UNIVERSITY OF CALIFORNIA, DAVIS

BERKELEY • DAVIS • IRVINE • LOS ANGELES • MERCED • RIVERSIDE • SAN DIEGO • SAN FRANCISCO • SANTA BARBARA • SANTA CRUZ

JOHN MUIR INSTITUTE OF THE ENVIRONMENT

ONE SHIELDS AVENUE
DAVIS, CALIFORNIA 95616-8527

12 September 2014

Michael J. Senn
Nevada Fish and Wildlife Office
4701 North Torrey Pines Drive
Las Vegas, NV 89130

Dear Dr. Senn,

Thank you for the opportunity to submit a peer review of the proposed designation of critical habitat for the Mount Charleston blue butterfly (*Plebejus shasta charlestonensis*).

In your letter of 15 July September 2014, you asked me to "provide comments on the scientific information and data contained within the proposed rule" and to specify whether I "believe that sufficient justification is lacking or specific information was applied incorrectly in reaching specific conclusions." The letter notes that solicitation of independent peer review is intended "to ensure decisions are based on scientifically sound data, assumptions, and analyses." The proposed rule states, "We are seeking comments from knowledgeable individuals with scientific expertise to review our analysis of the best available science and application of that science and to provide any additional scientific information to improve this proposed rule. (pages 5-6, lines 112-115)[1]."

Unfortunately, I do not believe sufficient justification for the proposed designation is presented in the proposed rule. I believe specific information was applied incorrectly in reaching specific conclusions. The proposed rule is not based on the best scientific data available nor on scientifically sound data, assumptions, and analyses.

The proposed rule states that the Service considers "*Astragalus calycosus* var. *calycosus*, *Astragalus lentiginosus* var. *kernensis*, and *Astragalus platytropis* to be the host plants used during Mount Charleston blue butterfly larval development" (page 35, lines 782-784). The Service assumes that the presence of any one of these three species may be interpreted as sufficient for "life-history processes" and "conservation of the species." Information was applied incorrectly in reaching this conclusion. First, the reference to *Astragalus lentiginosus* is incorrect. Throughout the proposed rule, the Service has overlooked the erratum to Andrew et al. (2013), which they provided to reviewers. That report noted, "The plant originally identified as *Astragalus lentiginosus* var. *kernensis* (Kern milkvetch) has now been confirmed to be *Oxytropis oreophila* var. *oreophila* ([m]ountain oxytrope). To correct this mistake, all references to the

[1] Page and line numbers in my review correspond to those in a .docx copy of the proposed rule that was provided to me by the Service. The Service also provided copies of the materials cited in the proposed rule.

plant *Astragalus lentiginosus* var. *kernensis* should be removed . . . and replaced with *Oxytropis oreophila* var. *oreophila*." More importantly, there is little evidence that *Oxytropis oreophila* and *Astragalus platytropis* function as host plants, let alone reliable or commonly used host plants.

Experts on *P. s. charlestonensis* have observed numerous ovipositions by that taxon on *A. calycosus*. Two of those many observations were documented in Austin and Leary (2008). It is incorrect to state, as did Andrew et al. (2013) and Thompson et al. (2014), that reports in the literature that *A. calycosus* is the larval host were based entirely on two observations. Moreover, *A. calycosus* is present at all locations at which *P. s. charlestonensis* has been detected. Application of the precautionary principle alone would lead one to suggest that presence of *A. calycosus* is a necessary biotic attribute of habitat.

The Service's assumption that the three species are functionally equivalent as larval host plants appears to be based on the observation of the Thompson team of oviposition by one female of one egg on each of *A. platytropis* and *Oxytropis oreophila*. Oviposition on a given species of plant, especially for a species of butterfly in which at least some individuals overwinter as eggs or unfed first instar larvae, is not a reliable indication that the plant serves as a larval host plant. It is common for butterflies to oviposit not only on larval host plants but on nearby plants, bare soil, and rocks. The Thompson team's claim that four additional *P. s. charlestonensis* eggs were found on either *A. platytropis* or *O. oreophila* is questionable at best.

It is extremely difficult to identify eggs to species with confidence, especially for relatively inexperienced workers such as those that comprised the Thompson team (even one or two field seasons does not an expert make). Even if one allows that *A. platytropis* or *O. oreophila* occasionally may serve as a host plant, observations of the subspecies over many decades and data on the distribution of the host plant relative to that of the butterfly strongly suggest that *A. calycosus* is by far the more common and reliable host plant. The best scientific data available do not suggest that the three species of plants are equivalent as larval hosts but that *A. calycosus* is necessary for persistence of the butterfly.

