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Comments Regarding Proposed Rule to Remove the Gray Wolf (*Canis Lupus*) from the List of Threatened and Endangered Species

To Whom It May Concern:

On behalf of the Natural Resources Defense Council and our over 1.4 million members and online activists, we write to comment on the U.S. Fish and Wildlife Service's ("the Service") proposed rule to remove the gray wolf, *Canis lupus*, from the list of threatened and endangered species. See Proposed Rule Removing the Gray Wolf (*Canis lupus*) From the List of Endangered and Threatened Wildlife and Maintaining Protections for the Mexican Wolf (*Canis lupus baileyi*) by Listing It as Endangered, 76 Fed. Reg. 35664-35719 (June 13, 2013).

The Service is proposing to remove the current listing for the gray wolf, *Canis lupus*, from the Endangered Species Act ("ESA") in part because the Service now believes that an "eastern wolf," (*Canis lycaon*) which it claims is a different species than the "gray wolf," historically occupied the eastern U.S. Therefore, the Service argues, gray wolves have never lived in the eastern United States and cannot be protected there. However, as we have previously explained, the best available science indicates that the "eastern wolf" is not a separate species and likely just represents historic and recent hybridization between gray wolves and coyotes. And, even if the eastern wolf was a different species, that would not have precluded eastern wolves and gray wolves from historically occupying overlapping habitat in the eastern U.S. In fact, the leading canid researchers have made this precise argument to the Fish and Wildlife Service already.

The Proposed Rule relies almost entirely on a single publication, Chambers et al. 2012, to propose recognizing *C. lycaon* as a separate species from *C. lupus*, thereby allowing the Service to remove protections from wolves in 29 eastern states. The faith the Service places in this study is disconcerting, as the Service already attempted to rely on

Chambers et al. to support this very same taxonomic change when it first proposed to remove protections from gray wolves in the Midwest, 76 Fed. Reg. 26086, but the study was met with such opposition from scientists (Appendix 1) that the Service retracted its reliance on Chambers et al. concluding that it “represents neither a scientific consensus nor the majority opinion of researchers on the taxonomy of wolves.” 76 Fed. Reg. 81669. In yet another reversal, less than two years later, the Service now asserts that Chambers et al. 2012 does represent the best available science despite the persistence of serious flaws in the document and its main conclusions. Indeed, it is now clear that Chambers et al. was fatally flawed from the outset, as the study was conceived of as an exercise to identify an alternative taxonomic scheme that would facilitate the removal of the nationwide listing of wolves. Far from representing the best available science, this paper is pretextual agency science, published in an agency journal, to support a specific policy outcome: delisting wolves across the United States.

The Proposed Rule’s reliance on Chambers et. al. to recognize the existence of a new species is particularly troubling. The ESA requires the Service to rely on scientific consensus when it comes to identifying new species and subspecies. Faced with similar scientific debates in the past, the Service has consistently interpreted its authority as limited to recognizing new species only when there is broad scientific consensus. The Service has applied this basic principle in case after case. As such, until there is more consensus regarding the existence of *C. lycaon*, the Service should not revise its taxonomy designation and should maintain protections for the gray wolf across its range.

Because the Service cannot recognize the existence of *C. lycaon*, it must continue to classify wolves occurring in the northeastern United States as gray wolves (*C. lupus*). The Northeast United States is a significant part of the gray wolf’s range, where the wolf remains endangered. The Service therefore cannot, consistent with the ESA, remove the gray wolf from the federal list of endangered species. In addition, the Service must continue to protect wolves in the Pacific Northwest, which, at the very least, represents a distinct and significant population.

#### **A. “*Canis lycaon*” Should Not Be Recognized as a New Species**

The Service’s proposed designation of a new wolf species centers around the fact that some wolf-like canids have been found to contain genetic material that is more closely related to coyotes than to gray wolves (Wilson et al. 2000, 2003, Kyle et al. 2006, Leonard and Wayne 2007, Koblmuller et al. 2009, Wheeldon et al. 2010). Although these coyote-like haplotypes have been recovered in some wolves from the eastern U.S. and Canada, the same haplotypes are not currently represented in modern coyote populations. This pattern has been explained by competing hypotheses. One hypothesis is that the existence of the coyote-like haplotypes in current wolves is a reflection of ancient hybridization between gray wolves (*C. lupus*) and coyotes (*C. latrans*), in which the corresponding haplotype in coyotes has since diverged or disappeared (Leonard and Wayne 2007, Koblmuller et al. 2009, Kays 2011, New York State 2011). The hybridization event would have taken place within the geographic range of overlap for the two species and moved east geographically via gray wolf

dispersal. The competing hypothesis, embraced by Chambers et al. 2012 and the Proposed Rule, is that although the haplotypes are closely related to coyotes, they actually represent a separate species of wolf—*C. lycaon*—which is distinct from gray wolves (*C. lupus*) (Wilson et al. 2000, Wheeldon and White 2009, Wheeldon et al. 2010). This conclusion, which Chambers et al. 2012 adopts and the Proposed Rule relies on, is simply not justified.

### 1. Chambers et al. 2012 Is Scientifically Flawed

Chambers et al. 2012, itself, is scientifically flawed, making its conclusions unreliable. All listing and delisting decisions must be based “solely on the . . . best scientific and commercial data available.” 16 U.S.C. § 1533(b)(1)(A). This standard gives “the benefit of the doubt to the species” in listing decisions. *Center for Biological Diversity v. Lohn*, 296 F. Supp. 2d 1223, 1239 (W.D. Washington 2003) (citing *Conner v. Burford*, 848 F.2d 1441, 1454 (9th Cir. 1988)). Accordingly, when the best available science is unclear or controversial, the Service must err on the side of providing ESA protection. The Service’s reliance on Chambers’ preferred taxonomy for the gray wolf does not meet this standard.

First, Chambers et al. 2012 does not rely on explicit criteria for defining the subspecies and species described in its account, a serious flaw in a study of this nature. Second, as noted above, simpler, alternative hypotheses explain the data. Third, the species, *C. lycaon*, that Chambers et al. 2012 proposes recognizing is not readily identifiable either through morphology or robust genetic testing. And, fourth, the wolves that Chambers et al. 2012 regards as *C. lycaon* are extensively hybridized with both coyotes and other gray wolves, further complicating their existence as a separate species, either historically or presently.

- a. Chambers et al. 2012 contains no explicit criteria and relies on one morphological study

During the various review processes that Chambers et al. 2012 went through, numerous reviewers consistently pointed out that the authors’ taxonomic conclusions are not based on explicit criteria about what constitutes a species or subspecies (often referred to by scientists as a “species concept”), which makes it difficult to evaluate whether any of the taxonomic units identified by the authors are valid.<sup>1</sup> For example, one reviewer notes that “[i]n the sections where Chambers et al. make taxonomic recommendations, it is not completely clear which concepts they are applying.” (Appendix 2 - SEI 2012, Dumbacher review). Similarly, another reviewer notes that the authors should “choose a

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<sup>1</sup> What criteria should be used to distinguish different species from each other has been the subject of lively scientific debate, and the selection of concept greatly influences whether an author concludes that a particular species does, or does not, exist. See George, A.L., R.L. Mayden. 2005. Species concepts and the Endangered Species Act: how a valid biological definition of species enhances the legal protection of biodiversity. See e.g., Nat. Resource Law J 45:369–407 (discussing a wide variety of species concepts).

particular species concept or (provide) interpretations based on multiple concepts since it is unclear what criteria are being used to draw the final taxonomic conclusions that are so important given the current debate.” (Appendix 2 - SEI 2012, Waits review). Yet another reviewer notes that the authors claim “to draw objective conclusions regarding the rank of taxa in the absence of any stated objective criteria.” This reviewer concludes, “All taxonomic conclusions are undermined in part by the authors not providing the species/subspecies delimitation criteria they used.” (Appendix 2 - SEI 2012, Stepan review). Chambers et al. 2012 ultimately defined their approach to taxonomy as “eclectic” and “integrative” (p. 5), but never explicitly identified specific criteria for defining their species and subspecies classifications. This makes it difficult, if not impossible, for the Proposed Rule to rely on Chambers’ conclusions regarding the existence of *C. lycaon*, as there is no set of criteria against which agency decision-makers, peer-reviewers, or members of the public can judge its conclusions.

