putting the livelihood of a mouse over that of a human (developer or land owner). When in reality, protecting the mouse habitat is actually a societal benefit, while impacting a few developers or land-owners.

Thank you for the opportunity to provide my comment.

Sincerely,

Daryl E. Mergen

MOUNTAIN
STATES
LEGAL
FOUNDATION

2596 South Lewis Way
Lakewood, Colorado 80227
303-292-2021 • FAX 303-292-1980
www.mountainstateslegal.org

March 31, 2006

Ms. Susan Linner, Field Supervisor
Colorado Field Office, Ecological Services
U.S. Fish and Wildlife Service
Post Office Box 25486
Denver Federal Center
Denver, Colorado 80225

Re: Notice of Six-Month Extension and Reopening of Comment Period for the Proposed Delisting of the Preble's Meadow Jumping Mouse, 71 *Fed. Reg.* 8556 (Feb. 17, 2006)

Dear Ms. Linner:

Mountain States Legal Foundation ("MSLF") fully supports any decision to remove the Preble's meadow jumping mouse (*Zapus hudsonius preblei*) from the list of endangered and threatened species. On behalf of itself and its members, MSLF submits the following comments on the proposed delisting of the Preble's.

MSLF is a non-profit, public interest, legal foundation organized under the laws of the State of Colorado. MSLF is dedicated to the defense and preservation of individual liberties, the right to own and use property, limited and ethical government, and the free enterprise system. MSLF's members include businesses and individuals who live, own property, and/or work in nearly every state in the Union. Most importantly, many MSLF members live, own property, and work in areas of Colorado and Wyoming encumbered by critical habitat designations for the Preble's. These members depend upon the federal and private lands within these states for their livelihoods. Thus, these members have a significant interest in the removal of the Preble's from the list of endangered and threatened species and the removal of its critical habitat designations.

Reopening of Comment Period

The Preble's meadow jumping mouse is listed currently as a threatened species on the Endangered Species Act's ("ESA's") list of threatened and endangered species.[1] However, the taxonomic classification of the mouse as a distinct subspecies of meadow jumping mouse (*Z.*

[1] U.S. Fish & Wildlife Serv., Final Rule to List the Preble's Meadow Jumping Mouse as a Threatened Species, 63 *Fed. Reg.* 26,517 (May 13, 1998).

hudsonius) is in question.[2] As a result, the U.S. Fish and Wildlife Service began the process of determining if the Preble's is a discrete taxonomic entity or if the putative subspecies was listed in error.[3] Weeks before the delisting determination was due, however, an unpublished report was tendered to the agency asserting that the Preble's is genetically distinct from other *Z. hudsonius* subspecies.[4] Because of the conflicting genetic studies, the Fish and Wildlife Service has extended the delisting determination and solicited comments addressing the conflicting scientific opinions, the appropriateness of the classification of the Preble's as a subspecies, and other information relevant to a Distinct Population Segment analysis.[5]

The ESA mandates that any listing or delisting decision be based solely upon the "best scientific and commercial data available."[6] The best scientific evidence is clear: the Preble's is not a morphologically, genetically, or ecologically distinct subspecies of *Z. hudsonius*. Further, the Preble's is not a Distinct Population Segment of *Z. hudsonius*. Therefore, the removal of the Preble's from the list of threatened and endangered species is not only appropriate, it is required.

The Preble's Is Not Morphologically Distinct

A common tool to evaluate the uniqueness of a species is to evaluate skeletal morphometrics.[7] Most commonly, measurements taken at various points of the skull provide the necessary distinctions that allow taxonomists to distinguish between species or subspecies. Most recently, Ramey, *et al.* used nine skull measurements in an attempt to distinguish *Z. h. preblei* from *Z. h. campestris* and *Z. h. intermedius*.[8] The results of the Ramey evaluation yielded no appreciable differences between the three putative *Z. hudsonius* subspecies.[9] Therefore, based

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[2] *See*, Rob Roy Ramey, II, *et al.*, *Genetic Relatedness of the Preble's Meadow Jumping Mouse (*Zapus hudsonius preblei*) to Nearby Subspecies of* Z. hudsonius *as Inferred from Variation in Cranial Morphology, Mitochondrial DNA and Microsatellite DNA: Implications for Taxonomy and Conservation*, 8 ANIMAL CONSERVATION 329 (2005).

[3] U.S. Fish & Wildlife Serv., 12-Month Finding on a Petition to Delist the Preble's Meadow Jumping Mouse (*Zapus hudsonius preblei*) and Proposed Delisting of the Preble's Meadow Jumping Mouse, 70 *Fed. Reg.* 5404 (Feb. 2, 2005).

[4] Tim L. King, *et al.*, *Comprehensive Analysis of Molecular Phylogeographic Structure Among the Meadow Jumping Mice (*Zapus hudsonius*) Reveals Evolutionarily Distinct Subspecies* (Jan. 27, 2006) (unpublished manuscript available at http://www.mountainprairie.fws.gov/preble).

[5] U.S. Fish & Wildlife Serv., Notice of Six-Month Extension and Reopening of Comment Period for the Proposed Delisting of the Preble's Meadow Jumping Mouse (*Zapus hudsonius preblei*), 71 *Fed. Reg.* 8556 (Feb. 17, 2006).

[6] 16 U.S.C. § 1533(b)(1)(A) ("The Secretary shall make determinations required by subsection (a)(1) of this section solely on the basis of the best scientific and commercial data available . . ."). *See* U.S. Fish & Wildlife Serv. and Nat'l Oceanic & Atmospheric Admin., Notice of Interagency Cooperative Policy on Information Standards Under the Endangered Species Act, 59 *Fed. Reg.* 34,271 (July 1, 1994) (outlining the agency's policies to implement the "best scientific data" mandate").

[7] *See, e.g.*, Mary M. Conner and Tanya M. Shenk, *Distinguishing* Zapus hudsonius preblei *From* Zapus princeps princeps *by Using Repeated Cranial Measurments*, 84 J. MAMMALOGY 1456 (2003).

[8] Ramey, *et al.*, *supra* note 2.

[9] *Id.*

on morphometrics, the Preble's is not a distinct subspecies of meadow jumping mouse, and instead should be synonymized with *Z. h. campestris* and *Z. h. intermedius*.

The Preble's Is Not Genetically Distinct

Morphometrics alone often are insufficient to identify distinct subspecies, as many factors may contribute to individual size. Often, taxonomists couple morphometric analysis with genetic analysis to better detect subspecies. Ramey, *et al.* employed a careful evaluation of both mitochondrial and nuclear DNA in an effort to verify the Preble's as a distinct subspecies.[10] The Ramey study evaluated 205 specimens of *Zapus* species and subspecies, and yet was unable to identify any genetic distinction between *Z. h. preblei*, *Z. h. intermedius*, and *Z. h. campestris*. In contrast, Ramey, *et al.* were able to draw genetic distinctions between the *Z. h. preblei/Z. h. intermedius/Z. h. campestris*-complex and other Zapid species and *Z. hudsonius* subspecies.[11] This result is nearly identical to that of King, *et al.*, which found *Z. h. preblei* more closely related to *Z. h. campestris* and *Z. h. intermedius* than to either of the other two *Z. hudsonius* subspecies (*Z. h. pallidus*, and *Z. h. luteus*).[12] Therefore, as the Ramey study illustrates, the *Z. h. preblei/Z. h. intermedius/Z. h. campestris*-complex are genetically synonymous; that is, the Preble's is not genetically distinct from other putative subspecies of meadow jumping mouse.

The Preble's Is Not Ecologically Distinct

Another factor used to evaluate the appropriateness of designating a subspecies is ecological distinction. The Ramey study conducted a close evaluation of several factors used to identify ecological distinction and again found the *Z. h. preblei/Z. h. intermedius/Z. h. campestris*-complex synonymous.[13] The putative Preble's subspecies occupies habitat identical to other meadow jumping mice. Further, all putative subspecies share common life histories. Therefore, Ramey, *et al.* concluded that there were no adaptive differences preventing the *Z. hudsonius* subspecies studied from being ecologically interchangeable.[14] As a result, the Preble's is not ecologically distinct from other meadow jumping mice and, consequently, may not be held out as a distinct subspecies.

The Preble's Does Not Represent a Distinct Population Segment of Z. hudsonius

Finding the Preble's not to be a distinct subspecies does not end the inquiry, however, as a Distinct Population Segment of a species may also be afforded protections under the ESA. In this case, the Preble's subpopulation should be evaluated in comparison to the entire population of the *Z. h. preblei/Z. h. intermedius/Z. h. campestris*-complex. Three elements are considered in a decision regarding the status of a possible Distinct Population Segment. The FWS looks to: 1) the discreteness of the population segment in relation to the remainder of the species to which it

[10] *Id.*

[11] *Id.*

[12] King, *et al.*, *supra* note 4.

[13] Ramey, *et al.*, *supra* note 2.

[14] *Id.*

belongs; 2) the significance of the population segment to the species to which it belongs; and 3) the population segment's conservation status in relation to the Endangered Species Act's standards for listing (*e.g.*, is the population segment, when treated as if it were a species, endangered or threatened?).[15] As outlined above and as the Ramey study found, the Preble's is not a discrete population; nor is it evolutionarily significant.[16] Moreover, taken as a whole, the *Z. h. preblei/Z. h. intermedius/Z. h. campestris*-complex is not in danger of, or threatened by, extinction throughout any of its range.[17] Therefore, the Preble's does not represent a distinct population segment of *Z. hudsonius* and is not entitled to protections under the ESA as such.

Conclusion

Because the Preble's meadow jumping mouse is not a distinct subspecies of *Z. hudsonius* or a Distinct Population Segment, it was listed erroneously as a threatened species on the list of threatened and endangered species. As Ramey, *et al.* found, the Preble's is not morphometrically, genetically, or ecologically distinct from other putative *Z. hudsonius* subspecies. While there has been a challenge to the results of the Ramey study, King, *et al.* only challenged the results of the genetic analyses of Ramey, *et al.*, and genetics are only one factor to evaluate the status of a putative subspecies. The morphometric and ecologic distinctions found lacking in the Ramey study also must be considered. Moreover, taken as a whole, the *Z. h. preblei/Z. h. intermedius/Z. h. campestris*-complex is not a discrete, significant population in danger of extinction. Therefore, the Preble's must be removed from the ESA's list of threatened and endangered species.

Respectfully Submitted By:

MOUNTAIN STATES LEGAL FOUNDATION

Ronald W. Opsahl
Staff Attorney

[15] U.S. Fish & Wildlife Serv., *et al.*, Policy Regarding the Recognition of Distinct Vertebrate Population Segments Under the Endangered Species Act 61 Fed. Reg. 4722 (Feb. 7, 1996).

[16] Ramey, *et al.*, *supra* note 2.

[17] *Id.*

"cjrapp"
<cjrapp@glencodistributing.com>
03/31/2006 11:52 AM

To <FW6_PMJM@fws.gov>
cc
bcc
Subject Preble's Meadow Jumping Mouse

My I begin by stating; "All life is Precious".

With this issue concerning the Preble's Mouse, there has been a very disturbing trend in this country to undermine scince for the benefit of corporate profit. So someone with scientific credentials in this day and age will take a bribe to present data that is less than honest for the advantage of a developer. I would hope that someone in the government has heard of the prcautionary principle; when in doubt, don't !!! As we might precieve a small mouse to be similare to other small mice, and not important, what if the same was to be said of our spieces by an extraterestrail visitor. Germans and French seem to be that same, Italians and Greeks are the same, see where this goes ?

Please consider the precautionary principle in your determination of protection.

Sincerely C. J. Rapp
15581 West Sampson Road
Littleton, CO 80127
303-697-6246

Fauxden@aol.com
04/04/2006 03:13 PM

To FW6_PMJM@fws.gov
cc
bcc
Subject Public Comment

My comments on the recent actions re: Preble's mouse are attached.

Ken Faux
303-807-2739

Pub Cmnt '06.doc

Kenneth R. Faux Jr.
5301 S Yosemite St #23-202
Greenwood Village, CO 80111
Phone 303- 807-2739/ Fax 720-529-8714
Email fauxden@aol.com

April 3, 2006

USF&WS, Region 6
Attn: Seth L. Willey, Ecological Services

Via email – FW6_PMJM@fws.gov

RE: Extended Public Comment, Preble's Mouse Delisting Process

Dear Mr. Willey:

I am responding to the reopened (now extended) public comment period regarding the accepted petitions to delist Preble's Meadow Jumping Mouse as a threatened species and, the, "Comprehensive Analysis of Molecular Phylogeographic Structure among the Meadow Jumping Mice (Zapus hudsonius) Reveals Evolutionary Distinct Subspecies," by T. L. King et al. (2006) {the King study}, as well as the failure of FWS in meeting the final decision date promised over one year ago by DOI.