References to climate change throughout the proposed rule do not reflect the best science available. On the basis of the information provided, one cannot make a scientifically sound conclusion that climate change poses a threat to the subspecies. Additionally, there is no indication of how the Service or other resource managers might "[manage] for the potential effects of climate change" (page 7, lines 140-141). The references to climate change seem cursory and obligatory rather than thoughtful, defensible, and specific to the biology of the subspecies and its potential management.

The proposed rule does not cite a recent, peer reviewed reference that is specific to the southwestern United States: Garfin, G., A. Jardine, R. Merideth, M. Black, and J. Overpeck, editors. 2013. Assessment of climate change in the Southwest United States: a technical report prepared for the U.S. National Climate Assessment. Southwest Climate Alliance, Tucson, Arizona. www.swcarr.arizona.edu/.

It is true that throughout the southwestern United States, mean temperatures are projected to increase and extreme precipitation (drought and storms) is projected to become more frequent. However, not all species will be threatened by these changes. The Service asserts, "climate change will impact the Mount Charleston blue butterfly and its high-elevation habitat through

predicted increases in extreme precipitation and drought" (page 40, lines 910-912). However, no mechanisms by which the subspecies or its resources may be affected directly or indirectly by changes in temperature and extreme precipitation are hypothesized. One could say that projected changes in climate might affect any species, but without reference to mechanisms, such an assertion is neither rigorous nor helpful. Moreover, no consideration is given to previous climatic fluctuations through which the subspecies has persisted. The summary for decision-makers in Garfin et al. (2013) notes, "Recent drought has been unusually severe relative to droughts of the last century, but some droughts in the paleoclimate record were much more severe. The areal extent of drought over the Southwest during 2001–2010 was the second largest observed for any decade from 1901 to 2010. However, the most severe and sustained droughts during 1901–2010 were exceeded in severity and duration by multiple drought events in the preceding 2,000 years."

Furthermore, local patterns at high elevation in the Spring Mountains are not necessarily the same as those averaged across the region. Events consistent with projected changes in climate are occurring worldwide, yet weather and climate are sufficiently variable that causation is quite difficult to attribute. Page 40, lines 909-910 cites Redmond (2007). This source, provided by the Service to reviewers, was an email that states, in its entirety, "The ongoing drought cannot be definitively tied to climate change in my opinion, but climate change is not precluded, either. As for more intense precip events, even though a state record was recently set at Mt Charleston, same story. The continued warming over the past 30 years of about +2 F [in southern Nevada] has a fair likelihood of a tie to climate change."

The document does not refer to the threats to the subspecies and its habitat posed by fuels treatment projects or infrastructure maintenance by federal agencies. Therefore, the proposed rule overlooks expert observations and inferences and scientifically sound assumptions. Boyd and Murphy (2008) commented, "despite the need to move quickly to reduce forest biomass and canopy cover, forest mangers cannot overlook the extreme sensitivity that will be required of the treatments. Common forest fuels treatments, including mastication and pile burning, would essentially destroy portions of the forb understory, and render previously suitable habitat areas unsuitable. Substantial substrate disturbance will certainly lead to weedy plant invasions. Direct disturbance of larval and potential nectar resources for the butterfly should be avoided to the extent practicable – otherwise their ability to support the blue butterfly will be compromised." In 2010, despite this clear guidance, the majority of known patches of host plants in Lee Canyon below the Las Vegas Ski and Snowboard Result were covered with masticated vegetation. Maintenance of campgrounds in upper Lee Canyon also reduced the abundance and condition of resources for *P. s. charlestonensis* in patches of host plants that were known to the USDA Forest Service and the US Fish and Wildlife Service.

As detailed below, numerous citations in the proposed rule do not correctly represent the original sources. Moreover, there are many instances of verbatim quotations from original sources that are not clearly indicated as such. A parenthetical citation implies either a paraphrase or evidence consistent with an author's statement or inference. Virtually all professions expect verbatim quotations to be indicated with quotation marks. Failure to do so falls somewhere along the gradient from unprofessional to plagiarism, and is highly disappointing in a proposed rule by a public trust agency. Many other errors are scattered throughout the document. Although they may not have affected the proposed rule per se, these errors call into question whether those who

drafted the proposed rule had the capacity or were given the time and resources to carefully assess the scientific information relevant to their charge.