Multiple reviewers also raised the concern that the Chambers et al. study relied too heavily on the taxonomic scheme developed by Nowak 1995. The primary concern of the reviewers is that Nowak 1995 was based exclusively on morphometric measurements and may not have been sufficiently peer-reviewed. As one reviewer remarked, Nowak 1995 “is probably the weakest of any of the recent analyses of population subdivision. It owes entirely to the efforts of one individual (Ron Nowak) who has applied the same measurements and techniques since the mid-1970s and the vast majority of his results appear in book chapters of uncertain review quality.” (Appendix 2 - SEI 2012, Wayne review). Yet Chambers et al. relies on Nowak 1995 to such an extent that it drives much of the analysis. This was described as a “fundamental problem” by a reviewer who explained, “[B]y using the morphologic data...the authors are putting the cart before the horse. (Taxonomy) should emerge from a critical analysis of all the data, especially that which directly assesses genealogy.” Ultimately, the combined criticisms of lack of explicit criteria and the over-reliance on Nowak 1995 lead this reviewer to conclude, “[T]he basic approach of matching genetic and historical data to a defined morphologic taxonomy rather than deriving the taxonomy directly from a critical analysis of genetic and morphologic data leaves the conclusions fundamentally flawed.” (Appendix 2 - SEI 2012, Wayne review).

Despite these criticisms, the final publication still contains these flaws.

#### b. Alternative hypotheses explain the data

The Service’s reliance on Chambers et al. 2012 is also flawed because the study purports to recognize a new species, *C. lycaon*, despite the fact that there are valid alternative hypotheses that do not require the existence of a separate species. Because the data are entirely consistent with the more parsimonious explanation that gray wolves (*C. lupus*) and coyotes (*C. latrans*) have periodically hybridized in this geographic region, the Service should not take the drastic step of declaring a new species of wolf to exist in the Proposed Rule.

Parsimony is a fundamental scientific principle in which “an explanation requiring the fewest undocumented assumptions should be preferred over more complicated

hypotheses that require more assumptions for which evidence is lacking." (Futuyma 1998, p. 96). As applied to evolution, the principle of parsimony favors the explanation that requires "the fewest evolutionary changes" to explain a given pattern. *Id.* In this case, the most parsimonious explanation favors the existence of two species, *C. lupus* and *C. latrans*, rather than the existence of a third species, *C. lycaon*.

Even the leading researchers who have suggested the possibility of *C. lycaon* as a species acknowledge that this hypothesis may not be valid. For example, many of these researchers admit that they cannot rule out the possibility that what they refer to as *C. lycaon* is actually a reflection of past hybridization between coyotes (*C. latrans*) and gray wolves (*C. lupus*). Wheeldon et al. (2010), who argue that wolves and coyotes are not hybridizing in the Great Lakes area, state that they "cannot rule out the occurrence of historic hybridization between [Western Great Lakes Region] wolves and coyotes" (p. 8). Similarly, Kyle et al. (2006) conclude, "The hypothesis that eastern wolves are the result of *C. lupus/C. latrans* hybridization cannot be rejected by all of the molecular data." Brent Patterson, a co-author of Wheeldon et al. (2010) wrote, "Future research might yet reveal that there never was a North American evolved... eastern wolf, and that these animals are indeed merely hybrids between *C. lupus* and *C. latrans*." (Appendix 3 - Patterson 2011, email communication). Even one of the authors of Chambers et al. (2012), Steve Fain, recently remarked about the recognition of eastern wolf as a separate species, "I'm not saying it's a done deal...but it's very convincing to me" (Beeland 2013, pg.121).

In short, although some researchers have suggested that a separate eastern wolf species, *C. lycaon*, may exist, these authors continue to acknowledge that they cannot definitively distinguish between this conclusion and competing hypotheses, including the conclusion that *C. lycaon* is not a species at all, but a product of hybridization between gray wolves and coyotes. It is thus inappropriate for Chambers et al. 2012 and the Service to recognize *C. lycaon* as a species, especially because a simpler explanation – past hybridization – exists and is preferred by many in the scientific community (See Appendix 1).

### c. *C. lycaon* is not identifiable

Furthermore, *C. lycaon* does not appear to be distinguishable from other wolves in the region, an issue that seems highly problematic for any proposal to designate it as a new species. *C. lycaon*, so-called "eastern wolves," do not look or act any different than other gray wolves or admixed canids (wolf-coyote hybrids) found in the Midwest and Canada today. Nor are they readily identifiable by their nuclear genomes, a seemingly essential component for identifying new species based on genetic distinctiveness (Rubinoff 2005). Indeed, as explained below, the fact that the genetic evidence of an "eastern wolf" is so hard to identify provides further evidence that no such species exists.

In the wild, the *only* way to distinguish an "eastern wolf" from individuals otherwise referred to as gray wolves, *C. lupus*, is to match genetic samples from individual wolves to a handful of "coyote-like" mitochondrial (mt-DNA) or Y-chromosome haplotypes.

But even this test is incapable of distinguishing “pure” eastern wolves from wolves of mixed descent, since this type of DNA is inherited from a single parent and thus non-admixed individuals cannot be identified by mtDNA and Y-chromosome data alone. Indeed, *C. lycaon* does not appear to be identifiable based on *any* bi-parentally inherited genetic markers such as autosomal microsatellites and SNPs. Thus, there is no unique nuclear genetic signature for this species (i.e., an easily identifiable and inheritable genetic signature that, under normal circumstances, readily identifies one species from another).

Chambers et al. 2012 attempts to overcome this problem by pointing to ‘private alleles’ – groups of genes that are found in *C. lycaon* and not shared with other “wolf populations.” However, the term ‘private alleles’ is used inappropriately here as the authors note that while these alleles are ‘private’ with respect to other wolves, “many of these alleles are shared with coyotes.” (p. 15). Thus, rather than providing support for an independent evolutionary history for *C. lycaon*, these alleles point to a history with coyotes, thus supporting the alternative hypothesis of hybridization between gray wolves and coyotes.

Similarly, the haplotypes that Chambers et al. 2012 do point to in order to identify *C. lycaon* are not monophyletic (i.e., that is, they do not appear in a single unique “clade” or “branch” of the genetic tree). Although Chambers et al. 2012 claims that “[b]oth mtDNA and Y-chromosome haplotypes place eastern wolves in monophyletic clades that are highly divergent from those of gray wolves” (p. 20), again, they fail to mention that the eastern wolf haplotypes themselves are not monophyletic and are scattered within a broader coyote, *C. latrans*, clade.

