

9 April 2019

USFWS Antioch Dunes Recovery Report

Review: Julian Dupuis, Junior Faculty, University of Hawaii

This document proposes an addendum of recovery criteria to the recovery plan for *Apodemia mormo langei* (LMB), *Oenothera deltooides* subsp. *howellii* (ADEP), and *Erysimum capitatum* var. *angustatum* (CCW), and this review serves to evaluate the proposal given the three questions: 1) Have we assembled and considered the best available scientific and commercial information relevant to this species? 2) Is our analysis of this information correct? 3) Are our scientific conclusions reasonable in light of this information? Other than a handful of specific comments (see below), and a few broader comments about practicality of implementing some of the recovery criteria, I would say the answer to the above questions is mostly yes. I am a lepidopterist, so my comments are focused on the LMB portion of this recovery plan. In the LMB system in particular, we don't necessarily understand many of the finer demographic details required to make detailed recovery plans (as opposed to some mammal/bird systems), and in that light I think the proposed recovery criteria are mostly valid and appropriate.

I don't have any specific critiques of the calculations of minimum viable population size, persistence probability, etc., although I recognize the general contention of the use of/reliance on some of those methods (e.g. the correspondence between Flather et al. and Brook et al. 2011, TREE). For LMB, the three Lepidoptera included in Traill et al.'s (2007) meta-analysis are at least relatively sedentary species that inhabit small patches of habitat (similarly to LMB), which would presumably increase the accuracy of extending Traill et al.'s analysis to this system. My only comment for these calculations is that it would be interesting to see the different estimates of MPV if only the three butterflies from Traill et al.'s analysis were considered (compared to all six insects that did represent much different estimates than the butterflies, e.g. the Orthoptera estimate is at least an order of magnitude greater than any of the Lepidoptera).

One broader critique that I have lies in the practicality of applying some of the recovery actions. For the ADEP and CCW, other populations exist besides the one at Antioch Dunes, so the possibility of establishing additional self-sustaining populations is plausible. However, for LMB, it only occurs in the Stamm and Sardis units as two population units. In trying to imagine how three or five populations would be established at separate, managed locations (ignoring the difficulty in objectively defining that concept, e.g. footnote on page 19), I am at a bit of a loss. Furthermore, there is no direct reference to actions that would provide that kind of habitat. Is there thought of establishing new dune habitat somewhere, or introducing LBM into other suitable habitat? Aside from taking such actions, I do not see how it would be possible to establish more independent populations within the confines of ADNWR. On the other hand, perhaps that level of practicality is less of a concern, and these criteria are more of a formality that comes along with protecting ADNWR. If that is the case, then this critique matters less; however, in that case, should that objective should be stated more explicitly? Alternatively, if this addendum is simply not the place for such recommendations (I am not overly familiar with the formatting/content of these USFWS documents), then this critique can be completely ignored.

Specific points:

Page 1. Why is the common name used first as the primary name for LMB, but not for ADEP and CCW?

Page 7. Although ITIS considers *A. mormo dialeuca* a valid name, most authors consider this entity as *A. virgulti dialecoides*, e.g. Pelham 2012 A catalogue of the butterflies of the United States and Canada: <https://butterfliesofamerica.com/US-Can-Cat-1-30-2011.htm>. This would decrease the number of subspecies of *A. mormo* to 6.

Page 12/13. The phrase “However, despite the improvement, the overall population is still not considered stable or self-sustaining due to the overall low population numbers, low redundancy of populations, and continuing and increasing threats” is used to describe both of the plants in this report. Is there any standard with regard to what these phrases mean in terms of raw population numbers, or some other objective criterion that supports this assertion? i.e. what is the upper limit to “low population numbers” or can this phrasing be related to the recovery criteria introduced later in the document? Or is this meant to be a more subjective statement given the long-term history of these populations?

Page 15. In comparison to the 2008 5-year review, the threats from disease or predation are similarly presented for the ADEP and CCW, but for LMB the threats noted in the 2008 publication are omitted from the current publication (this also applies to Tables 4-6).

Page 19 (and Table 4, page 31). What constitutes “vegetation monitoring” in this regard? As this paragraph is mostly concerning the host plant, I assume this means some kind of census work, but would it include census of all plants or only buckwheat and potential nectar sources?

Page 22. Metalmarks belong to the family Riodinidae, not Lycaenidae. Regardless, both families are generally sedentary, as described.

Page 31. Reference to factor C is included in the table in the top cell of “Five Factors Ameliorated”, although omitted below in the text.

Additionally, I am unsure how recovery objectives 1a, 1b, and 1c really apply to the recovery criteria of establishing three/five populations. Those recovery objectives (specifically 1a) really only apply to a maximum of two possible populations (the two units of Antioch), so where are those other populations going to come from? Are there efforts underway to introduce LMB to other dune habitat outside of ADNWR?

Page 47. Although no revisions directly threaten the taxonomic validity of *A. mormo langei*, the taxonomy of the *A. mormo* species complex is far from resolved (see Proshek et al. 2015 BMC Evol Biol, Dupuis & Oliver et al. 2018 Cons Genet). Furthermore, many of the defining morphological characteristics of LMB (medial orange scaling on the hindwing, orange scaling within the distal cell of the hindwing basal spot & forewing spot) have been observed in individuals in other proximate locations, such as at Monocline Ridge (same references). Those specimens are not necessarily contemporary ones (1995, although I know there’s been talk by the USFWS to do contemporary surveys), so it’s hard to say if similar morphological polymorphism

exists today, but their existence begs the important question of “what are those butterflies in the context of the whole species group?”. If there are butterflies that look like LMB in other locations, and are classified as LMB, then that opens a whole other can of worms. This taxonomic uncertainty doesn’t really change the focus of this document, and I am not suggesting that it needs to be discussed at length. However, I think it should be brought up here briefly, to at least document that taxonomic uncertainty exists in the general realm of this subspecies.