According to your web site: "Because of the contradictory conclusions provided by two recent reports, and the controversial nature of Preble's listing status, we intend to convene a panel of experts to evaluate all available information on Preble's genetics and taxonomy. This process will assist us in making a final decision regarding the taxonomic entity to be considered under the listing criteria of the Endangered Species Act. The Structured Decision Making (SDM) process will help to insure that the appropriate expertise is made available during the decision making process and that all pertinent information is adequately considered. The SDM process provides a rational and often quantitative analysis of the facts, maps the analytical process, and limits potential bias in the subjective elements of the analysis. Use of the SDM process makes any decision we ultimately make more scientifically defensible and transparent and provides a clear understanding of the thought processes behind the decision, thereby increasing legal defensibility." *I would ask; who determines appropriate expertise and what does all pertinent information consist of? If recent actions by the Service, or the comments by those who it apparently considers as a core of expertise are examined two themes are shown. First, the Service has sought to find expertise that might conflict with that submitted with the petition to delist PMJM rather than play by the rules that now exist and, the peer group with appropriate expertise has proven to be flexible in opinion, no matter which side of the fence they defend, or jump. They also indicate that it is impossible to reach a point where all pertinent information can be a realistic goal. A common suggestion to each level of study is, "If only we knew more. Why don't we add this technique and do more research before we are happy with any answer." The bar to clear is the current law, not new standards set by Regional whim or suggested as potentially better solutions by reviewers.*

The site also stated: "Seeking to use the best science possible in making a final decision, the Service also commissioned Dr. Tim King of the U.S. Geological Survey (USGS) to do an independent genetic analysis of several meadow jumping mouse subspecies. The USGS study results, provided to the Service on January 25, 2006, raise significant questions about the conclusions drawn by Dr. Ramey in his study." *There is no requirement for the "best science possible" to conclude this process. The statement is not only incorrect in terms of the governing law but suggests Dr. King as so superior, in the eyes of officials in Region 6 to any other possible biologist in the history of the universe, as able to provide the final, indisputable answer. How would the Service even define the term when the possible is unknown? "When pigs fly" comes to mind, as the peer reviews shown below indicate that few, if any, of those involved can come to the same conclusion twice in a row. Yet, FWS holds King's debated past work up as the best possible opinion they can solicit in an effort to refute Ramey. One would think they could have found a less aquatically focused expert within FWS rather than going out to USGS to debate a mammalian issue. You not only went out of the box, but under the atmosphere!*

"Because of the complexity of this issue, the Service has extended its original proposal to delist the PMJM for six months, as allowed under the Endangered Species Act. At the same time, the Service has reopened the public comment period for 60 days to solicit additional information and provide additional time for the Service, peer reviewers, and the public to examine and comment on Dr. King's report and all other available information." *As discussed, this is a laudable but incredibly naïve goal. Those involved seem hard wired to never believe ALL information has been made available. The majority of the peer review group suggested that additional information and time was needed to make a decision. They have held this opinion at every stage of the process and I see no indication that will ever change. There is always a potential discovery, just around the corner, that may provide a better answer if we just spend enough resources on finding it. Unfortunately, the resources are limited and the expenditure unjustified in light of other serious needs within agency programs.*

The Regional office not only failed to meet the promised decision date, but appears to have acted independently to ensure the delisting process continue on as a time and money consuming source of frustration for many private property owners and local governmental units in the habitat area. Region 6 did not follow ESA procedures by considering the facts provided in the approved petition on their own merit. In subcontracting another agency to provide a new study with their biologist of choice, the Region made a unilateral decision not to accept the work of Ramey, the basis of the approved delisting petition (already subject to both FWS and external peer review of the published work), but to privately solicit additional comment as necessary scientific study. Given a history of sub-species bias and oft disputed or recanted findings by the source of the new study, it is not unlikely King's findings might derail the DOI acceptance of the petition and find that the published, reviewed work accepted in that decision must be wrong.

The King study may have been done, without notification of or authorization by, DOI officials that originally agreed to the validity of the evidence presented in the petition. The Region waited until nearly the end of the time frame announced by DOI officials to even commission the study, causing a rather hasty attempt to overturn the work provided in the petition. This action is very questionable, with a high degree of exposure to potential administrative action or legal challenge. Seeking peer review on the King study, instigated by Regional supervisors who subverted the decisions of senior management, seems more an effort to discredit Ramey than to act on the petition and related science as submitted. Why the Region took the process down that path should be subjected to a review regarding any misuse of authority, budget or process.

The questionable decision to seek a second opinion caused the promised response period to be missed by a month. Funds and time were spent to forward what could be considered a Regional agenda. That may seem fair if the process was considered a rush to judgment; that is not the case. The delisting petition was submitted in December of 2003 and subject to more than a year of review prior to being deemed worthy of consideration. Another year passed before the answer given was that more time and review was need to consider a new study, which despite its title including, "Reveals Evolutionary Distinct Subspecies" has been described as offering only a .05% genetic variance. This hyperbole regarding the result has since caused a DQA challenge, likely to cause further foot dragging by FWS. Pure data aside, King's own comments reported in that challenge indicate he questioned both his own ability to interpret the results and questioned any material differentiation, a far cry from the title of his report. Even that title indicates his effort did not actually contradict Ramey, whose work was based on genetics. King's finding was titled as based on evolution, an ongoing process that does not preclude genetic synopsis. King's conclusion that his findings indicate a geographically split level of distinction of Preble's subspecies strains the imagination and discounts the critical habitat argument. If the evolution is different, what is the reason? I would think environment, so if the current single definition of the habitat is correct, King is not. No evidence of sufficient variation in the habitat to determine north and south subspecies is given for what is now considered a single habitat region. If King is right, the critical habitat definition must be revised to reflect the resulting subspecies based on material variation in the environment that would cause a new subspecies. Won't that be fun? And, what if one group remains threatened but sufficient population and habitat would support delisting of the other were to occur? We could keep the argument going for years! Something I fear is the actual goal of many involved in the discussion.

Regional officials solicited and accepted peer reviews of an unpublished draft of the King study. Oddly, and indicative that suspicion of an internal agenda would not be far fetched, the draft was originally not listed for public consumption on the Service web site but a link was available via the site of a well known conservation group. They are openly opposed to any delisting and frequently work to add more species to the already overburdened and under funded list. This followed on the Ramey peer review being sought from basically the same (some such as the Governor of Wyoming might say predisposed) group that had provided the initial listing review. If split on support of his work, that group had shown as much agreement as might be expected - given that they would have to admit to a change of heart to support the Ramey findings. Having done this, the Service has extended the delisting period to allow more costly study, with no specific budget, more internal debate and no specific date for a decision. Public comment is also sought on an issue that the original period found to be of limited interest and is likely to see less on this go-round. It is, after all, a mouse. And, a fairly common one if the increasing incidence (five times the number of active population areas identified when first listed) of finding them, as supported by research and permit related trapping, is accurate.

Given the above flaws in the Region's efforts to date, I offer the following re: the new peer review on the report, A list of various questions that were circulated for the peer review sought by FWS regarding the Ramey and King work follows.

Specific Questions to Consider for Review of Dr. R.R. Ramey's Report on Genetic Analysis of Preble's Meadow Jumping Mouse

Question 1: Do the morphological, ecology, and mtDNA data presented in the report support the authors' conclusions on synomyzing *Z. h. campestris* and *Z. h. preblei*?

1. Please analyze the techniques used in the population and phylogenetic evaluation of *Zapus hudsonius preblei* and other taxa. Were appropriate methodologies and markers used?

Question 2: Could you support synomyzing *Z. h.campestris* and *Z. h. preblei* without additional genetics studies (i.e. microsatellite data)? If not, what additional analysis is needed?

2. Are the conclusions about the taxonomic validity of *Z.h. preblei* logical and defensible as presented in the manuscript?

Question 3: What is the importance of potential ecological, behavioral, or physiological differences between *Z. h. campestris* and *Z. h. preblei* in substantiating or refuting synonomy?

3. Are there possible alternative interpretations of the genetics data?

Question 4: What is the likelihood that the *Z. h. preblei* is substantially reproductively isolated from other groups within the Z. hudsonius complex, especially from *Z. h. campestris*?

4. Are there additional or divergent taxonomic conclusions that could be drawn from the genetics data?

Question 5: Would the loss of what is now *Z. h. preblei* represent a substantial diminution of *the Z. h. campestrix* taxon? Its Range? Biological characteristics? Evolutionary legacy? Other?
5. Do you agree with the interpretation about possible mechanisms of reduced gene flow between *Z.h. preblei* and other subspecies of *Z. hudsonius*?

Question 6: Do you agree with the concepts of Crandall et al. (2000)* for defining evolutionarily significant units?

Question 7: Are there clear ecological distinctions between *Z. h. preblei* and closely related taxa that would suggest a need for specific conservation actions for this taxon?

The following are excerpted and edited for brevity from the FWS published comments. This was done not to promote a bias of opinion by this writer but to make the document reasonable in length and use comments that allowed for comparison of the views expressed by the respondents. It also allowed for the sometimes detailed references to terms obscure to the layman or multiple use of scientific terminology or cited references to be deleted. I urge interested parties to view the full text provided on the web, under the FWS Preble's home page, peer comment link. Comments on the reviews retain this font format, otherwise the information represents outtakes from the actual reviews as posted.

David M. Armstrong, Professor, Museum Associate Curator - Mammals, University of Colorado re:King

"I am not a molecular systematist or phylogeographer so will not presume to evaluate laboratory, analytic, or statistical methods." *So why was he asked to evaluate a pyylographically based study and go on to say,* "it is neither a thorough phylogeographic study of an entire species nor a taxonomic revision." *And* "Unlike Ramey, however, King is based on considerably larger samples of individuals and larger fragments of genetic material." *This comment is questionable if the DQA challenge is accurate as to sampling procedures used.*

He goes on to say "King presented no genetic data for southeastern and east-central Wyoming. King demonstrated significant genetic differences between meadow jumping mice from northeastern Wyoming and adjacent South Dakota and mice from north-central Colorado. That is not particular*(ly)* surprising, because there is a significant area of east central Wyoming that seems to be unsuitable as habitat for the meadow jumping mouse." *Given his argument, why would he want information from the Thunder Basin, which has little riparian environment. The area of east central Wyoming discussed, is shown on maps as having even major watersheds often being seasonal at best. That geographic fact would not preclude a spread (or support a DPS) of the species along the I-25 corridor, through Wyoming and into Colorado. But, as mentioned by others, a lack of study in the more likely SE corner of the state, where interaction with Colorado populations are more likely may well be a critical oversight.*

Yet he concludes, "Lack of information about the presence of meadow jumping mice in east-central Wyoming was a critical flaw in the argument of Ramey. If the populations are geographically isolated it would be interesting to know whether that isolation dates to post-glacial warming and drying, the Hypsithermal Interval, or perhaps the dramatic but much more recent ecological changes wrought in the late 19th Century by open range livestock grazing in the region." *He notes Ramey also failed to study an area he feels is not good habitat, so why does this lead to a flawed report? As to the rest of the paragraph, perhaps, but is the "interest" material to the facts presented and, given the area had been Bison habitat for hundreds of years prior to the introduction of beef cattle in the last half of the 19th century, how significant are ungulate habits to degradation of mouse land? They seem to have faired just fine.*

"Overall, I found the report by King to be well-reasoned, thorough, well-written, and convincing. It is a principle of science that we need to "follow the data where they lead." It is also true that the more data there are and the more reliable those data are the better the "leadership" the data can provide." *Perhaps true, but typical of a common thread in comments of the academicians asked to respond that, it is better to study than to conclude. The drive to publish, research and seek grant money to do so seems to provide powerful incentive to look for, rather than provide, an answer.*

"On that common-sense basis, this unpublished report by King is a far more useful document than Ramey. I hope King will be published soon in the open, peer-reviewed scientific literature." *This has not yet, and unlikely to happen within the next six months allowed for further study & comment.*

But, in March of 2004, responding to a request for comment on the Ramey study, Professor Armstrong made the following comments in a forty two page response.