If a species, *C. lycaon*, had been evolving in isolation for 150,000-300,000 years one would expect to be able to detect unique nuclear genetic markers, yet none have been described. The fact that the only identifying characteristics for this supposed taxon is a very limited number of mtDNA or y-chromosome haplotypes, which are not monophyletic but rather are found within the coyote’s clade, is simply not a sufficient criterion for recognizing a species under any existing species concepts, including the biological species concept (reproductive isolation) or the phylogenetic species concept (monophyly) (Avice 2004).

d. Even if *C. lycaon* existed historically, it has been hybridized out of existence

While the Service and Chambers et al. 2012 describe *C. lycaon* as occurring “in the general area from southern Ontario and Quebec, west to Minnesota and Manitoba” (Chambers et al. 2012, p. 14), researchers who have suggested the existence of *C. lycaon* actually claim that only “the wolves in Algonquin Provincial Park, Ontario, Canada are currently the best representative of the eastern wolf” (Rutledge et al. 2012, p. 188). In fact, the researchers admit that hybridization of what they refer to as *C. lycaon* with *C. lupus* and *C. latrans* has been so extensive that “there are likely no

remaining unhybridized eastern wolves in the wild" (Appendix 3 - Patterson 2011, email communication). Ironically, even Chambers et al. 2012 makes this point by discussing the hybridization of *C. lycaon* with *C. lupus* and *C. latrans* as evidence that the 'species' may be undergoing "reverse speciation or despeciation" (p. 27) but finds that "it is premature to conclude that the eastern wolf is no longer an identifiable taxon." On the contrary, as described in the previous section, it is premature to conclude that the eastern wolf *is* an identifiable taxon.

Ultimately, if there are no true representatives of the species in the wild that are identifiable either by morphology, geography or robust genetics, there seems to be no basis for the designation of a separate, extant species and the removal of protections from *C. lupus* as a whole.

## **2. Chambers et al. 2012 Is Agency Science, Not the Best Available Science**

The significant flaws that underlie Chambers et al.'s analysis should not be surprising. That is because Chambers et al. does not represent an objective, independent evaluation of the science regarding wolf taxonomy. Rather, it was an inherently biased project, conceived of from the outset to identify alternative taxonomic schemes for "managing" gray wolves. Once the manuscript was completed, the Service submitted it for publication in an agency journal. The Proposed Rule's reliance on Chambers et al. is thus contrary to the ESA's best available science standard and contrary to the standard process by which science is investigated.

Rather than being an objective analysis prompted by scientific inquiry, Chambers et al. 2012 was motivated by the Service's desire to address a policy problem. Specifically, the Service was interested in identifying an alternative taxonomic scheme that would facilitate the removal of the nationwide listing of wolves. Four of the agency's own biologists were asked to review the taxonomy of wolves to identify alternative listable entities. In an email sent to reviewers of an early draft of Chambers et al., Chambers himself characterized the study as such, stating, "The Service is evaluating advantages and disadvantages of using subspecies as units for listing, compared to the distinct population segments that have been used since 1978. A subspecies that is found to be scientifically valid and supportable during this review may or may not eventually be identified as a unit for listing or delisting." (Appendix 4 - Email from Steve Chambers, dated 11/12/2009, titled "Re: Charge to reviewers of wolf taxonomy report"). It should also be noted that the communication states, "No changes in listing status of wolves will be based solely on this taxonomic review." This assertion is belied by even a casual glance at the Proposed Rule.

Talking points regarding the taxonomic study that were drafted by the Service clearly demonstrate that Chambers et al. 2012 was initiated at the direction of the Service for the purposes of guiding their wolf policies: "In 2008, the U.S. Fish and Wildlife Service directed a group of Service scientists to conduct a review of the taxonomy of North American gray wolves to help guide wolf recovery efforts in the U.S." (Appendix 4 - Email from Gary Frazer via Maricela Constantino, dated 10/16/2009, titled "FW: Wolf taxonomic review including Talking Points"). Therefore, rather than being an objective

evaluation of the current state of canid taxonomy, Chambers et al. 2012 was conceived of and motivated by a desire for the Service to achieve a particular policy outcome (i.e., the ability to replace the national listing with alternative taxonomic units).

More importantly, by instructing their own agency biologists to come up with an alternative taxonomic scheme for managing wolves, the Service inherently inserted bias into the process in a way that is likely to have influenced the outcome of the study. While Chambers and his coauthors may not have been instructed to reach a specific conclusion, their work was necessarily influenced by the instructions they received to examine alternative taxonomic units for the purposes of managing wolves under the ESA. This bias is evident in the nature of criticisms that the document received during its review, including its over-reliance on the taxonomy of Nowak 1995, its lack of explicit criteria, and the perception of a predetermined conclusion.

Furthermore, rather than being published in an independent scientific journal, Chambers et al. 2012 was published in *North American Fauna*, a recently resurrected U.S. Fish and Wildlife Service journal that hadn't published any monographs in over twenty years. Although *North American Fauna* conducts its own peer review process, its association with the agency that houses the authors of the publication does not present an appearance of independence. In fact, at the time of publication, Steve Chambers, the primary author of Chambers et al. 2012, was on the editorial board of *North American Fauna's* sister journal, the *Journal of Fish and Wildlife Management*.

Internal communications within the Service indicate that the authors chose to submit their article to this journal "because of its length, primarily." (Appendix 4 - Email from Ralph Morganweck titled "Re: Independent Formal Review of Wolf Taxonomy Report" 10/19/2009 from FOIA). However, there are other independent scientific journals that are equally able to accommodate lengthy monographs, such as *Wildlife Monographs*, for example, which is published by The Wildlife Society. Furthermore, in articulating what distinguishes the journal from other scientific journals, the Editor-in-Chief, Paul Flint, explains, "What makes this monograph unique is that there is no requirement that articles appeal to a broad general audience, be perceived as being novel, or have a high scientific impact. That is, within the confines of the work environment, many agency personnel are constrained to focus on fairly narrow topics or specific management problems. The resulting products may be fundamentally sound, but may not be publishable in common commercial outlets because the topic or presentation is not sufficiently broad to appeal to a wide journal audience."

(<http://www.fwspubs.org/userimages/ContentEditor/1274882898961/nafa-10-00-1-2.pdf>). That is, the journal is designed to serve the purpose of publishing articles by agency biologists that may cover obscure or narrow topics that address a specific management problem. Although the topic of canid taxonomy does not appear to be obscure or narrow in focus, it does address a management problem of interest to the Service. And, as Gary Frazer explained, "There was no other journal that would accept that kind of paper, because it was a synthesis with no new research," (Nature <http://www.nature.com/news/grey-wolves-left-out-in-the-cold-1.13716>). In short, the



Service viewed the document as unsuitable for publication in an independent scientific journal.

Although Chambers et al. 2012 does represent a 'peer-reviewed' publication, it ultimately only represents the opinions of four agency biologists that were tasked with addressing a specific policy problem – the results of which were published in an agency journal that is designed to favor publications by agency biologists for the purpose of addressing management issues. This amounts to agency science, and it, without question, fails to meet the standard of best available science. 16 U.S.C. 1533(b)(1).