Unfortunately, the "best scientific and commercial data available" standard is not met in the proposed rule. The proposed rule seems to conflate *available* with *best available*. The best available information on *P. s. charlestonensis* that can inform conservation of the subspecies is contained within Weiss et al. (1995 and 1997), DataSmiths et al. (2007), and especially Boyd and Austin (1999) and Boyd and Murphy (2008).

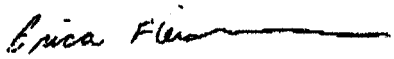
The proposed rule relies heavily on Thompson et al. (2014), which is essentially the same as Andrew et al. (2013); Andrew et al. covers a narrower time period, but the personnel were substantially similar and its content largely is contained within Thompson et al. (2014). The Service's identification of primary constituent elements of critical habitat mirrors (but does not cite in that section) the habitat characteristics suggested by Thompson et al., which are not supported by scientifically sound data, assumptions, and analyses. In my informed opinion, Thompson et al.'s work would not pass a formal peer review, such as that by a reputable journal, without substantial revision of all aspects of the work. Application of specific information from this work was applied incorrectly by the Service in reaching specific conclusions in the proposed rule.

I am among those who were asked by Thompson to review an earlier version of his team's report on autecology of the Spring Mountains. On 17 September 2013, I provided extensive comments on the report to Thompson, copied to Jim Hurja and Kimberly O'Connor of the USDA Forest Service. The response to the comments was disappointing. On 23 September 2013, Thompson sent me an email that included, "I spoke with USFS personnel and we agreed that after completion of a substantial revision (already in progress before receipt of your review) it would be beneficial to take you up on your offer and have you look at the Final Report again. I will be in contact in the coming weeks to determine a good time to contact you by phone, at your convenience. Revision of the Final Report is expected to be completed by the end of October." I received no further communication from Thompson until 7 August 2014, after I communicated my dissatisfaction with the review process to Corey Kallstrom when I was asked to review the proposed critical habitat designation. On 7 August 2014, Thompson wrote, "We were able to address a majority of your editing suggestions. I had intended to use multi-model inference and model averaging for logistic regressions in the Final Report (replacing step-wise regressions) so by the time I got your review I had already completed those analyses. Your suggestion to reorganize sections, place results before recommendations, and enhance the connection between results, literature, and recommendations were also implemented. The USFS, however, had stipulated the inclusion of all data tables and descriptions of observations so we were not able to follow your suggestion to condense results and text despite our recognition that the text is lengthy relative to total information content. In summary, my coworkers, USFS and USFWS are very appreciative of the time and effort you devoted in providing feedback which substantially improved the document." To make an already long story short, and noting my extensive experience as an editor of peer-reviewed journals, I do not feel that my comments were addressed adequately. It is my understanding that responses to comments from other experts on the subspecies, including Bruce Boyd, also were left to Thompson's discretion and were not addressed to the reviewers' or commenters' satisfaction.

The type locality for *P. s. charlestonensis* (Austin 1980), commonly referenced as Lee Canyon Meadow, should be included within the critical habitat designation for unit 2. Other areas within the Lee Canyon unit (page 51, lines 1151-1153) should not be considered for removal until the current distribution, abundance, and condition of larval hosts, nectar sources, and other environmental characteristics consistent with occupancy have been assessed. Current habitat quality in these areas was reduced by the fuels treatments and campground maintenance noted above, and it may be possible for these areas to recover given special management considerations and protection. If special management is being presented as an option to reduce potential effects of climate change, special management certainly should be considered as a means to reduce potential effects of past management by federal agencies trusted with conserving the subspecies.

Again, thank you for the opportunity to submit a peer review of the proposed rule. If you have questions or if I can be of further assistance, please contact me at efleishman@ucdavis.edu or (530) 754-9167.

Sincerely,



Erica Fleishman, Ph.D.

Background

page 13, line 283. "The Mount Charleston blue butterfly is a distinct subspecies."

This statement is puzzling. First, a subspecies implies distinct (an indistinct subspecies would make no sense). Second, it should be clarified that the designation of the subspecies was based on morphology.

page 13, line 292 onward. "The first mention of the Mount Charleston blue butterfly as a unique taxon was in 1928 by Garth (p. 93), who recognized it as distinct from the species Shasta blue butterfly (Austin 1980, p. 20)."