I have taken literally the invitation to provide comments and constructive criticisms. I trust that the comments to follow will be taken in the spirit offered. As with any pure review, a little country wisdom may be salutary: considered the source, remember how much it cost you, and take it or leave it

Caveat lector: my comments may reveal some annoyance or appear to be hypercritical. I believe this is annoyance with the process and not with the substance of the report or its authors. The background is this: I have experience providing reviews of technical manuscripts for professional journals. In that process one presumes that comments can be considered and if appropriate and relevant can be incorporated to improve a paper. The "industry standard" is either an informal, collegial "preview", before a manuscript is finalized and submitted for publication, or it's a collegial peer review, at the request of a professional journal, intended to fine-tune a submitted paper and/or to recommend against publication.

You'll find that I continue to suggest changes in spelling, typography, grammar, and the like although I realize that this report is *fait accompli.*

In the present process, the report apparently has been submitted and publicized, so it is beyond the point of useful, collegial, constructive input. Still, I will make editorial comments as well as substantive to comments because I can't help myself and because the two are not unrelated. The medium is part of the message. Further, the comments may be useful as this report is rewritten as a manuscript for possible publication.

For what it's worth it, I had the same trouble reviewing (at the request of FWS) various notices in the *Federal Register* concerning Zapus hudsonious preblei; I had the same conflicted feeling that, on one hand it, I wanted to the right thing for colleagues or the mice, but-on the other hand-from any practical standpoint the review was irrelevant. In fact case, I did the reviews anyhow because they were required by some bureaucratic and political process. In I decided to do the reviews anyhow because of they were requested by friends and I suppose there is the chance that they will it influence in eventual published a paper on this topic or eventual conservation success in a local and regional ecosystems.

I'm not sure how pertinent this display of pique is to the request for comment on the scientific work done. The comments indicate he feels both put-upon and this is a waste of his time. That attitude fails to move the process forward in a substantive way or set a tone that makes the reader assume lack of bias but, prone to discounting whatever the message delivered may be.

The substantive, take home message from of the report is that the authors performed sophisticated molecular genetic studies, tested statistically some *a priori.* hypotheses, conducted a multivariate statistical assessment of the quantitative measurements used by Kru(*t*)zsch (1954), and evaluated the logic of qualitative descriptions and comparisons in Krutzsch's original description of Zapus hudsonius preblei.

Space prohibits going into great detail about the remainder of the Armstrong response but his, review includes many tangents that add little to the central point. This is not to discount his willingness to provide input or demean his credentials but simply to note that his dissatisfaction with single spacing, grammatical style and the general failures of the FWS process overtake his

constructive comment or conclusions on the substance of Ramey's work. He quibbles on matters of report v. manuscript and argues at the philosophical level about Ramey's approach rather than the quality of the work.

He disqualifies himself as competent to provide peer review of molecular systematics but continues to offer his opinion on the study. To what end? He launches into nearly a dozen pages of comment that run the gamut from legitimate questions of biology to being the grammar and spelling fairy. A task that his own writing indicates he does not serve well. He even makes notations on an included copy of the study that suggest the title be modified to Evaluating v. Testing and use of the word subspecies, the very premise that the study discounts, and which he had objected to in prior comments. Armstrong dives off another cliff with comments about agreeing that the ESA needs reform and then offers up the idea that the entire concept should be reconsidered along the lines of moving to declare an international Endangered Ecosphere Treaty. Without disagreeing that a macro/global effort may be of value, one can only imagine the costly can of legal worms that would open or how it relates to questions asked. Even he admits to this being a digression from the input he was asked to provide.

Mary Ashley PhD., Department of Biological Sciences, University of Illinois at Chicago

It is surprising to me that these two studies, which used similar approaches for the genetic work, came to such strikingly different conclusions. I do not know the complete explanation for this conflict. The data collection and analyses in both studies seemed to be technically sound. My best guess is that the conclusions of Ramey et al. were based much too heavily on a few questionable museum specimens rather than more recent field collections (a point I brought up in an earlier review). I would also point to the sampling for the microsatellite study in King et al. was much more appropriate than that of Ramey et al., both in terms of population sampling and number of loci assayed. I believe these two factors contributed more to the conflicting conclusions than specifics of data analyses. I previously felt that conclusions of the Ramey et al. study were premature, and was not in favor of synonymizing the subspecies at that point. I feel that King et al. have shown convincingly that such synonymy is not warranted. *I fail to see, even by King's own result and comments made, there is any such "strikingly different" conclusion made and may well be indicative of greater similarity than thought given the different sampling techniques used.*

Yet she stated in her review of the original Ramey study:

Although I am not an expert on morphological data analysis, I found that the authors' strongest case for synomyzing the two subspecies comes from their thorough morphological study. However, it does not appear that the two subspecies are morphologically distinct. This is a fairly straightforward conclusion based on solid data set.

Robert D. Bradley, Associate Professor *(his letter did not say where or of what)*

Excerpted from comments on King

Overall, the manuscript is technically and scientifically sound. Actually it represents an ambitious approach to resolving the systematic issue at hand. Philosophically, I disagree with their conclusions, mostly as a result of their interpretations. I will address this and other issues in responding to the series of questions outlined in the review.
1). Techniques...I like their approach. The inclusion of 21 microsatellite loci is outstanding. This is a minor criticism and is more for thought than anything. Overall, the appropriate markers and methods were used.

2). Conclusions...This is the gray area that unfortunately is at the heart of the study. The authors definitely demonstrate that genetic variation exists between populations and that the variation corresponds to taxonomic units. Here is crux of the problem. If you randomly picked populations that were somewhat isolated (genetically or by geographic distance) you might find evidence of little or no gene flow, especially with microsatellite data. It is too difficult to determine if this is the case in this instance. Given that you would fall back to the position – how different are the groups genetically? I would conclude that they are genetically different but the level of differentiation is extremely small. Does this warrant recognition as a separate subspecies? Probably not, in my opinion.
3). Two populations...The authors have shown a northern and southern genetic division in *preblei*. Again the answer is similar to question number 2 – I think you would expect this in most populations.
4). Alternative interpretations... I think the authors have demonstrated difference at the populational level but I am not sure that the case can be made at a higher taxonomic level.
5). Additional analysis... I would recommend a Bayesian analysis with clade probability values.
6). Conflicts...I am not sure the two conflict as much as the authors state. Both show similar groupings of taxa. The differences lie in where you draw the line and infer either populations or subspecies. In many ways, this is a philosophical debate and may not be resolvable.

His original comments re: Ramey were -

This is an excellent piece of work on a controversial issue. I like the way the authors set up the study by specifically testing a series of hypotheses related to the bigger picture.
1. The data presented do support the synomyzing *Z. h. campestris* and *Z. h. preblei*. Both the morphological and mtDNA analyses are convincing in that the two taxa actually represent a single taxon.
2. I could support the placement of the two taxa in synomy without further data.
3. Ecological, physiological, or behavioral differences are or would be extremely important in building a case that *Z. h. preblei* is significantly different from *Z. h. campestris*. In the absence of significant differences concerning (such) patterns between the two taxa; these parameters are of lesser importance.
4. My guess from the data and distribution is that the two are not reproductively isolated. The best example is with the mtDNA data.
5. It does not appear that that loss of *Z. h. preblei* would significantly impact *Z. h. campestris* in terms of genetics.

This is one of the few instances where brevity is combined with a conclusion and the position taken remains fairly consistent. Bradley seems willing to give credence to both bodies of work while not defaulting to a need for further study or tangential comment.

Keith Crandall, Thomas L. Martin Professor of Biology, Brigham Young University

{King} Summary Conclusion (2006)

If I were forced to draw a conclusion from this study, I would first of all include many caveats including all those listed above in terms of additional analyses, additional data collection, explicit hypothesis testing, and additional taxon sampling. Yet, given the results, my interpretation would be that (*it*) clearly shows three distinct clades. These correspond to what I would call subspecies (given the current taxonomy and lack of sampling of the other subspecies), which include *Z.h.preblei/intermedius/campestris*, *Z. h. pallidus,* and *Z. h. luteus*. Indeed, these three subspecies also correspond to the three clusters found by structure. Thus, there seems to be agreement in

both the nuclear and mtDNA in the rejection of genetic exchangeability between these three subspecies. Conservation implications should take into account these evolutionarily significant units. This conclusion assumes that the data and analyses presented in these reports are reasonable and accurate (King correctly point(s) out the weaknesses of the Ramey data with respect to museum samples etc.). Furthermore, this conclusion could very well be rejected with further and more appropriate sampling (as outlined above). Given the sampling issues, the prudent action at the moment is to leave the subspecies listed as is (endangered) and reevaluate after a full study of the species can be accomplished including extensive sampling across the distributional range of each subspecies. *Note that Preble's is considered merely threatened.*

His review of Ramey's work (based on questions in order shown above) was -

Appropriate markers and methods were used. The control region would provide the highest possible resolution using mtDNA. As the authors state, microsatellites would provide additional insights but would not alter the general conclusion.

Indeed, the conclusions are right on. This work is particularly impressive by its inclusion of both genetic and morphometric data coupled with an evaluation of previous work. The author is spot on in every respect. Indeed, it looks like you will have some more work to go to figure out an appropriate taxonomy for this group. The current taxonomy clearly does not reflect the inferred evolutionary relationships. But it is clear that the *Z. H. preblei* is not a valid taxon and that the animals on the front range of CO are genetically represented in other areas.

I can't think of any – at least not relative to the taxonomic status of the Preble's Meadow Jumping Mouse. Some additional work could be done to develop a reasonable taxonomy and make global inferences about population structure, bottlenecks, range expansions, etc. for the species.

Are there clear ecological distinctions between *Z. h. preblei* and closely related taxa that would suggest a need for specific conservation actions for this taxon?

The morphological analysis suggests that there are not. If there were clear ecological differences that were persistent over evolutionary time and adaptively important, one might expect the evolution of morphological differences. In many cases, this occurs long before divergence of neutral genetic markers. For example, Polar Bears are obviously morphological distinct from Brown Bears, yet genetically they do not form distinct clades. Here we see no obvious morphological distinctiveness that relates to the designated subspecies. Indeed, the critical review of the previous work designating this subspecies identifies a number of significant problems with it. There is always a possibility that we are simply not looking at the right (critical) character. But of those examined, there does not appear to be any distinction.

In summary, I found this to be an excellent study covering all the appropriate bases. The conclusions drawn are, in my opinion, well founded and well supported by the data. The investigator has done an exceptional job in planning the study, selecting appropriate data to collect, collecting data, analyzing data, and interpreting the results. I agree with the conclusions provided by the investigator in this report and find them based on solid science.

Having made such glowing comments about the validity of the science and techniques used by Ramey, one can only wonder at why Crandall decided to throw him under the bus in his comments on King. This is a nearly 180 degree switch in opinion, suggesting he really doesn't have one or, is not the most qualified to comment. The credentials are there as to qualifications

and I can only believe he mirrors the inability of many respondents to stick to a story. While new evidence can always be cause for a change of heart, it needs to be of enough significance to cause a true mea culpa, as was the case when Ramey's work was acknowledged as superior to his own by Krutzsch, not merely a minor variation possibly explained by shift in technique.

Sara Oyler-McCance, Assistant Research Professor, Biological Sciences, University of Denver

Professor Oyler-McCance's comments were detailed and very lengthy, the material shown cut many pages of material while attempting to maintain comparison points. Starting with Ramey -

Overall, I agree with the authors' approach to investigating the taxonomic validity of Preble's Meadow Jumping Mouse. Specifically, I believe it is a good idea to use multiple lines of evidence (not just genetic data) to clarify taxonomic borders. My comments will focus more on the genetic aspects of this study than on the morphological aspects as is consistent with my experience and expertise. From a genetic standpoint, this study uses an appropriate genetic marker and does an excellent job analyzing the data from a phylogenetic standpoint. This study provides a great data set from which to **begin** to answer the question at hand. I do not feel, however, that this study by any means resolves the taxonomic question. Further, I feel that some of the conclusions made by the authors are debatable. Trying to define a subspecies is even more nebulous but has resulted in a similar discussion of how to define a "unit" for conservation below the species level. My point here is that there is not one "accepted" definition in the literature of how to define a subspecies or even a species. Different species or ESU concepts can be applied to the same data with widely different results.

The authors of this study use three criteria to determine whether or not the Preble's subspecies is a valid one from the genetic standpoint: reciprocal monophlyly, Ramey's AMOVA test, and the criterion of Crandall et al. 2000. The author's state in this report that they feel the reciprocal monophyly definition is too strict. I agree with this idea particularly in light of my experience with the genetics of Sage-Grouse. The second test is Ramey's assertion that the subspecies boundary exists when there is more variation among groups than within groups using AMOVA analysis. This measure, while discussed in 3 papers published by Ramey is not well tested in the literature or accepted as a standard measure. The authors make the statement that this measure is less restrictive than the reciprocal monophyly definition and I am not sure that in all cases it really is.