### **3. Other Contemporary Studies Do Not Support the Findings of Chambers et al. 2012**

In its rule, the Service relies almost exclusively on Chambers et al. 2012 as its entire scientific basis for supporting its own proposed taxonomy – a taxonomy which, as explained above, is fundamental to the Proposed Rule's delisting of wolves nationally. In doing so the Service largely ignores one of the most recent and extensive genetic surveys ever conducted on canids, which concludes that rather than multiple species of wolves in North America existing, there is only the gray wolf, *Canis lupus* (vonHoldt et al. 2011). This study examined 48,000 single nucleotide polymorphisms (SNPs) from a global sample of canids – making it the most robust genetic analysis ever completed for any wild vertebrate group. The authors found that the genome of the hypothesized eastern wolf was largely representative of gray wolf, *Canis lupus*, (85%) with the remaining 15% belonging to coyote, *C. latrans*. Unlike the extremely limited mtDNA and Y-chromosome markers used by Chambers et al. 2012 to identify *C. lycaon*, the vonHoldt study examines the canids' entire nuclear genome and shows no evidence of an independent evolutionary history suggesting the existence of a separate species of North American wolf. These findings are in direct contrast with the conclusions of Chambers et al. 2012.

In discounting the findings of vonHoldt et al. 2011, both Chambers et al. 2012 and the Service cite Rutledge et al. 2012, which is a direct rebuttal to vonHoldt et al. 2011. However, Rutledge et al. 2012 suffers from significant flaws and cannot be relied on to support the Service's acceptance of *C. lycaon* as a species. For one, rather than supporting the conclusion that *C. lycaon* is a separate species, Rutledge et al. 2012 actually concludes that the taxonomic issue "deserves further investigation." Furthermore, much like Chambers et al. 2012, Rutledge et al. 2012 provides little new data. Rather, it presents a re-analysis of existing data that was presented in vonHoldt et al. 2011 and 12 autosomal microsatellite data presented in Wheeldon et al. 2010. The new analysis presented in Rutledge et al. 2012 is likely not appropriate to address the central question of whether there is a separate species of eastern wolf, *C. lycaon*, or simply hybridization between gray wolves and coyotes, *C. lupus* and *C. latrans*.

First, Rutledge et al. 2012 relies heavily on PCA (Principle Components Analysis), a commonly used analysis that lacks any DNA-based model of population genetic theory. However, the authors fail to consider the combined impacts of modern and historic

hybridization on contemporary genetic patterns in interpreting their results. While PCA may be helpful for identifying clusters, it is difficult to provide evidence for interpreting those clusters as discrete genetic units (Novembre & Stephens 2008; Reich et al. 2008).

Furthermore, due to the lack of population or evolutionary genetic model, PCA results should be interpreted with serious caution, particularly in relation to addressing hybrid issues and when making phylogenetic or taxonomic claims (Novembre and Stephens 2008, Reich et al. 2008).

Second, Rutledge et al. 2012 relies on an over-interpretation of phylogenetic analysis using SNP data (single nucleotide polymorphisms). Phylogenetic analysis with SNPs is likely not a valid application when applied to hybrid taxa as it assumes that evolution occurs by strict bifurcation rather than reticulated evolution (vonHoldt et al. 2010).

Third, while Rutledge et al. 2012 provides some new data (12 microsatellites), it is an independent data set that cannot be compared directly to the data presented in vonHoldt et al. 2011. Additionally, there is insufficient statistical resolution in those data to resolve admixture patterns. A recent study demonstrates that a minimum of 25 microsatellites would be required to resolve recent hybridization events among populations (Haas & Payseur 2011).

In the simplest terms, Rutledge et al. 2012 selected methods for analysis that favor a straightforward bifurcated model of evolution and overlook the possible effects of a more complicated history of repeated hybridization. Perhaps most importantly, Rutledge et al. 2012 does not conclude that their study definitively provides evidence for the existence of a separate species, *C. lycaon*. Much like the other researchers on this issue, they actually conclude from their own results that the taxonomic issue deserves "further investigation" (pp. 186, 187, 189). That is, Rutledge et al. 2012 concludes that the taxonomic debate over whether *C. lycaon* is a separate species *is not resolved*.

To sum up, the conclusions of Chambers et al. 2012 have been entirely contradicted by a much more thorough and robust genetic analysis that concludes that the hypothesized eastern wolf is simply a gray wolf that demonstrates a certain level of introgression with coyotes (vonHoldt et al. 2011). Chambers et al. 2012 tries to discount this study by relying on Rutledge et al. 2012, but that study is significantly flawed and ultimately does not support Chambers et al. Therefore, the only other studies to have recently addressed this issue do not provide support for the acceptance of *C. lycaon* as proposed by Chambers et al. 2012.

## **B. There is No Scientific Consensus Regarding the Existence of *C. lycaon***

Under the ESA, the Service can only recognize new species or subspecies when there is scientific consensus. The Service's regulations clearly state: "In determining whether a particular taxon or population is a species for the purposes of the Act, the Secretary shall rely on *standard taxonomic distinctions* and the biological expertise of the Department and the *scientific community* concerning the relevant taxonomic group." 50

CFR 424.11(a) (emphasis added). As set forth below, the Service has consistently interpreted this regulation to require that the scientific community achieve widespread agreement regarding the existence of a new species or subspecies before these populations are recognized as “species” within the meaning of the ESA.

The Service has therefore relied on currently recognized taxonomic classifications, not new taxonomic theories. Indeed, faced with similar scientific debates in the past, the Service has consistently interpreted its authority as limited, recognizing new species only when broad scientific consensus has been achieved. In doing so, the Service often refers to scientific organizations like the International Commission of Zoological Nomenclature and the American Society of Mammalogists, whose recognition of a new species or subspecies demonstrates broad consensus in the field. Until now, the Service has applied this basic principle in every taxonomy debate it has encountered, relying on a conservative, consensus view rather than the opinions of a few scientists.

For example, in determining whether the pygmy owl warranted listing as an endangered species, the Service used the “currently accepted taxonomic classification,” stating, “[I]n the presence of unresolved inconsistencies, the Service relies upon the ‘standard taxonomic distinctions . . . .’” 76 Fed. Reg. 61861 (October 5, 2011). Similarly, when evaluating the taxonomy of the Atlantic Sturgeon, the Service adopted the consensus view, noting that “the Alabama sturgeon is currently and widely recognized in published literature as a valid taxonomic species” and that “[o]nly a single peer-reviewed paper has been published that questions the taxonomy of the Alabama sturgeon.” 65 Fed. Reg. at 26443-44.

For the Preble’s Meadow Jumping Mouse (PMJM), the Service relied on the taxonomy “generally accepted by most small taxonomists for the past half-century,” rather than a single study that reached a different conclusion. 78 Fed. Reg. at 31682. The Service reasoned that because “available data are broadly consistent” with its subspecies classification, “delisting the PMJM based on the conclusions of Ramey *et al.* alone might be premature.” 78 Fed. Reg. at 31864.

There are countless other examples of taxonomic debates in which the Service based its taxonomic classifications on established scientific consensus, including:

- The Service’s determination that the coastal California gnatcatcher was a valid subspecies by relying on peer-reviewed taxonomic studies and the input and expert opinion of the American Ornithologists’ Union and its constituent Committee on Classification and Nomenclature, 60 Fed. Reg. at 15693;
- The Service’s conclusion that the Pacific pocket mouse constituted a valid subspecies based on the 90-year taxonomical history of the species, peer-reviewed and published classification of the heteromyid rodents that includes the pocket mice taxa, and the opinion of the American Society of Mammalogists, 59 Fed. Reg. at 49755;

- The Service's classification of the Geirisch Mallow as a valid species based on a peer-reviewed journal article and its recognition by taxonomic authorities, including the Integrated Information System and the Utah Rare Plant Guide, 78 Fed. Reg. at 49149-50;
- The Service's decision that the Olympic Mazama pocket gopher was a subspecies as opposed to a species based on the expertise of the Smithsonian National Museum of Natural History's Curator of North American mammals and the American Society of Mammologists, 77 Fed. Reg. at 73773; and
- The Service's refusal to reclassify the Cactus ferruginous pygmy-owl as a subspecies based on the "currently accepted taxonomic classification," despite recognizing that "the taxonomy of the pygmy-owl has been ever-changing, and still requires considerable investigation and additional information." 76 Fed. Reg. at 61856.