This statement is a misrepresentation of the literature. Garth did not indicate that *charlestonensis* (i.e., the population of *P. shasta* in the Spring Mountains) was distinct from *P. shasta*. Instead, Garth noted that specimens of *P. shasta* from Charleston Peak were darker than *P. shasta* form *comstocki*. The latter form, the nominate *shasta* in California (i.e., not the species-level taxon), is the taxon that was referenced by Austin (1980). Much of the text in this paragraph is copied with little modification from Austin (1980), with the references included by Austin but not with the clarity of Austin's writing.

It is important to understand Ferris' rationale for including Spring Mountains specimens within *P. shasta minnehaha*: "I can see no reason . . . to erect any new taxa to describe local colonies within the geographic area occupied by *minnehaha*. There are too many variables involved, including possible changes in environmental conditions on a yearly basis which seem to have some influence upon facies. Additionally, *minnehaha* occurs in rather isolated colonies and one would not expect any substantial interchange of individuals between colonies. This situation appears to have fostered the development of some local phenotypes, but these should be considered as local forms only rather than separate subspecies."

page 13, line 298. "Austin asserted that Ferris had not included specimens from the Sierra Nevada . . . ."

This is not an assertion but fact. Moreover, "did not include" implies a decision, whereas Ferris simply did not have the specimens.

page 14, lines 321-322. "which when viewed up close distinguishes it from other small, blue butterflies occurring in the Spring Mountains (Austin 1980, pp. 20, 23; Boyd and Austin 1999, p. 44)."

This statement misrepresents the literature. Austin was referring to morphological differentiation among subspecies of *shasta*. Boyd and Austin (1991) noted that "a dark bar at the end of the dorsal hindwing discal cell" is not present on *Icaricia acmon*. The latter certainly is true, but there are many, and in my opinion easier, ways to differentiate the taxa at close range.

page 16, line 358 onward. "Observations indicate that above- or below-average precipitation, coupled with above- or below-average temperatures, influence the phenology of this subspecies (Weiss *et al*. 1997, pp. 2–3 and 32; Boyd and Austin 1999, p. 8) and are likely responsible for the fluctuation in population numbers from year to year (Weiss *et al*. 1997, pp. 2–3 and 31–32)."

First, fluctuations in temperature and precipitation affect the phenology of virtually all butterflies; one would not need direct observations to make this assumption. Second, averages typically are long-term averages (e.g., over 30 years); in few years do weather patterns approach

statistical averages. Third, any functional relations between weather and abundance are unknown.

page 16, lines 364-365. "Like most butterfly species, the Mount Charleston blue butterfly is dependent on specific plant species" for nectar.

This is incorrect. Many species of butterflies are relative generalists with respect to nectar whereas they are relative specialists with respect to host plants.

page 17, lines 380-381. "we presume information on the diapause of the closely related Shasta blue butterfly . . ."

P. shasta charlestonensis **is** a Shasta blue butterfly.

pages 17-18, lines 388 onward. "recent observations of late summer hatched and overwintering unhatched eggs of the Mount Charleston blue butterfly eggs [sic] laid in the Spring Mountains may indicate that it has an environmentally cued and mixed diapause life cycle"

It is highly uncertain whether these eggs were eggs of *P. s. charlestonensis*. The Thompson team observed two eggs being laid. They interpreted all other eggs in the areas they surveyed as those of *P. s. charlestonensis*. As noted above, it is extremely difficult to identify eggs to species with confidence, especially for relatively inexperienced workers such as those that comprised the Thompson team (even one or two field seasons does not an expert make).

An observation of some hatched and some unhatched eggs in the same area at the same time may not provide evidence that some individuals will overwinter in the egg stage. Eggs in the same cluster may not hatch simultaneously. Additionally, at slightly coarser spatial resolution, there may be considerable variation in hatching phenology as a function of solar insolation and other environmental variables.

All butterflies have "environmentally cued" life cycles, and it is likely that many more species than historically assumed have some plasticity with respect to overwintering stages or durations.

page 18, lines 299-401. "Boyd and Murphy (2008, p. 21) suggest the Mount Charleston blue butterfly may be able to delay maturation during drought or the shortened growing seasons that follow winters with heavy snowfall and late snowmelt by remaining as eggs."

This is only part of what Boyd and Murphy (2008) said: "[The subspecies] may be able to delay maturation during drought or the shortened growing seasons that follow winters with heavy snowfall and late snowmelt by remaining as eggs through one or more years, or returning to diapause as larvae, perhaps even more than once." The point Boyd and Murphy (2008) were making was not that eggs can overwinter, but that the subspecies may be able to remain in diapause for more than one winter.

page 18, lines 410-411. "Boyd and Austin (1999, pp. 17 and 53) suggest this [a metapopulation structure] is true of the Mount Charleston blue butterfly."