In this study the authors find that *Z. h. campestris* is most closely related to *Z. h. preblei*. Further, they show that all four haplotypes found in *Z. h. preblei* are shared with *Z. h. campestris*. This does seem to suggest that somehow *Z. h. preblei* are a subset of *Z. h. campestris*. I would be very skeptical to conclude **undeniably** that *Z. h. preblei* and *Z. h. campestris* are synonymous without including nuclear data.

The authors claim that using all three criteria, they reject the idea that *Z. h. preblei* is a valid subspecies. Certainly the data show that *Z. h. preblei* are not reciprocally monophyletic. This concept, however, can be overly restrictive (in my opinion) and only utilizes genetic data, which in my mind, is problematic. The second criterion based on AMOVA has not been well tested (at least that I know about, see below) and therefore I am not comfortable using it to define a subspecies. The third criterion is conceptually a good one. My biggest problem with this criterion is how the authors report their finding of ecological exchangeability.

Specific comments:Pg 3 – It is unclear from this report whether the Preble's Meadow Jumping Mouse was listed as a DPS or subspecies. *Listed as a subspecies, the idea of DPS was a backup position for those supporting continued listing.* Include a range map.
Pg 4 –I am skeptical of Ramey's method of defining subspecies based on the relationship between variability among vs. within populations. How does this method work across different molecular markers? Further, I am not convinced that it is always less restrictive than reciprocal monophyly.
Pg 13 –This data suggest that they may not be separate, but without further analysis I don't believe the question can be answered undeniably.
Pg 14 –While I do agree that they have made a good start to answering the question, without the addition of nuclear markers their data is severely limited. I believe that the cost of adding nuclear markers and additional samples to address the *preblei/campestris* question at a population level will not be trivial and that is misleading to USFWS and other agency personnel to suggest otherwise.

Answers to specific questions to consider for review of Dr. R.R. Ramey's report on genetic analysis of Preble's Meadow Jumping Mouse

The use of mitochondrial control region data is an appropriate marker to use to begin to address the taxonomic question at hand. It is important, however, to include nuclear markers as well before definitive answers about taxonomic delineations are made. The authors used the proper methodology for the phylogenetic analysis. I am less comfortable with the "population analysis" mostly because it is based solely on only one test.

I have no problems with the study itself except for some of the conclusions made by the authors. I feel that in some cases they have made recommendations based on an incomplete data set.

Other studies have found discordance between mitochondrial and nuclear data sets. It is possible that nuclear data might reveal a difference between the two subspecies that was masked in the mtDNA through introgression. It seems odd to me that 5 of the 7 *Z. h. campetris* samples that most closely resemble *Z. h. preblei* all are found in one location

I do agree with the authors that the data seem more consistent with a southward colonization from *Z. h. campestris*.

Conceptually I think the concepts of Crandall are reasonable. Additionally, I don't feel that the concepts of Crandall are necessarily any better than some of the other concepts in the literature.

I know nothing about the ecological distinctions between the subspecies and am concerned that the authors used this as a criterion yet failed to report what variables they used. In this report, the authors investigate the taxonomic relationships among five previously defined subspecies of *Zapus hudsonius* using both genetic and morphological data. Such taxonomic delineations, in my opinion, should always be investigated using multiple lines of evidence so I am in agreement with the approach taken by Ramey to address this question. I will focus my comments primarily on the molecular aspects of this work and less on the morphological aspects. Previously, I commented that I thought it was premature to synonomize *Z. h. campestris* and *Z. h. preblei* given several questions I had about the data set and given that Ramey et al. had considered only mitochondrial DNA and not nuclear markers.

General Comments
In this report, the authors base their conclusions entirely on Ramey's own criterion that "there must be greater diversity among putative subspecies than within them". This criterion, while discussed in three papers published by Ramey and colleagues, has not been tested in the literature or accepted by the scientific community as a standard.

Answers to Specific Questions Posed
Regarding the molecular side of this study, the authors use an appropriate genetic marker and do a good job analyzing the data from a phylogenetic standpoint. I feel that from a population standpoint, their study is weak due to small sample sizes, yet the goal of the study was not to study populations within a subspecies, but rather to investigate the taxonomic validity of the subspecies. Their report provides some good basic information regarding the relationship among the previously described subspecies, yet it by no means resolves the taxonomic questions. Further, I feel that it is a bit reckless of the authors to suggest otherwise. I question the seemingly haphazard way that some samples were dropped from the original report because they were assumed to be misidentified. It is impossible to determine whether the calculations that the authors used for mutation rates are reasonable or not.

I do not support the authors' conclusion to synonymize *Z. h. preblei*, *Z. h. campestris*, and *Z. h. intermedius* based on this data set. I feel it is premature to even consider changing the taxonomic status of these subspecies without first examining data from the nuclear genome and also addressing the specific issues stated above.

There are several issues surrounding the MDIV data that make me question the results and the conclusions of this analysis. Even if I could be convinced that the differing population sizes did not matter and that their use of mutation rate was valid, I would still be hesitant to consider *Z. h. campestris* and *Z. h. preblei* a single connected population with migration rates on the order of one individual per generation

The addition of nuclear markers is essential before any real conclusions can be made regarding these subspecies for all the reasons stated above. The new information presented in this report has not changed my conclusions regarding the synonymizing of *Z. h. campestris* and *Z. h. preblei*. There were numerous interesting and valuable comments provided by the reviewers of the first report. This report presents only additional data from one additional subspecies and does not address any of my concerns from the first review.

Now working for the same agency as the King study her version was -

Sara Oyler-McCance, Rocky Mt. Center for Conservation Genetics & Systematics, USGS, c\o Department of Biological Sciences, University of Denver

There are several significant differences between the two studies. The study by King uses more samples *(than Ramey)* much more sequence data from two mtDNA regions and many more microsatellite. The two studies use some different methods to analyze their data although some are similar. Perhaps the biggest difference is in sampling design and choice of sampling material. Ramey collected a small number of samples from many locations throughout the range of each subspecies, while King collected a large number of samples from a fewer number of sample sites.

She went on to describe the issues related to sampling techniques, then answered the specific questions asked of the reviewers.

1) King used appropriate markers and collected an impressive amount of information from the samples. Their analyses were valid and quite thorough.
2) The data provided are notable in that they show strong and significant differences among subspecies. These data suggest a high level of genetic differentiation and no real evidence of gene flow among them. This result, however, may be influenced by the sampling design of the study. I feel, however, that King did do a thorough job of sampling within the *Z. h. preblei* subspecies and sampled reasonably well within *Z. h. campestris,* which were of primary interest to the study. I would concur that these are likely good subspecies.
3) The data provided in this report do show two distinct populations of *Z. h. preblei* that do not share any haplotypes.
4) King did find shared haplotypes within each of the putative subspecies (some sampling sites of which were very geographically distant) and the results from the STRUCTURE analysis really suggest otherwise.
5) King did an excellent job getting the most information out of the samples collected. Additional analyses of these samples would not, in my opinion, change the outcome.
6) I have highlighted several differences between the two studies above. The most striking issue about the comparison between these two studies, in my opinion, is that they are so different. Given the discrepancies between the two studies, I would not be supportive of changing the taxonomic delineations without strong evidence suggesting that the strong differentiation reported by King was merely an isolation by distance effect resulting from limited dispersal capability of *Z.hudsonius*.

It appears the reviewer is well versed in the work of many in related fields, much of the detail which I edited and defer to interested parties to investigate is available in the full text. But her feelings that more data related to those sources would be good and the jury was out in her mind moved dramatically to dismissing Ramey and support of King. Her current connection to USGS, the agency that FWS used in obtaining the King study makes me discount the value of her support of King and her lack of continuing interest in holding him to the same additional standards applied in her review of the Ramey works.

David J. Hafner, Ph.D, Chair, Science Division, New Mexico Museum of Natural History

Ramey et al. employ the appropriate methods, markers, evidence, and interpretation to convincingly argue that *Z. h. preblei* is not a valid subspecies, and should be synonymized under *Z. h. campestris*. I think that the ESU is an appropriate and useful genetic unit that should be employed by conservation agencies as well as phylogeographers. The study by Ramey et al. has several small editorial errors and reference omissions and the tone is unnecessarily ponderous, condescending, and preachy. However, I agree with all of the systematic and taxonomic conclusions, and would also encourage regulatory agencies to employ systematists to provide such systematic reviews wherever it is practicable. I think it's rather absurd to consider regulatory agencies to be responsible for supporting in-depth systematic studies of this sort for every taxon under consideration, but an accurate taxonomy a laudable goal.

While I support the taxonomic interpretations of Ramey et al., I disagree strongly with their implied conclusion that synonymy with *campestris* automatically translates into conservation security for the geographically expanded taxon. Yes, the expanded subspecies is "more common and widespread than previously thought," but that does not necessarily mean that the new taxon is secure, or that this represents a "misallocation of scarce conservation resources to populations that are not genetically or ecologically unique." Here Ramey went well beyond their data, and failed to consider the conservation status of *campestris*. Hafner and Yensen (1998) consider *preblei* to be Endangered (*note this is beyond the current listing status agreed to by FWS)* but

also consider *campestris* to be of concern *(from)* - Continuing decline, observed, inferred, or projected, in area, extent, and/or quality of habitat. Overgrazing and loss of riparian habitat has been implicated as the major deleterious impact on populations of *campestris* in Wyoming, South Dakota, and Montana. *NOTE: the current petition affects Preble's only, the population of which (and habitat range of) has been found significantly greater than at the time of the cited study.* Thus, although the expanded *campestris* enjoys a larger geographic range, it (including populations previously assigned to *preblei*) is of conservation concern throughout its range.

Later, May 20, 2005, Hafner recanted -

In my original review of the first of Ramey et al.'s reports, I generally agreed with the methods and taxonomic conclusions made by the authors, but disagreed strongly with their view of conservation implications of those taxonomic conclusions. I was also critical of the condescending and ponderous, "preachy" tone of the report. Subsequent to filing my review, I have been increasingly offended by Ramey's attempts to portray himself as some sort of champion of truth and right opposed to poor scientists entrenched in dogma. Ramey has also charged, in newspaper articles, that only two of the eight reviews of his work were "independent," and did not include my name among those "independent" reviews. I take personal exception to his implication that my review was in some fashion biased. Such a comment is unprofessional and uncalled for, and serves to underscore his grandstanding, soapbox approach to science. *OK, we get it, you are upset and in so stating in such a dramatic way, why would the reader conclude that your following comments were not as much revenge bent as serious and thoughtful comment on the actual work rather than an objection to style? Keeping any supposed slight to the scientific community out of the argument would better serve the purpose but, Hafner chose to let it get in the way of how material his comments might be and is equally unprofessional as any personal (or general) attack aimed in his direction by Ramey. He finally gets around to saying (admitting?) not only was he off point but now thinks his first review was incorrect. On the informed scientific level necessary for peer review, this seems a serious oops!* More to the point, the addition of samples of *Z. h. intermedius* samples to this study and the resulting addition of geographic and taxonomic perspective has revealed to me some basic errors in my initial review and, importantly, in the taxonomic as well as conservation conclusions of Ramey's reports. I now interpret the pattern of mtDNA haplotypes among these four western subspecies of *Z. hudsonius* to support retention of all four subspecies, including *Z. h. preblei* and *Z. h. campestris*. The most important and overriding consideration of both of these reports is that this is the application of mtDNA sequence data to *subspecies*, not to taxa of a higher level that have been reproductively isolated. Even in *species* that have been recently isolated reproductively, one would expect to find common, ancestral haplotypes during the time necessary to sort mtDNA haplotypes (referred to as "sorting errors"). Thus, the ancestral haplotypes shared among multiple subspecies of *Z. hudsonius* mean nothing, and one cannot simply dismiss subspecific differentiation based on the existence of shared mtDNA haplotypes between neighboring subspecies. Such sharing is *expected*. Based on mtDNA sequence data alone, recognition of subspecies (to my mind) becomes rather "squishy" and subjective. Furthermore, while Ramey et al. may preach against it, the fact is that it has always been more difficult to synonymize subspecies than to recognize them: *any* difference becomes a measure of differentiation, however slight. The challenge, then, is to establish a "benchmark" for comparison within the broader geographic group: if one recognizes a particular form as a valid subspecies, then any geographic group *more* differentiated must also be recognized, and those *less* differentiated may be considered as candidates for synonymy. It's not an exact science, no matter how much Ramey rail(*s*) against reality. *The unprofessional and personal attitude re: Ramey aside, this total about face is nearly* Amazing Grace *like in his saying, "Once I was lost, but now am found."*

Hafner then launches into a ramble of scientific reference that appears to reinvent the wheel regarding his initial comments on Ramey. Even if skilled in the area discussed, one would have to do some serious comparison and research into the sources cited to make sense of, let alone buy into his shift in opinion. It certainly sounds impressive, but a continuation of his inability to stick to the point in terms of muddying the central issue with comment on the other various species of mouse studied. He appears to miss the mark when it comes to a simple opinion on Preble's.
By the way, there are a number of omissions in the reports that would certainly have been picked up if this were reviewed for publication. *It was and they weren't.*

Marlis Douglas, Asst. Professor Biology, Colorado State University

General Comments:
As the title suggests, this is a comprehensive assessment of molecular diversity within and among subspecies of *Z. hudsonius*. It is a well-done, thoroughly planned and meticulously executed study. Molecular methods and analytical protocols are clearly explained and descriptions contain sufficient detail. Sampling is thorough, molecular approaches technically competent, and appropriate analytical procedures were applied. Genetic patterns were examined by a variety of approaches, and tested for congruence among data sets and methods. Findings are assiduously discussed and interpretations stem from results and do not extend beyond the data. Comparisons to a previous study are factual, impartial and straight-forward. Authors do a good job at illuminating potential reasons for discrepancies between theirs and the Ramey findings.