As set forth below, applying the Service's own regulations and its past practices to the Proposed Rule quickly reveals that there is no basis for recognizing *C. lycaon* as a new species. First, there is widespread disagreement within the scientific community over the existence of *C. lycaon* as a species. Second, the existence of this new species has not been recognized by any authoritative scientific body. Finally, the Service itself rejected Chambers et al. 2012 as a sufficient basis for recognizing *C. lycaon* less than two years ago.

### **1. Chambers et al. 2012 Does Not Reflect the Scientific Consensus**

According to the proposed rule, Chambers et al. 2012 was "prompted" by "the lack of consensus among researchers on these issues." 78 Fed. Reg. 35669. This gets at the heart of the problem: there is a lack of consensus because there is not sufficient scientific evidence to support recognition of *C. lycaon*. As one peer reviewer put it, "This lack of ... consensus should tell us something; it is not time yet to make pronouncements about the species status of the Great Lakes wolf [*C. lycaon*]" (SEI 2012, Wayne review). As will be explained in more detail below, if Chambers et al. 2012 does not reflect the consensus view of the best available science, it cannot be relied on to designate a new species.

The peer reviews of Chambers et al. 2012 demonstrate the wide range of scientific opinions on the topic of canid taxonomy. For example, in 2009 the Service initiated its own informal review of an early draft of Chambers et al. 2012 by soliciting feedback from 17 experts in the field. Of the 14 experts that responded, 5 indicated general support for the proposed taxonomy, 3 reviewers indicated that the conclusions required further support, and the remaining 6 raised significant concerns or disagreement with the proposed taxonomy (Appendix 5 - Excel sheet and corresponding reviews).

The Service then conducted a second review of the revised manuscript in which they hired an independent group, Sustainable Ecosystems Institute, to conduct the review. Rather than use this external, independent group to referee the reviews, however, the

Service used Ralph Morganweck, an employee of the Service, and Robin Waples of NOAA. Five experts reviewed the manuscript and submitted a wide range of responses from general agreement to fundamental disagreement. Notably, several of the reviewers pointed out that the data support alternative taxonomic scenarios and Chambers et al. 2012 failed to adequately consider those alternatives (Appendix 2 - SEI 2012, Dumbacher, Stepan and Wayne reviews). In their summarizing comments, Ralph Morganweck and Robin Waples reflect these concerns by indicating that Chambers et al. 2012 did “not adequately explain the rationale for their conclusions” including “explicit statement of the criteria the authors used to weigh the diverse types of information.” They also conclude, “On several of the key questions, top researchers in the field have divergent opinions.” Morganweck and Waples suggest a number of changes, including a restructuring of the document, stating, “We believe that the proposed framework would be less likely to lead to the perception that the information was being presented in a way that best supports a predetermined conclusion.” However, despite the fact that the review revealed highly divergent expert opinions, insufficient scientific support for the document’s conclusions, and the “perception” of a “predetermined conclusion,” no significant changes were made to the originally proposed taxonomy. Nor did the authors or the Service consider abandoning the taxonomic review.

Chambers et al. 2012 does not represent a consensus scientific opinion based on settled science. In fact, if anything, the combined review processes that the manuscript underwent further highlight exactly how much disagreement there is on the topic of canid taxonomy and underscore the fact that Chambers et al. 2012 merely represents the opinions of the four agency biologists who authored it rather than representing the consensus opinion of the scientific community. It is therefore an inappropriate basis for designating a new species of wolf.

## **2. *Canis lycaon* is Not Recognized as a Species by Any Scientific Body**

Recognition of new species is a conservative process, conducted by official scientific organizations. Due to the unsettled science surrounding the taxonomy of the gray wolf, these organizations have not recognized the eastern wolf, *C. lycaon*, as a wolf species.

The American Society of Mammalogists maintains a list of recognized species and currently only recognizes coyotes, gray wolves, and red wolves in North America (<http://www.science.smith.edu/msi/>). Similarly, the official International Commission on Zoological Nomenclature does not recognize *C. lycaon* as a species ([http://iczn.org/sites/iczn.org/files/Official\\_List\\_updated\\_December\\_2012.pdf](http://iczn.org/sites/iczn.org/files/Official_List_updated_December_2012.pdf)).

A status review of *C. lycaon* that the Service commissioned acknowledged the ongoing controversy over the recognition of *C. lycaon* and concluded that the species is not recognized by authoritative taxonomic societies:

Geneticists, especially, use peer-reviewed scientific journals to advance their position that this unique wolf type, labeled the eastern wolf, *Canis lycaon*, is a distinct species. This posture is presently subject to robust and healthy scientific debate. The fact remains these large canids presently *lack official designation* as a

species. We encourage those who promote eastern wolves as a distinct species petition an international authority such as the International Commission of Zoological Nomenclature, the American Society of Mammalogists, etc., to resolve this issue so that conservation efforts may proceed (Thiel and Wydeven 2012) (emphasis in original).

Furthermore, this conclusion by Thiel and Wydeven 2012 that *C. lycaon* is not formally recognized was made *after* Chambers et al. 2012 had been accepted for publication, suggesting that they do not view Chambers et al. 2012 as sufficient to officially recognize the species.

The Service has ignored the best available science, illustrated by these official organizations, which demonstrates that recognition of a new species is premature. Until the science concludes that the eastern wolf is, in fact, a new species, the Service should not base listing decisions on an unrecognized species.

### **3. The Service Determined That There Is Inadequate Consensus To Recognize *C. lycaon* Just Two Years Ago**

In fact, the Service recently rejected designating *C. lycaon* as a new species due to the “ongoing scientific debate” regarding its status as a separate species. 76 Fed. Reg. at 81866, 69. The Service stated that Chambers et al. 2012 “represents neither a scientific consensus nor the majority opinion of researchers on the taxonomy of wolves.” *Id.* Now, however, just two years later, based on the same science, the Service has completely flip-flopped, finding that Chambers et al. 2012 does support designating *C. lycaon* as a new species. Chambers et al. 2012 did not change in any substantial way between the Midwest delisting rule and this Proposed Rule, and thus it defies logic that merely two years later, the same scientific document now *supports* recognition of a new species.

In sum, the ESA requires scientific consensus before designating a new species. The Service has consistently applied the rule in its past listing decisions, including in a similar rule rejecting *C. lycaon* as a new species just two years ago. There is widespread disagreement in the scientific community regarding the existence of *C. lycaon*, and recognized scientific bodies have thus far declined to designate the species. It is therefore premature for FWS to recognize *C. lycaon* under the ESA.