Boyd and Austin (1999) did not suggest that *P. s. charlestonensis* occurs as a metapopulation. Page 17 of that publication simply notes that small populations may not persist in all years. Page 53 notes, "The population dynamics and metapopulation structure . . . need to be investigated . . . it is not known if the smaller colonies are persistent over time or are periodically subject to extirpation and recolonization." Additionally, extirpation and

recolonization of small populations and persistence of large populations may reflect a source-sink dynamics or a patchy population structure rather than a classically defined metapopulation structure.

page 19, lines 428-434. A multiple-year diapause, if it exists in this subspecies, would be one of many ways in which the subspecies has adapted to environmental variability. However, a multiple-year diapause alone is unlikely to "account for periodic high numbers of butterflies observed at more sites in years with favorable conditions." Any number of factors likely contribute to fluctuations in abundance, including condition and phenology of larval host plants and adult nectar sources, parasite loads, and other sources of mortality of juvenile life stages.

Critical Habitat

page 21-22, lines 480 onward. " . . . critical habitat designations identify, to the extent known using the best scientific and commercial data available, those physical or biological features that are essential to the conservation of the subspecies (such as space, food, cover, and protected habitat)."

Protected habitat is not a physical or biological feature. It is a regulatory designation.

pages 24-25, lines 550-553. "We also determined from peer and public review of the proposed listing rule that identification and mapping of critical habitat is not expected to exacerbate the threat of collection, because location information is available on the internet and the closure order reduces the threat of collection."

This misrepresents the potential threat of collection to the subspecies and the extent to which the review process supported the Service's contention that collection is a threat. The Service continues to rely on faith and, essentially, hearsay in its assertions that collection is a major threat.

page 26, lines 587-590. "in determining which areas within the geographical area occupied by the species at the time of listing to designate as critical habitat, we consider the physical or biological features that are essential to the conservation of the species and which may require special management considerations or protection."

"essential to the conservation of the species" is an overstatement for many of the features described in the proposed rule.

page 28, lines 624-625. "the open forest conditions may also exist as a function of an area's ecological system"

This statement makes little biological sense; after consulting the citations, my best guess it that it means "some habitat is above treeline." The page reference in the proposed rule leads to a so-called biophysical setting model for Rocky Mountain alpine fell-field, which is a land-cover class developed by the Landscape Fire and Resource Management Planning Tools Project (LANDFIRE). LANDFIRE refers to its land-cover classes as "biophysical settings" (defined on the LANDFIRE website as "the vegetation that may have been dominant on the landscape prior to Euro-American settlement and is based on both the current biophysical environment and an approximation of the historical disturbance regime") and Provencher refers to the land-cover class as an "ecological system." The land-cover class is above treeline (3400-3700 m according

to Provencher) and therefore would overlap the highest elevations typically occupied by *P. s. charlestonensis*.

page 28, lines 636-639. "we identify flat or gently sloping areas between 2,500 m (8,200 ft) and 3,500 m (11,500 ft) elevation in the Spring Mountains as a physical or biological feature essential to the Mount Charleston blue butterfly for space for individual and population growth and for normal behavior."

This is not quite accurate. As Boyd and Murphy (2008) explained, " 'Ridges' have been described as providing habitat for shasta blue butterflies by some investigators, and frequently cited descriptions of *Icaricia shasta* habitats also include flats, slopes, and hilltops. These designations might be misleading. Many species of butterflies congregate at elevated locations with open exposures, a common strategy used by butterflies for locating conspecific mates; furthermore, most butterflies in temperate zone situations avoid dense cover. [Therefore] many collectors and observers disproportionately target open, elevated locations in searches for butterflies, creating increased opportunities to encounter *Icaricia shasta* in such situations, and in turn potentially over emphasizing the importance of those situations in habitat selection by the species."

page 29, line 653 onward. "most frequently these species [from which *P. s. charlestonensis* takes nectar] are *Erigeron clokeyi* (Clokey's fleabane), *Eriogonum umbellatum* var. *versicolor* (sulphur-flower buckwheat), *Hymenoxys cooperi* (Cooper rubberweed), and *Hymenoxys lemmonii* (Lemmon bitterweed) (Weiss *et al*. 1997, p. 11; Boyd and Murphy 2008, pp. 13, 16; Pinyon 2011, p. 17; Andrew 2013, pp. 3–4; Thompson *et al*. 2014, pp. 117–118)."