(1) Were appropriate methodologies and markers used? - Yes.
Sampling: A concerted effort was made to collect tissues from live-trapped specimens. Also, sample sizes are sufficiently large to appropriately reflect diversity found at microsatellite loci. Multiple approaches are used to examine genetic patterns, and results based on different methods are compared. Overall, results are consistent among different approaches, and where discrepancies occur, authors provide plausible explanations as to why this might be.

(2) Are authors' conclusion about taxonomic validity of *Z. h. preblei* and neighboring subspecies supported by the data presented in the report? –
Yes. The authors provide multiple lines of evidence in the form of different molecular markers and various analytical approaches to support their conclusions. Results and interpretations are indeed convincing. The data clearly show genetic differentiation among the five subspecies, and further reveal genetic substructure within two of these.

(3) Are author's conclusions that *Z. h. preblei* is comprised of at least two distinct population segments worthy of individual management supported by the data presented in the report? –
Yes. Multiple lines of evidence support this conclusion and lack of shared haplotypes for CytB between northern and southern populations. *This reviewer supports a further breakdown of preblei into two subspecies which the genetic differentiation and environmental variations in the habitat area do not seem to support. This gives credence to King's opinion, which seems to have little basis in the data quality provided, or prior comment by King.*

(4) Possible alternative interpretations: could such be drawn from the genetics data? If so, how likely are these possibilities?
Authors discuss alternative conclusions and provide explanations why some are more plausible than others. Thus, they evaluated those alternative interpretations that could be reasonably assumed, and supported their conclusions with fact-based arguments. One alternative

interpretation could be: Population substructure identified in *Z. h. preblei* could reflect clin*(ic)*al variation and might appear distinct due to insufficient sampling (i.e., only extremes of the cline were sampled and not intermediates). However, this is highly unlikely since sampling locations appear relatively equidistant and continuous along Front Range, as far as the patchy distribution of *Z. h. preblei* permitted. *Preble's has proven to be less patchy than this comment would indicate and one of the failures of the King study is that the primary critical habitat areas along the front range were not the focus of the study.*

(5) Additional analyses: are any needed to verify the study's assertions and why?
A comprehensive array of analyses was already employed, and data were analyzed in a variety of ways. A few minor aspects that could be considered (but are unlikely to change any conclusions).

(6a) Conflicting conclusions of Ramey et al. (2005) and King et al. (2006): What are most likely explanations?
King provide(*s*) a detailed discussion as to why findings/conclusions differ from the ones made by Ramey Most reiterate concerns I raised in my review of the reports provided to USFWS by Ramey. More samples and more data allowed King to employ are variety of analytical methods for the examination of evolutionary history and population structure of the study taxa. In turn, multiple lines of evidence were provided to support particular interpretations of findings, and underscore high probability of conclusions. *The DQA challenge questions the actual impact of the different sampling methods used.*

(6b) Does new information change conclusions regarding synonymizing of *Z. h. preblei* and neighboring subspecies?
Ramey provided insufficient data to draw conclusions about distinctiveness of *Z. h. preblei*. Thus, their findings neither supported nor rejected the notion of genetic distinctiveness of *Z. h. preblei* as a unique subspecies, and their suggestion to synonymize *Z. h. preblei* with neighboring subspecies went far beyond their data. In contrast, data provided by King document genetic differences, albeit shallow, between *Z. h. preblei* and other subspecies. In light of these new findings, the synonymization of *Z. h. preblei* with other subspecies is not warranted and cannot be recommended. *Meaning, I agree there is little proof that differences are significant but let's leave it as is anyway.*

Douglas A. Kelt, Professor of Wildlife Biology, University of California, Davis

In the spirit of full disclosure, recognize that I am an ecologist with particular emphasis on community to biogeographic scales. I am not a systematist although mammalian taxonomy is a hobby, and I am not strongly versed in contemporary methods of phylogenetics or molecular systematics.

The best scientific and commercial information available certainly gives reason to question the distinctiveness of Preble's mouse. However, I find myself agreeing with S. Oyler-McCance's comment that Ramey "provides a great data set from which to **begin** to answer the question at hand" From the data available, Ramey make(*s*) a strong argument that *Z. h. preblei* is not clearly distinguishable from other subspecies of *Z. hudsonicus*. Given this, the real questions at hand are whether or not the data available are adequate to make this assessment, and if the most prudent course of action is to accept this "best assessment" or, if these data are deemed insufficient, to defer until sufficient data are available such that the allocation of populations of *Z. h. preblei* is more clear. My personal preference would be to defer, and to urge scientists to address this issue more clearly and especially, in the peer-reviewed literature. One problem with the taxonomy of *Z. hudsonious* appears to be the fact that key papers or analyses have not been exposed to peer

review and subsequent scientific scrutiny. *No longer the case.* However, the PCA presented in Ramey convinces me of the lack of morphological differences.

I believe that the genetic and morphologic analyses are more convincing than ecological differences might be. I would place the greatest intellectual stock in the genetic analyses, and the least I *(in?)* the ecologic analyses, in determining the validity of a population as a unique evolutionary unit such as a subspecies or evolutionarily significant unit (ESU).

My general philosophy on issues of threatened species is that they should be given the benefit of the doubt until it is clear that they are not threatened. Because the "best available information" is somewhat limited, and because of the lack of virtually any ecological information, the lack of a more comprehensive morphological study, and the limited mtDNA sequence and absence of any nuclear DNA data, I believe that we should give the mouse the benefit of the doubt while time is available.

In contrast, the *cost of declining to delist* Preble's mouse can be measured primarily in terms of economic terms (i.e., limitations to use of lands considered to be Critical Habitat) and potentially in political capital (anger towards USF&WS and its personnel; increased public questioning of the Endangered Species Act and possibly political movement to dilute the Act). These are undoubtedly important considerations. The ESA was developed, however, to protect species under the seemingly interminable onslaught of habitat consumption by humans, and presumably it was hoped that this would protect species by setting aside lands. *This has less to do with the issue at hand than the larger, and now highly public, issue of ESA reform. Humans will continue to take up land as the population grows but a major concern is if setting aside land via use of the act, when applied to private property, violates the 5th Amendment rights of land owners.*

Available genetic analyses do not support recognizing *Z. h. preblei* as a distinct entity. Morphological analyses presented by Ramey) concur with the genetic analyses. Ecological data are largely lacking, but given the similarities among all *species* of *Zapus* in habitat selection, diet, etc., it seems highly unlikely that *Z. h. preblei* will be found to differ ecologically from other subspecies of this species.

That more reviewers were generally favorable towards Ramey than were critical of it does not avoid the fact that some very serious limitations have been noted by some reviewers,

In summary, however, the available morphological and genetic data provide no strong support for recognizing *Z. h. preblei* as a taxonomic entity, and I believe they suggest that this taxon is indeed not distinct from other populations of *Z. hudsonius*. Consequently, I would support delisting *Z. h. preblei* under the current circumstances.

It appears that this reviewer has concerns about surrounding issues but accepts Ramey as the best available science. His comments indicate a desire to not list (or continue to do so) species of questionable scientific background. Take them off the list and if those concerned wish to continue to seek a better answer, cross that bridge when it appears.

Dr. Hopi Hoekstra, Assistant Professor of Biology, UC San Deigo

I commend the authors both for the hypothesis-testing approach and the integration of genetic and morphological data. While I think the report raises several interesting questions about the "uniqueness" of *Zapus hudsonicus preblei*, I also have several concerns. In particular, additional data is necessary to confirm and/or refute these preliminary results.

The details of the procedures used here are needed, including specific explanations as to how contamination was controlled and checked. It would not be un-reasonable to ask that a second lab (which does not work on *Zapus*) to repeat these results.

She did not support, but listed three further areas of study as necessary to conclude otherwise

Dr. Wayne D. Spencer, Senior Conservation Biologist, Conservation Biology Institute, San Diego

Dr. Spencer has a BS in Biology, MS in wildlife management & PhD in ecology and evolutionary biology

Yes, the weight of scientific evidence I reviewed appears to support that Preble's meadow jumping mouse (*Z. h. preblei*) is not a distinct subspecies and appears to be synonymous with *Z. h. campestris*. However, I share concerns raised by others about over-extrapolation of some conclusions in the principle scientific report cited to support this finding Nevertheless, I must conclude, based on a balanced review of all available evidence, that the *taxonomic* conclusion of Ramey is probably correct.

Although I find the taxonomic conclusion in the proposal likely sound (that *Z. h. preblei* and *Z. h. campestris* are one subspecies) taxonomy alone does not determine conservation value of species populations. I strongly endorse the stated intentions of the Service that, prior to any final, affirmative decision on delisting *Z. h. preblei*, the Service will evaluate (1) the status and threats to the combined *Z. h. campestris* entity in all or a significant portion of its range, and (2) the potential for the Preble's portion of *Z. h. campestris* range to qualify as a DPS. I urge completion, *and publication,* of nuclear DNA analyses. Although, following the letter of the law, delisting based on available information may be warranted, it does not seem at all prudent given the very real uncertainties about potential adaptive variation among populations and its potential importance to the continued existence of the subspecies (especially in light of climate change). Delisting could do irrevocable harm pending results of further analyses.

The ongoing theme of these reviews is that it may be good work but, there is never enough work done to lead to either consensus or conclusion. There is the appearance that even those who find the work compelling and certainly a higher level of science than that leading to the listing, there is a subconscious – sometimes stated – desire to go outside the scope of the questions and hypothesize on various non-scientific aspects or the potential result of delisting and the academicians desire to seek more knowledge instead of just admitting it probably is a duck. Even if they concur about the duck, there is the seemingly never ending desire to make sure they know what version of a duck it is. And, there is rarely agreement in peer reviews. This is the situation leading to the comment by - - - - ? that, "If you torture science long enough, it will give whatever answer you desire." Supportive comment is couched in terms that indicate a professional minefield, that the writers are hesitant to tread, exists to a degree that those in the field will become pariahs in the green community. This in fact was reported in *The Wall Street Journal* regarding Dr. Ramey's possibly forced departure from his position at the Denver Museum of Nature & Science. Then, when he was hired to consult for a DOI agency, held out in local press as having sold out the mouse and used his report as springboard to a job. FWS responded to these calls for more study by having King do more work, ending in a statistically immaterial result that was equally dissected by peers without significant shift in participant opinion, and causing questions about the way the delisting process was manipulated. This caused the King study to become the subject of a data quality challenge which may prove to drag the decision time well into year three.

Carron Meaney, Ph.D., Research Associate, Denver Museum of Natural History, and Curator Adjoint, U C Museum

I find this paper very clear in its presentation, use of hypothesis-testing, and overall good science. The evidence suggests that *Zapus hudsonius preblei* is not genetically or morphologically distinct from *Z. h. campestris*.

The morphometric data and analysis appear solid. Also, it was recently brought to my attention that a more recent study of the systematics and biology of the genus *Zapus* found insufficient morphological evidence to support subspecific status for *Z. h. preblei.*

Jeffry B. Mitton, Professor, Ecology and Evolutionary Biology, University of Colorado

Two important results emerge from these studies:
1) the haplotypes detected in *Z. h. preblei* are a subset of the haplotypes in *Z. h. campestris*--that is, the samples of *Z. h. preblei* did not reveal any unique haplotypes;
2) a discriminant function of skull measurements could only correctly classify 48% of the individuals to their correct subspecies—about the percentage (50%) that could be correctly assigned by random guessing.