### **C. The Best Available Science Shows That *C. lupus* Lived in the Eastern United States**

In concluding that *C. lupus* did not occur in the eastern United States, the Service cites “the results of recent molecular genetic analyses (e.g., Wilson et al. 2000, Wilson et al. 2003, Wheeldon and White 2009, Wilson et al. 2009, Fain et al. 2010, Wheeldon et al. 2010, Rutledge et al. 2012) and morphometric studies (e.g., Nowak 1995, 2000, 2002, 2003).” 78 Fed. Reg. 35670. These studies, however, do not support the Proposed Rule’s conclusion; the best available science clearly shows that there is no sufficient basis to revise the historically accepted range of *C. lupus* in the eastern United States.

While, as discussed above, the genetic studies that the Service references suggest the presence of *C. lycaon* in certain areas in the eastern United States, this evidence does not support the notion that gray wolves (*C. lupus*) also did not exist in the eastern United States. Indeed, the authors of these “recent molecular genetic analyses” specifically told the Service that, even if the Service were to recognize *C. lycaon*, “there is not sufficient information to support the geographic range change for *Canis lupus* in the eastern US as currently proposed.” (Wheeldon, et al. 2011). That is, in coming to the conclusion that gray wolves did not occur in the eastern US, the Service is going against the scientific opinion of the very experts that it cites.

Thus, the only other studies cited by the Service that provide any support for the notion that gray wolves did not exist historically in the eastern United States are, instead, Nowak’s “morphometric studies” (studies of the size, shape and appearance of an animal), referenced above. But these studies are insufficient to demonstrate that, contrary to current scientific opinion, *C. lupus* did not live in the eastern United States. Morphology – particularly of mid-sized canids – has been shown to be unreliable for accurately identifying species when compared to genetic data, and therefore cannot be relied on to support a range change for *C. lupus*. (Wayne and Jenks 1991, Roy et al. 1996).

Finally, genetic analyses on modern, extant species show evidence of genetic signal from *C. lupus* in coyotes (*C. latrans*) and red wolves (*C. rufus*) currently in the eastern United States, suggesting that these species overlapped in that portion of their range historically (e.g. vonHoldt et al. 2011, see Wayne et al. 2012 comment letter in Appendix 1). Thus, to the extent that genetic evidence does exist about the historic range of gray wolves, it demonstrates – contrary to the Service’s conclusion – that gray wolves once lived, and interbred, with coyotes in the eastern United States.

In concluding that gray wolves (*C. lupus*) did not occur in the eastern United States, the Service presents no convincing evidence. In fact, the Service goes against the conventional recognition of the species’ range and against the direct admonition of many of the leading canid experts (Wheeldon et al. 2011). These experts explicitly state that even if the Service recognizes the existence of *C. lycaon* in the eastern United States, there is not sufficient evidence that *C. lupus* did not also occur there. Therefore, in reaching this conclusion, the Service has not relied on the best available science. 16 U.S.C. 1533(b)(1).

#### **D. The Eastern United States Represents a Significant Portion of the Species’ Range**

There is no doubt that wolves historically occupied the eastern United States. The only question for this proposal is whether those wolves were *C. lupus* or *C. lycaon*. As we have argued, *C. lycaon* should not be recognized as a separate species. However, even if the Service recognizes *C. lycaon*, it still must protect *C. lupus* in the eastern United

States because the best available science indicates that even if *C. lycaon* did exist, then *C. lupus* coexisted with *C. lycaon* in the eastern United States.

In either case, the eastern United States was and remains a part of the gray wolf's range. As explained above, a species must be protected under the ESA if it is "in danger of extinction throughout all *or a significant portion of its range* . . ." 16 U.S.C. § 1532 (6) (emphasis added). Because the eastern United States is a significant portion of the range of *C. lupus*, it must be protected.

Because the best available science indicates that *C. lupus* lived in the eastern United States, the Service must protect the species because the eastern United States is a significant portion of the species' range.<sup>2</sup> Although the Proposed Rule does not make a finding on this point (because it erroneously concludes that *C. lupus* never lived in the eastern United States), we are concerned that the Service will rely on its draft policy regarding the phrase "significant portion of its range" to ignore the historic range of gray wolves found in the eastern United States. Indeed, the proposed rule largely reiterates this policy in its evaluation of "SPOR" for those areas that it does not recognize as constituting the current range of *C. lupus*.

As outlined in our comments submitted on March 8, 2012, we disagree with the Service's proposed policy that would ignore lost historic range in analyzing whether a portion of a species' range is significant. *See* 78 Fed. Reg. at 35673 (stating "the word 'range' refers to the range in which the species currently exists."); NRDC Comment Letter, Draft Policy on Interpretation of the Phrase "Significant Portion of Its Range," March 8, 2012 (Appendix 6). To ignore habitat where a species once lived but no longer does simply makes no sense given the ESA's purpose to provide expansive protection for species in danger of extinction. *Defenders of Wildlife v. Norton*, 258 F.3d 1136, 1144 (9th Cir. 2001). Indeed, when Congress enacted the ESA in 1973, it expressly extended protection to a species endangered in only a "significant portion of its range," while the two earlier endangered species statutes only protected those species facing total extinction. 239 F. Supp. 2d 9, 19 (D.D.C. 2002), *vacated on other grounds*, 89 Fed. Appx. 273 (D.C. Cir. 2004); *see* Endangered Species Conservation Act, Pub.L. 81-135 § 3(a), 83 Stat. 275 (Dec. 5, 1969) (endangered species are those threatened by "worldwide extinction"); Endangered Species Preservation Act, Pub.L. 89-669 § 1(c), 80 Stat. 926 (Oct. 15, 1966) (endangered species are those whose "existence is endangered because its habitat is threatened with destruction, drastic modification, or severe curtailment, or because of over exploitation, disease, predation, or because of other facts, and that its survival requires assistance"); *see also* H. Rep. No. 412, 93rd Cong., 1 Sess. (1973) (noting that the new definition's expansion to include species in danger of extinction "in *any* portion of its range" represented "a significant shift in the

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<sup>2</sup> As discussed in more detail below, wolves continue to disperse into Maine and other New England states from Canada. We also expect wolves to disperse from the Great Lakes into New York and other eastern states. These areas contain abundant suitable habitat.



definition in existing law which considers a species to be endangered only when it is threatened with worldwide extinction.”) (emphasis added).

Courts examining this issue have also concluded that the Service may not ignore lost historic range when determining whether a portion of a species’ range is significant. In *Defenders of Wildlife v. Norton*, the Ninth Circuit held that “a species can be extinct ‘throughout a significant portion of its range’ if there are major geographical areas in which it is no longer viable but once was.” 258 F.3d 1136, 1145 (9th Cir. 2001). A number of district courts have agreed. See *Greater Yellowstone Coalition, Inc. v. Servheen*, 672 F. Supp. 2d 1105, 1123 (D. Mont. 2009) (relying on Ninth Circuit’s test for “significant”); *Defenders of Wildlife v. Department of the Interior*, 354 F. Supp. 2d 1156, 1167 (D. Or. 2005) (vacating final wolf downlisting rule in eastern and western DPSs for failure to consider historic range outside of core recovery areas); *National Wildlife Federation v. Norton*, 386 F. Supp. 2d 553 (vacating final wolf downlisting rule for eastern and western DPSs for failure to consider historic range); *Defenders of Wildlife v. Norton*, 239 F. Supp. 2d 9, vacated on other grounds, 89 Fed. Appx. 273 (D.C. Cir. 2004) (finding Service’s failure to consider key areas of lynxes’ historic range arbitrary and capricious); *Tucson Herpetological Society v. Kempthorne*, 2007 WL 2023477, at \*4 (D. Ariz. 2007) (“The Secretary may not close his eyes to the possibility that the lost habitat constitutes a “significant portion” of the species’ total range.”).