This is a misrepresentation of the literature. It suggests that each of the cited authors observed the subspecies taking nectar from this suite of plants. Weiss et al. (1997) observed nectar being taken from *Hymenoxys lemmonii*, *Aster* sp. (not noted by the Service), and *Erigeron* sp. (not noted by the Service). Boyd and Murphy (2008) did not cite any observations of *P. s. charlestonensis* feeding on nectar. Instead, Boyd and Murphy (2008) listed 20 species that they suggested may provide nectar given observations of other Lycaenids. Boyd and Murphy suggested that the ten species most likely to serve as nectar sources were *Antennaria rosea*, *Cryptantha angustifolia*, *Ericameria nauseosa* ssp., *Erigeron clokeyi*, *E. flagellaris*, *Guiterrezia sarothrae*, *H. cooperi*, *Monardella odoratissima*, *Petradoria pumila*, and *Potentilla concinna*. Pinyon (2011) observed *P. s. charlestonensis* taking nectar *Hymenoxys lemmonii* and *Erigeron clokeyi*. Pages 3 and 4 of Andrew et al. (2013) do not mention nectar sources. Thompson et al. observed three males taking nectar from *Eriogonum umbellatum* at one location on one day (this sample does not qualify *E. umbellatum* as a "frequently" visited species). Thompson et al. suggested that *E. clokeyi* was the primary source of nectar, with *Hymenoxys* spp. as a potential secondary source.

It is not true (line 653) that *P. s. charlestonensis* has been "documented using as nectar plants" *Antennaria rosea*, *Cryptantha angustifolia* (or *Cryptantha* spp.), *Ericameria nauseosa* ssp., *Erigeron flagellaris*, *Guiterrezia sarothrae*, *Monardella odoratissima*, *Petradoria pumila*, and *Potentilla concinna*.

page 30, lines 677-679. "*Astragalus calycosus* var. *calycosus, Astragalus lentiginosus* var. *kernensis*, and *Astragalus platytropis* are all considered larval host plants for the Mount Charleston blue butterfly."

This statement is not defensible. See the cover letter.

page 33, line 741. P. s. charlestonensis has a "short lifespan" and page 33, lines 746-747: "its lifespan . . . may range from 2 to 12 days."

This statement is a misrepresentation of biological reality. An individual lives for a year or more. The adult stage should not be confused with the lifespan of the organism.

page 34, line 762 again references "the closely related Shasta blue butterfly." *P. s. charlestonensis* is not closely related to *P. shasta*—it is a *P. shasta*.

page 35, lines 797 and onward.

It is reasonable to assume that fire in the subspecies' habitat historically was infrequent. However, the discussion of fire regimes throughout this section misrepresents the cited literature, and some of the cited literature is of limited clarity and questionable defensibility. In other words, the inference may be correct, but the evidence on which the inference was based is weak.

The proposed rule confuses fire regime groups and fire regime condition classes. Page 15 of Barrett et al. (2010) lists five fire regime groups, which indeed are based on frequency and severity. The three (not five) fire regime condition classes summarized in Barrett et al. (2010) are not based on fire frequency and fire severity. Instead, they are based on what is assumed to be their consistency with the assumed natural or historical range of variation.

Citations to Provencher (2008, Appendix II) do not clarify which of the so-called biophysical setting models are assumed to describe the habitat of the butterfly. As noted above, Rocky Mountain alpine fell-field has relatively little overlap with the butterfly's habitat. The model for this land-cover class has high uncertainty given that it appears to have been based almost entirely on one person's opinion. Without greater clarity, it is not possible to crosswalk the proposed rule with tables and appendix II in Provencher (2008).

Horses in the Spring Mountains are feral, not wild, and should be acknowledged honestly as such.

The Service notes, "we are required to identify the physical or biological features essential to the conservation of Mount Charleston blue butterfly in areas occupied at the time of listing, focusing on the features' primary constituent elements. We consider primary constituent elements to be those specific elements of the physical or biological features that provide for a species' life-history processes and are essential to the conservation of the species."

Many of the so-called primary constituent elements as determined by the service are not based on scientifically sound data, assumptions, and analyses. Citations throughout this section are minimal. However, the Service appears to have relied quite heavily on the "proposed habitat criteria" of Thompson et al. (2014). Thompson et al. (2014) stated that their proposed criteria were "for the most part, similar to what has been reported in previous work and can be grouped into four elements (the first three match Boyd and Murphy's [2008] description of habitat)." As detailed below, the latter statement is incorrect.