This reviewer agrees. The most parsimonious assumption is that *Z. h. preblei* is simply an arm of the distribution of *Z. h. campestris*, and therefore contains a subset of the variation in *Z. h. campestris*.

To the specific question #1 asked - Yes, appropriate markers and methods were used. He was not asked (or did not respond) to the King study.

David J. Hafner, Ph.D, Chair, Bioscience Department, New Mexico Museum of Natural History

King et al. have successfully addressed all of the criticisms directed at the study design, scope of analysis, and analytical techniques that were directed at Ramey. In doing so, they have produced strong and conclusive results that fully contradict those of Ramey. King demonstrates not only that *Zapus hudsonius preblei* is a distinct subspecies, but also that there are two genetic units within *Z. h. preblei* that probably warrant separate conservation status. I am very impressed with the comprehensive nature of this study, and fully support the conclusions and interpretations.

In reviewing the history of the Ramey et al. study, I note that most of the reviewers of the initial report (including me) agreed in general with the techniques employed and taxonomic conclusions. At that time, most of the disagreement dealt with the conservation interpretations of those taxonomic conclusions, as well as the condescending tone of the paper. Indeed, it was probably the unnecessarily insulting tone of the paper as much as the conservation implications that encouraged closer scrutiny of the genetic techniques employed by Ramey et al.

All of these criticisms, as well as those directed by better geneticists were provided on reviews of the report by Ramey et al. These criticisms were in regard to limitations of skin samples for sequence analysis; the low number of base pairs and, subsequently, microsatellites employed; the overly stringent criteria; and the incorrect application of various statistical tests. Ramey et al., who submitted the manuscript to *Animal Conservation* for publication without modification,

ignored all of these important criticisms. I consider it a failure of the review process of *Animal Conservation* that such a flawed study was accepted for publication.

Ramey represented, in my opinion, a perversion of the scientific method and the peer review process. After its publication, I heard charges that the USFW was somehow guilty of collusion with Ramey, and that the paper's publication would be taken on face value by the USFW as proof that it was the "best available science." I'm proud of the USFW for seeking a resolution to the scientific debate by funding this comprehensive study by a federal laboratory. I suspect that Ramey, who describes himself as a "conservation genetics expert," will continue to declare that he is the only honest voice in the wilderness, and that all of his critics are politically motivated. In view of the fact that Ramey has been awarded by the current federal administration with a DOI contract as a "genetic consultant" and has announced his intention of targeting additional taxa for delisting, his motivation is quite clear. I may have been mistaken when I assumed that Ramey merely represented shoddy science and stubbornness. The vast political and financial pressures that fuel the debate between an endangered subspecies of jumping mouse and developers should never influence the objective science of the issue.

6. *What are the most likely explanations for the conflicts between the conclusions of Ramey et al. (2005) and King et al. (2006)*?—I agree with the explanations provided by King et al. Ramey et al. used a poor sampling strategy, unrealistic criteria, inadequate sequence length, and an inadequate number of microsatellite loci. In addition to the lack of quality assurance/quality control efforts pointed out by King et al., I would add that Ramey et al.'s study most likely suffered from cross-contamination of laboratory samples (not uncommon in the sequencing of "rare" DNA). Basically, in my considered opinion, Ramey is sloppy science. If the peer reviews received by *Animal Conservation* were anything like Hafner: review of King et al., 14 February 2006 p. 3 the reviews that Ramey et al.'s report to the USFWS received, it should never have been published. In effect, *Animal Conservation* failed to protect Ramey et al. from themselves. This is a good reminder to the USFWS that

Yet, his review of the original work by Ramey stated –

Ramey et al. employ the appropriate methods, markers, evidence, and interpretation to convincingly argue that *Z. h. preblei* is not a valid subspecies, and should be synonymized under *Z. h. campestris*. I think that the ESU is an appropriate and useful genetic unit that should be employed by conservation agencies as well as phylogeographers. The study by Ramey has several small editorial errors and reference omissions and the tone is unnecessarily ponderous, condescending, and preachy. However, I agree with all of the systematic and taxonomic conclusions, and would also encourage regulatory agencies to employ systematists to provide such systematic reviews wherever it is practicable. I think it's rather absurd to consider regulatory agencies to be responsible for supporting in-depth systematic studies of this sort for every taxon under consideration, but an accurate taxonomy a laudable goal.

But, even while agreeing with the Ramey study, he questions any real value of such efforts by FWS and shows a tendency to focus on style instead of substance, continuing into his review of King. I wonder why his opinion is sought at all, given the result.

Dr. Jesus E. Maldonado, Genetics Program, National Zoological Park, Smithsonian Institution

7) Has this new information changed your conclusions regarding the synonymizing of *Z. h. preblei* and *Z. h. campestris* and neighboring subspecies? Please elaborate as necessary.

After reviewing the first report by Ramey et al. I was not fully convinced that they had gathered enough information to support their conclusions. I stated, *"While I support the taxonomic interpretations of the authors based on the data they presented, I would strongly suggest that they consider analyzing microsatellite data to corroborate their findings."*

Aside from the usual let's do more study theme, it should be noted that Maldonado supported the initial Ramey work and did not review the report used in the delisting petition process or the published version with further peer review. Yet, he changed his opinion after reviewing King despite his initial comments on the first Ramey report.

1) Analyze the techniques used etc.

The statistical techniques used are appropriate however; since the authors made the *a priori* decision to group *preblei* and *campestris* then the test is limited to a two-group comparison. I suggest that if groupings have to be decided *a priori* that different combination of groupings (populations) besides subspecies be tested. The methods for population genetic and phylogenetic analysis appear adequate.

2) Based on the data presented in the report do you support the author's conclusions?

While I support the taxonomic interpretations of the authors based on the data they presented, I would strongly suggest that they consider analyzing microsatellite data to corroborate their findings.

3) Based on the MDIV data do you view *Z. h. preblei* and *Z. h. campestris* as a single connected population?

The MDIV analysis of the mtDNA data strongly suggests that there is gene flow between *Z. h. preblei* and *Z. h. campestris*. All of the haplotypes present in *Z. h. preblei* are also found in populations considered to be *Z. h. campestris*.

4) Are there possible alternative interpretations of the data? How likely are these possibilities?

This study uses museum specimens for most of the sampling. While in the report, little mention was done as to the precautions that are commonly taken when dealing with ancient DNA, I suspect that the authors are well aware of the problems with contamination that are magnified when dealing with ancient DNA samples. I should point out that all of the *Z. h preblei* haplotypes were also found in *Z. h. campestris* samples Although the possibilities that these haplotypes are all the product of contamination is low and very unlikely if the investigators have a Laboratory facility that is equipped to deal with problems with ancient DNA extraction and processing.

5) What additional analysis, if any, is needed to verify the study's assertions and why?

I would strongly recommend that they add microsatellites to their study. Of course it would be ideal to have more data than just genetics and morphology and in particular more detailed ecological, physiological, behavioral, and geographic and habitat data.

6) **Has this new information changed your conclusions regarding the synonymizing of *Z. h. preblei* and *Z. h. campestris* as proposed in Ramey et al. 2004a? Please elaborate as necessary**.

I think that the addition of the *Z. h. intermedius* samples and the additional morphometric and genetic analysis has made this a stronger and more interesting study. However, I feel that evidence from additional genetic markers (i.e. microsatellite data) is needed to confirm their conclusions. Although these markers are not likely to provide a signature of substantial

evolutionary subdivision they might allow one to detect finer-scale population.
I note that Maldonaldo did not review (or if so it was not posted in the reviews provided on the FWS site) 2004a and his final comment is hard to evaluate on that basis.

Dr. Lisette Waits, Associate Professor, Department of Fish and Wildlife, Univ. of Idaho

I will start by addressing the following questions and then add two additional comments at the end of review.

Question 1:
Yes, I think that the authors provide convincing evidence for synomyzing these two subspecies since the hypothesis testing did not reject the hypothesis that the two are essentially the same for the morphological and genetic data.

Question 2:
I can support synonomyzing subspecies without additional microsatellite data.

Question 3:
This question is really philosophical and depends on what species/subspecies definition one accepts. I think genetic data can provide important information for subspecies classifications but I do not think it is the only data that should be used. I feel that subspecies classifications can be justified based on substantial morphological, ecological, or behavioral differences even if mtDNA data do not demonstrate long-term separation.

Question 4:
Detailed field studies or microsatellite analysis (preferred approach) will be necessary to address this question.

Question 5:
This is a difficult question. If we take only a mtDNA diversity perspective then the answer is no. I do not see any evidence of unique biological or ecological characteristics but I am not certain this has been thoroughly evaluated for *preblei*. Because of potential recent isolation (within the last 5,000 yrs) of this population, it may be on a unique evolutionary trajectory that might have future importance under Waples definition of evolutionary legacy.

Other General Comments:
1) When evaluating whether preble's would qualify as an ESU the authors do not apply Waples definition. Since this definition was cited by NMFS/USFWS in the 1996 joint policy that addresses ESUs, I think it would be important and useful to apply Waples' ESU definition.
2) On the bottom of page 9, the authors state that based on the Crandall approach the two species would be considered a single population for management purposes. I think it is a premature overstatement to conclude this without microsatellite data.

Dr. Brett R. Riddle, Professor of Biological Sciences, University of Nevada Las Vegas

1. Do the data support conclusions that *Z. h. campestris* and *Z. h. preblei* should be synonymized?
I believe that the data support a lack of substantive morphological, ecological, and molecular differentiation between these two subspecies. This is not surprising, and in fact is a very common outcome of molecular analyses of taxonomic subspecies that are in close geographic proximity, are ecologically similar, and appear to have no surmounting biogeographic obstacles to movements across an historical landscape.

2. Are additional genetics studies required?
Two potential problems are associated with the use of a single genetic marker: it might not reflect "overall" evolutionary affinities between lineages due to sorting or to introgression effects; and it might not be evolving rapidly enough to capture an evolutionary distinction between lineages. The first issue is not likely to be a serious problem in this case. The second issue is also not likely to be an issue, because while microsatellites or SNPs might allow one to detect finer-scale population structure than mtDNA, they are not likely to provide a signature of substantial evolutionary subdivision at the level of taxonomic subspecies if the mtDNA did not do so. *So, these are non-issues?*
3. What is the importance of ecological, behavioral, or physiological differences between subspecies in supporting or refuting synomy?
I have little to say about this issue, in part because I doubt that one would find interesting biological differences between populations representing the different, and in part because, without a genetic signature of historical evolutionary separation between lineages, I would not support recognition of taxonomic distinctness based solely on ecological, behavioral, or physiological traits.
4. What is the likelihood that *preblei* is substantially reproductively isolated from *campestris*?
I see neither a genetic nor a biogeographic reason to predict that populations within these two subspecies are reproductively isolated from one another. Note that reproductive isolation has never been a criterion employed by mammalogists to recognize distinct taxa at the level of subspecies, so the question actually is not relevant to the issue of a substantial and recognizable history of isolation and divergence between populations.
5. Would loss of *preblei* represent a substantial diminution of *campestris* range, biology, or evolutionary legacy?
I suspect that populations of what is now considered *preblei* represent a substantial and important portion of the overall viability of what is now considered *campestris*. My opinion has to do with the fact that both subspecies represent a set of disjunct, peripheral populations at the western edge of the range of the species. Macroecologists identify core vs. peripheral ecological characteristics of species ranges, and these include, for example, a core to peripheral decrease in overall quality and continuity of habitat; and thus a core to peripheral decrease in overall population connectivity, population abundance, and population viability. I suspect that both subspecies represents, in large part, a set of populations that are disconnected from one another, restricted to small, discrete and isolated microhabitats surrounded by decidedly non-*hudsonius* habitats, and thus quite vulnerable to local extinctions without the possibility of rescue via dispersal from surrounding populations. Nevertheless, because of their peripheral isolation t perhaps, a subset of what might be considered as ecologically and physiologically extreme environments for the species, these populations could well contain a set of ecological traits that have selective advantage in extreme environments and therefore are unique and interesting (perhaps irreplaceable) within the context of the species as a whole. As such, even if the two subspecies are taxonomically synonymized, I would strongly urge agencies and recovery teams to regard all remaining populations as potentially valuable within a recovery plan.

Final comment

While I see no reason to support an opinion that *preblei* and *campestris* should be retained as separate taxonomic subspecies, I believe that there is still a case to be made for considering the collective set of populations originally represented as separate subspecies as an evolutionary lineage of conservation concern.