Further, courts have expressly rejected the exclusive protection of regions where a species is more prevalent when it has historically occurred in additional regions. For example, in *National Wildlife Federation v. Norton*, the court found the Service’s refusal to create a gray wolf eastern DPS because two viable wolf populations already existed in the Northern Rockies and Western Great Lakes arbitrary and capricious. 386 F. Supp. 2d 553 (D. Vt. 2005). The court reasoned that rendering portions of a species’ historical or current range “insignificant and unworthy of stringent protection” by focusing on only a few core areas was contrary to the expansive protection intended by the ESA. *Id.*; see also *Defenders*, 354 F. Supp. 2d at 1166 (finding the final rule arbitrary capricious because it made “all other portions of the wolf’s historical or current range outside of the core gray wolf populations insignificant and unworthy of stringent protection,” which is “contrary to the plain meaning of the ESA phrase “significant portion of its range . . .”).

While some courts have simply relied on the dictionary definition of “significant,” an increasing number have held that an area must be important to the conservation of the species. Compare *Defenders*, 239 F. Supp. 2d at 19 (relying on fact that “significant” is defined in the dictionary as “a noticeably or measurably large amount.”) (quoting Webster’s Ninth Collegiate Dictionary at 1096 (Merriam–Webster Inc.1990)), and *Greater Yellowstone Coalition, Inc. v. Servheen*, 672 F. Supp. 2d 1105 (area must be biologically important to the conservation of the species because of its historical value, uniqueness for feeding, migration, etc., or other reasons). Whether an area is important to the conservation of a species depends on whether it contributes to representation (i.e., the conservation of genetic diversity), resilience (i.e., provides unique, large, or high quality habitat that will enhance the species’ ability to recover from disturbances),

and redundancy (i.e., provides a way to conserve enough areas of the range such that random perturbations in the system impact only a few populations).<sup>3</sup> *Western Watersheds Project v. Ashe*, 2013 WL 2433370, at \*18-\*20 (D. Idaho 2013); *Greater Yellowstone Coalition, Inc.*, 672 F. Supp. 2d at 1124 (upholding Service's definition of "significant" based on a variety of factors, including quantity, quality, distribution, historical value, food availability, and absence of human conflict).

The eastern United States is a "major geographical area" in which *C. lupus* "is no longer viable but once was," and it is also biologically important to the conservation of the species. *Defenders*, 258 F.3d at 1145 (9th Cir. 2001). The 29 states comprising the eastern portion of the country are certainly "significant" by any dictionary definition. *See Defenders*, 239 F. Supp. 2d at 19. In addition, this region is biologically important to *C. lupus* due to its high quality wolf habitat, which would enhance the species' resiliency and redundancy. Indeed, Harrison and Chapin (1998) found that the eastern United States contains tens of thousands of square kilometers of contiguous wolf habitat needed to host a wolf population that will be viable in the long-term—particularly in Maine and New Hampshire, which could likely support 488 - 1,951 wolves, but also in other states, including Vermont and New York. *Id.* Similarly, Mladenloff and Sickley (1998) identified a contiguous area of habitat extending from Maine to New Hampshire and into the northeastern corner of Vermont with the potential for 1,070 wolves. *Id.* The fact that there is such a uniquely vast and uninterrupted swath of land available to support such a large population of wolves would certainly enhance the resilience of *C. lupus* and help ensure their long-term viability.

Further, the eastern United States is important for the redundancy of *C. lupus* as an area to "provide a margin of safety for the species to withstand catastrophic events." *See Western Watersheds*, 2013 WL 2433370, at \*18. The distance between surviving wolf populations in Canada and suitable habitat in northeastern states is relatively small when compared with potential wolf dispersal distances (70-230 km, Harrison and Chapin 1998), suggesting that wolves could disperse into the Northeast and, in fact, documented evidence shows that they have. At least eight large wolves were shot and recovered in the Northeast between 1984 and 2001 (Kays and Feranec 2011). Each of these animals was substantially larger than eastern coyotes and classified as *C. lupus*

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<sup>3</sup> Although the Service discussed the values of redundancy, resiliency, and representation, it applies these principles only to the extent that they are impaired in a way that increases a species' "vulnerability to threats to the point that the overall species would be in danger of extinction." 78 Fed. Reg. 35714. Thus, the Service has cited the importance of these concepts, while implementing a policy that focuses instead on viability alone. This is wrong. Protecting redundancy, resiliency, and representation only when their loss would make a species in danger of extinction does not adequately protect species from the incremental increase in their likelihood of extinction that accompanies the erosion of these values. (Carroll et al. 2010). Nor does it provide the "margin of safety" the Service has said is necessary for species to withstand "catastrophic events." 76 Fed. Reg. 76994.

or a possible wolf hybrid by state and/or federal wildlife officials. *Id.* Isotopic tests suggested that three of these animals had not been raised in captivity. *Id.* In October 2007, a wolf was killed on a farm in Shelburne, Massachusetts, and in March 2008 Thomas J. Healy, head of the Service's Northeast regional office, said that recent DNA tests at the Service's Oregon labs confirmed it was a gray wolf with no indication it was ever held in captivity. Additionally, two wolves were killed in Maine in 1993 and 1996. The Maine Department of Inland Fisheries and Wildlife also notes that tracks and other evidence suggest there may be additional wolf-like canids in the state, but there is no conclusive evidence of reproduction or establishment of packs. The establishment of a *C. lupus* population in the Northeast would be extremely beneficial in helping to protect the species in the event that the isolated populations in New Mexico, the Northern Rockies, and the Western Great Lakes experience catastrophe. This is especially true since many potential recovery areas are too small to maintain a wolf population large enough for long-term viability, especially if these areas are isolated from Canada. (Mladenloff and Sickley 1998).<sup>4</sup>

Because it is a major area in which *C. lupus* historically occurred that enhances both *C. lupus* resiliency and redundancy, the eastern United States constitutes a significant portion of *C. lupus* range. Thus, the Service must protect the species.

#### **E. The Service Should Create a Pacific Northwest DPS and Protect that DPS under the Endangered Species Act**

Finally, if the Service delists wolves nationwide, it should protect wolves in the Pacific Northwest as a Distinct Population Segment (DPS). The Service concludes that it cannot designate a Pacific Northwest DPS because the wolves there do not qualify as a "population." 78 Fed. Reg. 35675, 35711. The ESA regulations define a population as a "group of fish or wildlife in the same taxon below the subspecific level, in common spatial arrangement that interbreed when mature." 50 C.F.R. 17.3. The Service states that for wolves, a population must meet a higher bar, and include "at least 2 breeding pairs of wild wolves successfully raising at least 2 young each year . . . for 2 consecutive years." 78 Fed. Reg. 35711. There is no authority for requiring wolves to meet this definition in order to qualify for listing as a DPS. Moreover, as the Service acknowledges, it lacks sufficient evidence regarding the wolf packs in this region to conclusively determine that they have not raised 2 young for 2 consecutive years. In fact, we know that one breeding pair has successfully raised 2 young for 2 consecutive years, and another breeding pair has been documented. *Id.* Given the fact that there are three packs of wolves in this region, and at least 2 breeding pairs, it is likely the wolves will meet this standard soon, if they have not met it already. As the Proposed Rule itself notes, "it is possible that additional breeding pairs have gone undetected or that the