Additionally, Thompson et al. suggest that the plant densities they offer as criteria "generally relate to estimates of the minimum number sufficient to sustain butterflies under otherwise ideal conditions with respect to factors such as soil moisture and length of growing season." The densities illustrate the fallacy of illusory precision. The estimates in Thompson et

al. (2014) are not based on scientifically sound data, assumptions, and analyses. It would be far more defensible to indicate ranges of densities with means and variances. Thompson et al. (2014) and the proposed rule might lead one to perceive that regulators desired numbers and the authors provided numbers regardless of whether the numbers were defensible.

I compared each of Thompson et al.'s first three criteria with Boyd and Murphy's description of habitat.

"1. The occurrence of any one of the three species of *Astragalus* or *Oxytropis* larval host plants at densities above 2 plants per m^2." Neither the identification of larval hosts nor the density is consistent with Boyd and Murphy. Boyd and Murphy referenced *Astragalus calycosus*, which, as detailed above, is the only species that reliably can be assumed to serve as a host plant. Moreover, Boyd and Murphy noted, "The availability of the known larval host plant . . . appears not to be related to the decline of the butterfly; thousands of host plants survive at historical sites from which the butterfly has disappeared—many millions exist in the Spring Mountains." Boyd and Murphy also commented, "the larval host plant exists in very large numbers, far exceeding amounts necessary to support butterfly populations at each survey site"

"2. The nearby (typically within 10 meters) occurrence of nectar plants (*Erigeron* sp., *Hymenoxys* sp., *Gutierrezia* sp., or *Eriogonum umbellatum* (other species are possible; Boyd and Murphy 2008) in a combination that, singly or collectively, provides nectar in mid-July and early August such that nectar is available during the flight period of adults. The density of nectar plants required to sustain a population depends in part on the plant species with small nectar plants such as *Erigeron clokeyi* or *Astragalus* sp. (in flower) at densities above 2 per m^2, and larger, taller plants such as *Hymenoxys* sp., *Gutierrezia sarothrae*, or *Eriogonum umbellatum* at densities above 0.1 plant per m^2 (1 plant per 10 m^2) . . . ." Boyd and Murphy did not comment on densities of nectar sources. Boyd and Murphy referenced observations of *P. s. charlestonensis* on *Hymenoxys* and *Erigeron*. They suggested that "any co-occurring, nectar-producing plant species that offers the [species] a landing platform and inflorescences (or flowers) that are accessible to the short proboscides of the [species] may be used." They further suggested that nectar sources should occur within tens of meters of hostplants for a location to be considered habitat. They referenced 20 species of plants that might serve as nectar sources. Boyd and Murphy noted, "Although dramatic differences in nectar species composition was observed between sites, all sites supported multiple suspected nectar species in numbers that suggest that they would be ample to support a resident local population of the Mount Charleston blue butterfly.

"3. Little to no tree canopy cover with tree densities below 0.05 per m^2 and tree canopy cover below 25% (for a 100 m^2 plot) such that nectar and larval host plant substrates have relatively high solar insolation, warm temperatures, and long growing seasons that enhance larval growth conditions and adult activity periods. It is likely that the benefits of low tree cover decrease with elevation such that the 50% tree cover category that Weiss et al. (1997) recorded for plots within LVSSR that had approximately 1-5 larval host plants per m^2 would represent the maximum tree canopy cover for low elevation populations." This criterion again goes well beyond Boyd and Murphy (2008) and well into speculation. Boyd and Murphy stated that to qualify as habitat, a location must have "a physical setting that optimizes solar exposure" and that areas occupied by the subspecies had "very limited or no canopy cover or shading." The statement of Thompson et al. about "benefits of low tree cover" is not clear, especially given that higher elevations, often approaching treeline, typically have less tree cover than lower elevations.

The Service suggests, citing only its "current knowledge of the physical or biological features and habitat characteristics required to sustain the species' life-history processes," that the following densities are essential to the conservation of *P. s. charlestonensis*.