What this study has done, in my opinion, is not reduced the level of conservation concern for a set of vulnerable populations along the western periphery of the species' distribution, but rather, established that the issue of evolutionary distinction needs to be addressed at a different geographic and sampling scale.

Everything is a work in progress; there is never a final answer. We can list but not delist, we can accept new species but not the traditional decline of old ones. We reject evolution and chose to blame man for the changes in or elimination of species. We embrace the idea that man is more powerful than whatever Gods may be, despite even several species of hominid having gone the way of the dinosaurs without giving credence to the idea that today's most (if incorrectly) heralded ESA success is an evolutionary descendant of those long gone reptiles, the Bald Eagle. In fact, it is all in progress but maybe we should accept Darwin rather than take the arrogant position that the current, likely temporary, version of Homo can save everything else in a world we barely understand.

The issue to this writer is that few casual, non-credentialed supporters of conservation are likely to deeply investigate the type of dialogue presented in the process of peer review and public comment. But, as a supporter of using limited resources to support truly recoverable species while also considering a multitude of needs such things protecting/maintaining/improving National Parks, wilderness areas and Federal lands as a general method of aiding the same goals I submit that this process and those that participate in it may well be considered the comments of self important, or at least narrowly focused reviewers that may have personal agendas and, little ability to say yes or no. Further, their livelihood may be impacted by taking a stand. This does not serve the species any more than the "non-profit" funds gathered from the morally correct, but detail uninformed, donators to litigation slush funds used to stir the pot, but never finish the stew.

The goal of making room in the world for ecological and environmentally significant flora and fauna species and aiding them in surviving various onslaughts by man or nature is laudable. But, historically it is futile. Not everything that evolves is destined to survive millenniums and rather than support litigation to save everything, I would prefer to spend limited resources on those that have the most impact. And, it isn't just the job of our country. We have many other areas where the resources available can be expended, including issues other than tossing $400 a year at a species that was lawyered onto a list that can't be financially supported. When I ask the casual observer if they support the ESA, the answer is generally yes. If I ask those who have been deeply effected/involved in the processes needed to overcome the regulations as it impacts their own property, or follow up with the question, "Can we afford a million dollars a year to protect a mouse in a single county?", the answer inevitably is not just no, but, "That's stupid."

WYOMING FARM BUREAU FEDERATION
P.O. Box 1348
Laramie, Wyoming 82073 • (307) 745-4835

March 31, 2006

US Fish and Wildlife Service
Field Supervisor
Colorado Field Office
Ecological Services
P. O. Box 25486
Denver Federal Center
Denver, Colorado 80225

RE: Preble's Meadow Jumping Mouse, Proposed Rule, reopening of comment period (FR Vol. 71, No. 33, February 17, 2006)

Wyoming Farm Bureau (WFB) appreciates the opportunity to comment on the referenced Proposed Rule and the re-opening of the comment period on the proposal to de-list the Preble's Meadow Jumping Mouse (PMJM). Here we are eight years after listing wondering if the entire *hudsonius* cohort of *campestris* and *preblei* will be listed. This question is illogical, especially considering the USGS biologist (Cryan 2005) was able to capture dozens of specimens at a variety of locations for the King et al. genetic analysis. If this mouse was truly in need of ESA protection such capture numbers would be out of the question.

WyFB supports the petition filed by the State of Wyoming for the delisting of *Zapus hudsonius preblei*. We are disappointed that the Service continues to be side tracked by the genetics controversy. The genetics discussion comprises a very small portion of the information found within the State of Wyoming's de-listing petition; rather that body of work expands on and informs the distribution, abundance and threats to the PMJM. As documented in the petition, the PMJM has been found across a much wider geographic range than previously known (17 hydrologic units vs. 9, at the time of listing). It has also been found in (1) substantially greater numbers than previously thought, (2) virtually all the historic capture locations (with the exception of the greater Denver metropolitan area) and (3) it is not at risk from the majority of threats provided in the decision to list.

We will not attempt to comment on the technicalities of the genetics study designs or methods used by either Dr. Ramey or Dr. King. We are able to comment on the delisting petition as a whole and to inform the DAT discussion, as requested in the federal register notice.

In the Federal Register Notice the Service specifically requested information regarding the taxonomic status of the various *Z. husdonius* subspecies (question #1). The information the Service needs on this subject is already in hand. The unpublished report cited on the FWS web page (Cryan 2004) points to the wide variation within and between subspecies of *Z. hudsonius* and the lack of agreement over the years between the various researchers in attempting to decide if there are three species of *Zapus* or if there are eleven subspecies of h*udsonius.* The one thing that remains consistent is the lack of agreement regarding the genetics of the genus, species and subspecies. This is the classic debate between "lumpers" and "splitters". Given the physical numbers of individual jumping mice that have been trapped and the new populations identified in the northern Rocky Mountains within the range of *preblei* and *campestris,* there is no support for the continued listing of *Zapus hudsonius.* This situation has not changed since trapping efforts began in earnest following listing in 1998.

We have concerns regarding the King et al. 2006 genetics work. We, along with Dr. Armstrong (see the King peer reviews on the FWS web page) and others, are disturbed by the lack of Wyoming specimens included in the King analysis. Given the controversy over the genetics of *preblei/campestris* and the petition filed by the state of Wyoming, we find the exclusion of Wyoming mice from the analysis suspect. The Service has stated that the rigorous nature of the King study required substantial numbers of mice from the same location and as there are no such numbers of Wyoming specimens available they could not be included in the study. We question if this is the real reason for not including the Laramie Range mice in the King analysis. Numerous trapping events have occurred over the last 6 years in the Laramie Range. The Laramie Range is a relatively small geographic area, given the relative proximity of these collection sites the argument could certainly be made that these collections together satisfied the "rigorous" requirements of the King study. The "rigorous" nature of the study did not appear applicable to the collection of *campestris* or *luteus,* both of which were gathered from a variety of sampling sites in a larger overall area, as illustrated in Cryan (2005) and Table1 of King et al. (2006). We question how hard the USGA worked to obtain permission to trap *Zapus* in southeastern Wyoming in-conjunction with Cryan's work.

It further appears that the majority (+80%) of the "Preble's" genetic material used in King 2006 was from populations of *preblei* from the southern extremes of the range (El Paso and Douglas Counties, Colorado) for comparison to *campestris* collected approximately 400 miles to the north, in Crook County, Wyoming. The remaining 20% of the *preblei* genetic material came from Larimer County, Colorado and is called the northern cohort in the King et al. (2006) paper. King states there is enough genetic difference between the southern and northern populations of *preblei* (as he defined them) to consider them differently from the stand point of conservation needs. Of course there is a genetic difference, the populations studied are 400 miles apart and would have limited genetic interchange!

The failure by King to include the Laramie Range cohort of mice creates a significant gap in the understanding of the genetics of *preblei* and *campestris.* If King had looked at these mice, as was done in the Ramey et al (2004) analysis, he may also have found genetic similarity between the Laramie Range strain of jumping mice and those in the Black Hills. Such genetic similarity is likely the result of habitat connectivity illustrated in the Wyoming Natural Diversity Database (WYNDD) model. The mice identified from the Big Horn Mountains of Wyoming were also not included in the King analysis even though they could shed light on the habitat and genetic connectivity of *Zapus* between the Laramie Range and the Black Hills. Connectivity through the Big Horn Mountains is a critical question which the Service refuses to address even after many requests to do so.

Dr. King makes recommendations for future conservation of *preblei* and *campestris* as a result of his genetic findings. It is completely inappropriate to base conservation recommendations on a single factor such as genetics. The Endangered Species Act includes five listing factors, not one.

The Service specifically requested information (question #5) regarding the threats faced by *preblei, campestris* and other *hudsonius* subspecies. As provided in the delisting petition, the threats faced by *Zapus* in the northern Rocky Mountains are minimal and, as stated in Cryan (2004), riparian habitats are expanding, likely increasing the available habitat and with it the populations of *Zapus.* The 4-d rule currently in place also provides ample evidence that the threats to the genus from agricultural operations and other human activities are not significant.

Given the ease with which USGS personnel (Cryan 2005) were able to capture large numbers of *campestris* for the King et al. 2006 genetics study, there is no support for statements and concerns that this subspecies is at risk. Cryan also states that there are too few data upon which to make meaningful comparisons of relative abundance or population trends among the subspecies of *Z. hudsonius*, and there is no reliable information to determine if populations have increased or declined over the past century. It appears, not surprisingly, that drought plays an important role in the annual population dynamics of the genus. Based on these statements by Cryan, the Service cannot justify maintaining the listing of any subspecies of *Z. hudsoiniius*.

The Service asks for additional information (question #7) regarding possibilities of connectivity between the subspecies of *hudsonius*. Cryan 2004 points to the gap in the distribution between *campestris* and *preblei* across the Powder River Basin as reason to continue the distinction between the two subspecies or to consider the Preble's as a Distinct Population Segment (DPS) of *campestris*. Again, the Service fails to look at the information provided in the de-listing petition which details the connectivity of habitat from the North Platte drainage to the Tongue River and east to the Black Hills and the Belle Fourche River system, via the Big Horn Mountains. This connectivity is further supported by *Zapus* captures in Fremont and Johnson Counties of Wyoming. The WYNDD *Zapus* habitat model used in this discussion was funded and embraced by the FWS.

Further, we point out that the Service has adopted the common name of the Bear Lodge Jumping Mouse for *campestris*. This common name was inappropriately adopted from the Biodiversity Associates comments against the delisting petition. The proper common name for *campestris* would be that provided by Edward Preble in 1899, the prairie jumping mouse.

Sincerely,

Ken Hamilton

Executive Vice President

References Cited

Cryan, P.M. 2004. Synthesis of existing information on meadow jumping mice (Zapus hudsonius) in the northern Great Plains. Unpublished report prepared for U.S. Fish and Wildlife Service, Denver, Colorado.

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King, T.L, J.F. Switzer, C.L. Morrison, M.S. Eackles, C.C. Young, B. Lubinski, and P.M. Cryan. 2006. Comprehensive analysis of molecular phylogeographic structure among meadow jumping mice (*Zapus hudsonius*) reveals evolutionarily distinct subspecies. A report submitted to the U.S. Fish and Wildlife Service. January 25, 2006.

Ramey, R.R., H.P. Liu, and L. Carpenter. 2004. Testing the taxonomic validity of Preble's meadow jumping mouse (Zapus hudsonius preblei). Report to the Governor of Wyoming and the U.S. Fish and Wildlife Service (revised). 27 pp.

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March 31, 2006

US Fish and Wildlife Service
Field Supervisor
Colorado Field Office
Ecological Services
P. O. Box 25486
Denver Federal Center
Denver, Colorado 80225

RE: Preble's Meadow Jumping Mouse, Proposed Rule, reopening of comment period (FR Vol. 71, No. 33, February 17, 2006)

True Ranches appreciates the opportunity to comment on the referenced Proposed Rule and the re-opening of the comment period on the proposal to de-list the Preble's Meadow Jumping Mouse (PMJM). As the Fish and Wildlife Service (FWS or Service) is well aware, True Ranches has been an active participant in the effort to define the range of the PMJM, in illustrating the expanded range and in the discussion of the threats, or lack there of, facing the subspecies.

True Ranches supports the petition filed by the State of Wyoming for the delisting of *Zapus hudsonius preblei.* We are disappointed that the Service continues to be side tracked by the genetics controversy. We have stated from the beginning of our involvement in 1999 that it does not matter what the mouse is called; *Zapus* are found throughout their historic range and beyond. The Service has never concentrated on the population ecology of the mouse, instead the Service was concentrating on genetics; first, the question was the differences between *princeps vs. hudonius.* Now the Service is fixated on *preblei vs. campestris or intermedius*); it does not matter. There are more mice in more locations than previously known and the threats originally listed by the Service are not valid. The Service is chasing the same rabbits down different holes to avoid the substantive delisting issues of distribution, abundance and threats (DAT).

We are not in the position to comment on the technicalities of the genetics study designs or methods used by either Dr. Ramey or Dr. King. We are able to comment on the delisting petition as a whole and to inform the DAT discussion, as requested in the federal register notice.

The genetics discussion comprises a very small portion of the information found within the State of Wyoming's de-listing petition; rather that body of work expands on and

informs the distribution, abundance and threats to the PMJM. As documented in the petition, the PMJM has been found across a much wider geographic range than previously known (17 hydrologic units vs. 9, at the time of listing). It has also been found in (1) substantially greater numbers than previously thought, (2) virtually all the historic capture locations (with the exception of the greater Denver metropolitan area) and (3) it is not at risk from the majority of threats provided in the decision to list.