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<sup>4</sup> This same analysis applies to other areas of the country like Colorado, Utah, and Northern California, all of which contain large areas of suitable wolf habitat, have seen dispersing wolves, and support redundancy, resiliency, and representation of the species.

documented breeding pairs have successfully bred in consecutive years without detection." *Id.*

Having recognized that wolves in the Pacific Northwest are likely to meet its elevated standard for qualifying as a "population," the Service evaluated whether the population qualifies as a DPS. When undertaking a DPS analysis, the Service examines two factors: the discreteness of the population and the population's significance to the taxon as a whole. Policy Regarding the Recognition of Distinct Vertebrate Population Segments Under the Endangered Species Act, 61 Fed. Reg. 4722, (Feb. 7 1996); 76 Fed. Reg. at 26101. If a population meets both of those tests, it can be identified as a DPS. *Id.* Then, a third factor, the DPS's conservation status, is evaluated in relation to the ESA's standards for listing, delisting, or reclassification. *Id.*

Contrary to the Service's conclusion, Pacific Northwest wolves constitute a discrete population. "[A] taxon may be considered discrete if . . . it is delimited by international governmental boundaries within which differences in control of exploitation, management or habitat, conservation status, or regulatory mechanisms exist that are significant . . ." 76 Fed. Reg. at 26102. The boundary to the north between the U.S. and Canada meets these criteria.

In arguing that the Northern Rocky Mountains (NRM) population is discrete, the Service has identified the Canadian border as sufficient to render wolf populations in the United States "discrete" from wolves to the north. 74 Fed. Reg. 15129. The Service writes, "The DPS policy allows us to use international borders to delineate the boundaries of a DPS if there are differences in control of exploitation, conservation status, or regulatory mechanisms between the countries. Significant differences exist in management between US and Canadian wolf populations . . . Therefore, we will continue to use the US Canada border to mark the northern boundary of the DPS due to the difference in control of exploitation, conservation status, and regulatory mechanisms between the two countries." *Id.* This same argument applies to a PNW DPS.

The population is also discrete from wolves to the east. "[A] population segment of a vertebrate taxon may be considered discrete if it . . . is markedly separated from other populations of the same taxon as a consequence of physical, physiological, ecological, or behavioral factors." 76 Fed. Reg. at 26102. Although the Service now argues that "geographic gaps in suitable habitat may not be reflective of long-term barriers to population interchange . . . as we previously implied," 78 Fed. Reg. 35712, there continues to be a significant north-south stretch of unsuitable habitat that separates the suitable wolf habitat in the Pacific Northwest from the Northern Rocky Mountains. Carroll et al. (2003, 2006). This constitutes a physical barrier that markedly separates the two populations.

Also, the Service points out that the wolves that historically occupied the area west of the NRM DPS boundary were biologically discrete and even considered a separate subspecies. "Historical subspecies delineations based on morphology suggest that a biological boundary limiting dispersal or reproductive intermixing likely existed between

eastern and western Oregon and Washington prior to the extirpation of wolves from the region.” 78 Fed. Reg. 35713.

Furthermore, the Service presents evidence that some wolves that now inhabit the Pacific Northwest and would likely inhabit it in the future demonstrate genetic, behavioral, and morphologic differences from the inland NRM population. The proposed rule states, “Moreover, recent genetic, behavioral and morphological data in British Columbia and Alaska show marked separation of coastal and inland wolves . . . which is indicative of ecological processes that may extend into the Pacific Northwest of the United States where climatic and physiographic factors of coastal and inland ecosystems parallel those to the north.” *Id.* The Service argues that if a significant population of wolves is successful in colonizing the Pacific Northwest, then the Service “would expect limited movement and interbreeding of wolves in coastal and inland areas, similar to the historical pattern of differentiation.” *Id.*

According to the Draft Wolf Conservation and Management Plan for Washington, and as outlined in the Proposed Rule, genetic testing of the breeding male and female of the Lookout Pack in north-central Washington suggested the male possessed a mitochondrial haplotype that is unique to the coastal British Columbia region. The origin of the female could not be definitively determined. Results of genetic testing of a wolf in the Teanaway pack indicated that it was likely a descendent of the breeding male and female of the Lookout pack, 78 Fed. Reg. 35713, suggesting that it would at least partially represent the genetic signature for the coastal wolf type.

Studies have indicated that the coastal wolf is genetically and ecologically differentiated due to its occurrence in a unique ecological setting (Leonard et al. 2005, Munoz et al. 2009, Weckworth et al. 2010, vonHoldt et al. 2011). Munoz et al. (2009) found that despite their close proximity to inland wolves, “coastal wolves are highly distinct and representative of a unique ecosystem” (p. 1). The authors conclude that “due to the coastal wolves’ unique ecological, morphological, behavioural and genetic characteristics,” they should be considered a unique evolutionary lineage, or Evolutionary Significant Unit, based on its “historical isolation and adaptive distinctiveness” (p. 12).

Wolves in the PNW therefore meet both of the criteria of discreteness and significance for a DPS. They are discrete from wolves to the north due to the international boundary with Canada, and they are discrete from wolves in the NRM DPS due to physical barriers. They are also ecologically and genetically distinct. Additionally, wolves in the PNW are significant due to their occurrence in a unique ecological setting and because they demonstrate adaptive genetic characteristics that distinguish it from other wolf populations. As the Service has pointed out, wolves in the PNW historically represented a discrete and evolutionarily unique population. At least several wolves currently present in the area west of the NRM DPS represent this unique coastal wolf ecotype. And, finally, the Service itself predicts that a future wolf population in the PNW would demonstrate a similar pattern of ecological discreteness and evolutionary significance as occurred historically.

## Conclusion

The question of whether *C. lycaon* is a valid taxon is an area of current and active research with new studies being published every year. The field of genetics is rapidly advancing, and with that comes the ability to detect greater taxonomic resolution. Additional geographic sampling of particular locations or populations of wolves as well as more historical sampling is currently underway and likely to contribute significantly to our understanding of canid evolution. As such, it is entirely premature for the Service to accept the taxonomic revision proposed in Chambers et al. 2012. In doing so, the Service is trumping the scientific process by prematurely declaring an outcome in an ongoing scientific debate in a way that has significant policy implications for the management of wolves in the United States. Simply put, the conclusion that *C. lycaon* is a separate species is highly controversial and uncertain, while the understanding that *C. lupus* occupied most of the lower 48 states is well accepted. Therefore, the Service should continue to recognize *Canis lupus* as the gray wolf species that historically occupied the United States, including the East.

Sincerely,



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## **Appendices**

Appendix 1. Scientific opposition to Chambers et al. and Midwest rule

Appendix 2. Sustainable Ecosystems Institute Review of Chambers et al.

Appendix 3. Patterson email communication

Appendix 4. Email correspondence referenced in the comment letter including:

- email from Steve Chambers 11/12/2009 titled "Re: Charge to reviewers of wolf taxonomy report"
- email from Gary Frazer via Maricela Constantino 10/16/2009 titled "FW: Wolf taxonomic review" including Talking Points
- email from Ralph Morganweck 10/19/2009 titled "Re: Independent Formal Review of Wolf Taxonomy Report"

Appendix 5. Informal review of Chambers et al.

Appendix 6. NRDC Comment Letter, Draft Policy on Interpretation of the Phrase "Significant Portion of Its Range," March 8, 2012.