- Tree cover ≤ 50%. The basis for this determination is unclear. Weiss et al. 1997 recorded "low" (< 1/ m^2) densities of *A. calycosus* in locations with canopy cover of 0, < 50%, and > 50% (the three classes they delineated), "moderate" (1-5 / m^2) densities of *A. calycosus* in locations with canopy cover of 0 and < 50%, and "dense" *A. calycosus* (> 5 / m^2) in locations with canopy cover of 0. Especially given that Weiss et al. (1997) did not record tree cover as a continuous variable, Thompson et al. (2014)'s comment, "the 50% tree cover category that Weiss et al. (1997) recorded for plots within LVSSR that had approximately 1-5 larval host plants per m^2 would represent the maximum tree canopy cover for low elevation populations," is based on speculation rather than data. Although I understand regulators' desire for thresholds, presenting a quantitative threshold in this case implies greater certainty about relations between the subspecies, its larval host, and canopy cover than exists.

- Forbs and grasses < 5 / m^2 if heights are ≥ 15 cm. This seems to be derived directly from Thompson et al. (2014), as described above.

- One or more of the three putative host plants at > 2 / m^2. This seems to be derived directly from Thompson et al. (2014), as described above. Weiss et al. (1997) found that hostplant densities in occupied patches ranged from 1 to 5 per m^2.

- Densities > 2 / m^2 for the taller and > 1 / m^2 for the shorter of the following putative nectar sources: *Erigeron clokeyi*, *Hymenoxys lemmonii*, *Hymenoxys cooperi*, *Eriogonum umbellatum* var. *versicolor*, *Antennaria rosea*, *Cryptantha* sp., *Ericameria nauseosa* ssp., *Erigeron flagellaris*, *Guiterrezia sarothrae*, *Monardella odoratissima*, *Petradoria pumila* var. *pumila*, *Potentilla concinna* var. *concinna*. This seems to be derived directly from Thompson et al. (2014), as described above.

The Service also suggests that the following heights are essential to the conservation of the subspecies.

- Forbs and grasses < 15 cm, or if ≥ 15 cm, density less than 5 / m^2. This seems to be derived directly from Thompson et al. (2014), as described above. Thompson et al. say, "Grass height at locations currently occupied or presumed occupied by *P. s. charlestonensis* is consistently less than 15 cm in height, and, if taller bunch-like grasses, tall forbs, or shrubs are present, the tall plant density is below 5 per m^2 (Figure 4.2.12 and Figure 4.2.13)." Figures 4.2.12 and 4.2.13 are photographs of occupied areas; the captions to these figures comment on grass height and density. This is not strong evidence.

pages 43, lines 983-984 and 986-987. "Management activities that could ameliorate [threats to the species and its habitat] include . . . monitoring of ongoing habitat loss and nonnative plant invasion"

This is incorrect. monitoring in and of itself does not ameliorate threats.

page 74, lines 1067-1075. Among the criteria for identifying a location as habitat were " . . . (b) delineated habitat that was rated by the investigator (Pinyon 2011, pp. 1–39) as either "moderate" or "good" quality; and (c) contained both larval host and nectar plants, or only larval host plants. It was inferred that nectar plants would also be present in areas where only larval host plants were detected and butterflies were observed since both larval host and nectar plants must be in close proximity for Mount Charleston blue butterflies to be present."

Pinyon (2011) based habitat-quality designations on "presence of larval host plants, nectar hosts, ground cover and canopy density (visual estimate), and terrain features." This qualitative work cannot be repeated, and therefore the criterion not highly defensible. Additionally, one cannot assume, especially given uncertainty in the distances that *P. s. charlestonensis* can move, that unobserved nectar sources can be assumed present.

page 48, lines 1091-1092. "Studies suggest that mobility in closely related butterfly species is similar (Burke *et al*. 2011, p. 2284)."

This statement contains some truth, but misrepresents the literature. Burke et al. (2011) stated, "Mobility [was] more similar among closely- than distantly-related taxa." Moreover, their work was based on the opinions of naturalists rather than on empirical data.

page 48, lines 1095-1097. "[*Plebejus icarioides missionensis*] is a subspecies of the closely related Boisduval's blue butterfly (*Plebejus icarioides*) (Gompert *et al*. 2008, Figure 2; Burke *et al*. 2011, Supplementary File S4)."

Again, although *P. shasta* and *P. icarioides* may be fairly close relatives, the citation misrepresents the literature. Gompert et al.'s work included only three species of New World *Plebejus*. Among those three species, *P. shasta* was more closely related to *P. icarioides* than to *P. saepiolus* or *P. argus*. The objective of the study, however, was not to create a phylogeny but to examine relations among Old World and New World *Lycaeides*. Gompert et al. cite Kandul et al. (2004) that *Plebejus* are closely related to *Lycaeides*, but the statement in the proposed rule goes too far. It is unclear why Burke et al. (2011)—which was not a phylogenetic study—is cited here.