In 1999 M. Jennings, from the FWS Wyoming Field Office, stated that she was pleased that the mouse was recovering. This statement was made after being notified of yet another capture of PMJM at a previously un-trapped location on True Ranches property. The PMJM was never at risk across the greatest part of its range, including southeastern Wyoming, and was inappropriately listed. Here we are eight years after listing wondering if the entire *hudsonius* cohort of *campestris* and *preblei* should be listed. This question is illogical; especially considering the USGS biologist (Cryan) was able to capture dozens of specimens at a variety of locations for the King et al. genetic analysis. If this mouse (whichever subspecies moniker we choose to apply to it) was truly in need of ESA protection such capture numbers would be out of the question.

In the Federal Register Notice the Service specifically requested information regarding the taxonomic status of the various *Z. husdonius* subspecies (question #1). The information the Service needs on this subject is already in hand. The unpublished report cited on the FWS web page (Cryan 2004) points to the wide variation within and between subspecies of *Z. hudsonius* and the lack of agreement over the years between the various researchers in attempting to decide if there are three species of *Zapus* or if there are eleven subspecies of h*udsonius.* The one thing that remains consistent is the lack of agreement regarding the genetics of the genus, species and subspecies. This is the classic debate between "lumpers" and "splitters". The Service fails to look further than the genetic confusion and appears to use that as a reason not to act on the remainder of the information found in the delisting petition. Given the physical numbers of individual jumping mice that have been trapped and the new populations identified in the northern Rocky Mountains within the range of *preblei* and *campestris,* there is no support for the continued listing of *Zapus hudsonius.* This situation has not changed since trapping efforts began in earnest following listing in 1998.

We have concerns regarding the King et al. 2006 genetics work. We, along with Dr. Armstrong (see the King peer reviews on the FWS web page), are disturbed by the lack of Wyoming specimens included in the King analysis. Given the controversy over the genetics of *preblei/campestris* and the petition filed by the state of Wyoming, we find the exclusion of Wyoming mice from the analysis suspect. We have been told by the Service that the "rigorous nature of the study required substantial numbers of mice from the same location" (M. Jennings pers. com.) and as there are no such numbers of Wyoming specimens available they could not be included in the study. We question if this is the real reason for not including the Laramie Range mice in the King analysis. Some of the collections listed in King et al. (2006) were not in excess of 30 mice captured within a short distance of each other. We remind the Service that True Ranches personnel collected thirty-three *Zapus* from a relatively small geographic area of southeastern Wyoming in the summer of 1999 and that tissue samples were

collected from these mice specifically for use in future genetics work. Tissue from *Zapus* collected on the Medicine Bow National Forest over a period of years is also available. Given the relative proximity of these collection sites (all from east facing drainages of the Laramie Range) the argument could certainly be made that these collections together satisfied the "rigorous" requirements of the King study. The "rigorous" nature of the study did not appear applicable to the collection of *campestris* or *luteus,* both of which were gathered from a variety of sampling sites in a larger overall area, as illustrated in Cryan (2005) and Table1 of King et al. (2006). We question if any effort was made to obtain permission to trap *Zapus* in southeastern Wyoming in-conjunction with Cryan's work.

It further appears that the majority (+80%) of the "Preble's" genetic material used in King 2006 was from populations of *preblei* from the southern extremes of the range (El Paso and Douglas Counties, Colorado) for comparison to *campestris* collected approximately 400 miles to the north, in Crook County, Wyoming. The remaining 20% of the *preblei* genetic material came from Larimer County, Colorado and is called the northern cohort in the King et al. (2006) paper. King states there is enough genetic difference between the southern and northern populations of *preblei* (as he defined them) to consider them differently from the stand point of conservation needs. Of course there is a slight genetic difference, the populations studied are 400 miles apart and would have limited genetic interchange!

The failure by King to include the Laramie Range cohort of mice creates a significant gap in the understanding of the genetics of *preblei* and *campestris.* If King had looked at these mice, as was done in the Ramey et al analysis, he may also have found genetic similarity between the Laramie Range strain of jumping mice and those in the Black Hills. Such genetic similarity is likely the result of habitat connectivity illustrated in the Wyoming Natural Diversity Database (WYNDD) model. The mice identified from the Big Horn Mountains of Wyoming were also not included in the King analysis even though they could shed light on the habitat and genetic connectivity of *Zapus* between the Laramie Range and the Black Hills. Connectivity through the Big Horn Mountains is a critical question which the Service refuses to address even after many requests to do so.

Dr. King makes recommendations for future conservation of *preblei* and *campestris* as a result of his genetic findings. It is completely inappropriate to base conservation recommendations on a single factor such as genetics. The Endangered Species Act includes five listing factors, not one. Again, we urge the Service to review the delisting petition as a whole. The species is not served by the Service extracting the most confounding segment (genetics) of the de-listing petition and becoming fixated on that. Only by setting politics and personal agendas aside and looking at the petition on its face will the Service clearly see that there is no justification for the continuation of the listing of *Z. hudsonius preblei.*

The Service specifically requested information (question #5) regarding the threats faced by *preblei, campestris* and other *hudsonius* subspecies. As provided in the delisting petition, the threats faced by *Zapus* in the northern Rocky Mountains are minimal and, as stated in Cryan (2004), riparian habitats are expanding, likely increasing the

available habitat and with it the populations of *Zapus.* The 4-d rule currently in place also provides ample evidence that the threats to the genus from agricultural operations and other human activities are not significant.

Given the ease with which USGS personnel (Cryan 2005) were able to capture large numbers of *campestris* for the King et al. 2006 genetics study, there is no support for statements and concerns that this subspecies is at risk from anything, except perhaps overly exuberant federal trapping efforts. Cryan also states that there are too few data upon which to make meaningful comparisons of relative abundance or population trends among the subspecies of *Z. hudsonius*, and there is no reliable information to determine if populations have increased or declined over the past century. It appears, not surprisingly, that drought plays an important role in the annual population dynamics of the genus. Based on these statements by Cryan, the Service cannot justify maintaining the listing of any subspecies of *Z. hudsoiniius.*

The Service asks for additional information (question #7) regarding possibilities of connectivity between the subspecies of *hudsonius.* Cryan 2004 points to the gap in the distribution between *campestris* and *preblei* across the Powder River Basin as reason to continue the distinction between the two subspecies or to consider the Preble's as a Distinct Population Segment (DPS) of *campestris.* Again, the Service fails to look at the information provided in the de-listing petition which details the connectivity of habitat from the North Platte drainage to the Tongue River and east to the Black Hills and the Belle Fourche River system, via the Big Horn Mountains. This connectivity is further supported by *Zapus* captures in Fremont and Johnson Counties of Wyoming. The WYNDD *Zapus* habitat model used in this discussion was funded and embraced by the FWS.

We reviewed Cryan 2004 for evidence that the trapping efforts undertaken by True Ranches were included in the data compellation, in doing so we identified the following errors. We assume the following two citations refer to the same document, which was provided to the FWS by True Ranches following completion of the 1999 trapping season. Pague and Schuerman were in no way involved in this effort and Ms Taylor's middle initial is "C", not W.

> TAYLOR, R. W. 1999. Trapping report: Preble's meadow jumping mouse (*Zapus hudsonius preblei*) on True Ranch Properties in southeastern Wyoming. Prepared for True Ranches, Casper, WY.
>
> PAGUE, C. A., AND P. T. SCHUERMAN. 1997. Natural heritage targeted inventory for the Preble's meadow jumping mouse: *Zapus hudsonius preblei*) on True Ranch properties in southeastern Wyoming. Prepared by Renee C. Taylor, Environmental Coordinator, True Ranches, Casper, Wyoming.

Further, we noticed that the *Z. h. campestris* specimens captured by True Ranches personnel and reported to the Wyoming Game and Fish Department in August 2002 (copy attached) were apparently not included in the information used in Cryan 2004. We wonder if these errors are reflective of the quality of work conducted by the USGS.

Further, we point out that the Service has adopted the common name of the Bear Lodge Jumping Mouse for *campestris*. This common name was inappropriately adopted from the Biodiversity Associates comments against the delisting petition. The proper common name for *campestris* would be that provided by Edward Preble in 1899, the prairie jumping mouse.

References Cited

Cryan, P.M. 2004. Synthesis of existing information on meadow jumping mice (Zapus hudsonius) in the northern Great Plains. Unpublished report prepared for U.S. Fish and Wildlife Service, Denver, Colorado.

Cryan, P.M. and L.E. Ellison. 2005. Distributional survey of the meadow jumping mouse (Zapus hudsonius) in the northern Great Plains: trapping report, summer 2005. Unpublished report prepared for U.S. Fish and Wildlife Service, Denver, Colorado.

King, T.L, J.F. Switzer, C.L. Morrison, M.S. Eackles, C.C. Young, B. Lubinski, and P.M. Cryan. 2006. Comprehensive analysis of molecular phylogeographic structure among meadow jumping mice (*Zapus hudsonius*) reveals evolutionarily distinct subspecies. A report submitted to the U.S. Fish and Wildlife Service. January 25, 2006.

Sincerely,

Dave True

Managing Member

Attachment

November 12, 2002

State of Wyoming
Game and Fish Department
5400 Bishop Blvd.
Cheyenne, WY 82006-0001
Attn: John Lund

RE: Scientific/Educational Permit #235

Dear Mr. Lund,

Following is the report for the referenced trapping event. If additional information is needed, please let me know. This was an extremely limited trapping event. No trap mortalities occurred. *Zapus hudsonius campestris* specimens captured were sacrificed and sent to the Denver Museum of Nature and Science, to the attention of Drs. Cheri Jones and Rob Ramey for use as voucher specimens and for DNA studies. The specimens have been prepared and are now housed in that collection.

In summary, total 970 trap nights, with approximately 93% trap availability, captured 96 deer mice, 25 harvest mice, 14 meadow voles, 3 jumping mice, and one house mouse.

LINE 001

Location: Center N½ Sec 31-T45N-R60W, Weston County, WY
Date: August 1, 2002
Species captured: *P. maniculatus*, total 18, 12 adults, 6 juveniles
Date: August 2, 2002
Species captured: *P. maniculatus*, total 26, 21 adults, 5 juveniles
Microtis sp., total 1, unknown age
Zapus hudsonius campestris, total 2, assumed juveniles
Effort: Set 180 traps each night, a total of 360 trap nights are included in this line. The weather was excellent, warm to hot, very calm, clear sky.

Habitat description:

Trap line was set immediately adjacent to Stockade Beaver Creek, north of LAK Reservoir. Immediately to the north is a public access picnic area. Habitat is very dense and comprised of willow, snowberry, wild plum, choke cherry, and narrow leaf cottonwood. Herbaceous vegetation and grasses included asparagus, thistle, ragweed, cow parsnip, bamboo, horsetail sedge, and variety of grasses. The site is within properties owned by True Ranches and is regularly used as pasture for cattle grazing.

LINE 002

Location: Center west line, NW 1/4 Sec 7-T44N-R60W, Weston County, WY; Trap line was set ¼ mi. west of the LAK Ranch headquarters on Stockade Beaver Creek, south of Hwy 16. The area is used for grazing of the horse herd and is adjacent a hay stack yard. The line was dissected by a culverted creek crossing. Immediately to the north is an old oil field.

Trapping results August 2 through August 9, 2002:

species/date	08/02/02	08/06/02	08/07/02	08/08/02	08/09/02	totals
P. maniculatus	7, 6A, 1J	9, 8A, 1J	12, 11A, 1J	13, 8A, 4J	11, 9A, 2J	52
Mus musculus	1	0	0	0	0	1
Z. hudsonius campestris	1	0	0	0	0	1
Rithrodontomys	0	3	7	6, 2A,4J	9	25
Microtis sp	0		2	3	8	13

Notes: Generally the weather was punctuated by thunderstorms in the evenings and through the night. Water level fluctuated in the creek causing some traps to be washed out of position. Daytime conditions were excellent, warm to hot, very calm, clear sky. Three out of four nights the line suffered raccoon damage but not a large percentage.

The *Zapus hudsonius campestris* captured August 2 appeared to be an adult and escaped on bagging. Trap set was extended on August 6 and again on August 7, to a total of 145.

Habitat description: The creek is sharply cut to the south of the crossing but well vegetated down the sides and at the bottom. North of the crossing, the creek has shallower sides and is well vegetated. Habitat is dense and comprised of willow, wild rose, sagebrush, wild plum, choke cherry, and narrow leaf cottonwood. Herbaceous vegetation and grasses included asparagus, thistle, ragweed, cow parsnip, bamboo, horsetail sedge, and variety of grasses.

Sincerely,

Renee C. Taylor
Environmental Coordinator