Review of "Range-Wide Status of Colorado River Cutthroat Trout (*Oncorhynchus clarkia pleuriticus*) by Hirsch et al.

Overview – This report represents an impressive collaborative effort to address the very challenging problem of assessing the conservation status of a widespread species. The team is to be heartily congratulated for such an accomplishment. As a reviewer, I found much to like about this document, although it was difficult for me in many places to give this work full justice. The size of the effort and quantity of information was a bit overwhelming for me (but nonetheless impressive). Consequently I focused on selected details that fell well within my ability to review and where I felt like I had expertise worthy of consideration by the team.

Specific comments -

Page 2. This comment will be repeated throughout the review… The core of this effort is focused on a scheme developed by Rieman et al. 1993. This was great stuff at the time and many elements have remained relevant. In the past 13 years however there has been a tremendous amount of work on viability assessments in general and on trout and salmon in particular. Some particularly noteworthy efforts include documents produced by NOAA-Fisheries (e.g., the "Viable Salmon Population" document), the National Center for Ecological Analysis and Synthesis (host of papers on risk assessments), IUCN, Natureserve, and other efforts of smaller dimension, but worthy of consideration. I won't provide a huge list of references here, but I will offer to send .pdfs that represent some of what I view as the most relevant documents if needed.

Page 4. Good stuff on data quality control. Did this apply to each source of information or estimates of different parameters?

Page 4. I would also add that relative health of populations is tied to their distribution representing the natural ecological conditions in which they evolved in. For a more complete list of criteria, see:

McElhany, P., Ruckelshaus, M.H., Ford, M.J., Wainwright, T.C., & Bjorkstedt, E.P. 2000. Viable salmon populations and the recovery of evolutionarily significant units. Seattle, Washington: U.S. Department of Commerce, National Oceanic and Atmospheric Administration, Technical Memorandum NMFS-NWFSC-42.

This document is quite comprehensive and I would not expect the team to consider every possible thing. However, there needs to be more justification for the criteria selected relative to recommendations like those put forth in McElhany et al. 2000.

Related to above, somewhere in the document, criteria relevant to population persistence *and* viability over the longer term might be clarified. See Fausch et al. 2006 for definition of persistence vs. viability:

Fausch, K., B. Rieman, J. Dunham, and M. Young. 2006. Strategies for conserving native salmonid populations at risk from nonnative invasions: tradeoffs in using barriers to upstream movement. USDA Forest Service, Rocky Mountain Research Station, GTR-RMRS-174, Fort Collins, CO.

In a nutshell, populations can persist without being viable. Viability refers to long-term evolutionary potential of a species and persistence is a necessary but not sufficient condition.

Page 5. Good on barriers to upstream movement. Always difficult to develop solid criteria for what makes a natural barrier. What about barriers to use of downstream habitats? I assume this is accounted for in the criteria for connectedness.

Page 5. When professional judgment was used to estimate distribution limits, did the professionals have any documentable criteria in mind? E.g., map rules related to stream size, channel slope, temperature, etc.? Worth documenting how the "educated guesses" were made. I think it's OK to rely heavily on expert opinion, professional judgment, etc., but also that it is important to try and document what criteria were in the individual's mind when making the calls.

Page 6. Source-sink terms are not used in accordance with how they are used in the scientific literature. A sink is a population that cannot sustain itself without demographic support from other populations. This is an important distinction from how the term is used in the CRCT assessment.

Page 6. Determination of connectedness was difficult to follow. In the protocol (page 78) is seems like there are more possibilities than the categories allow. For example, it may be possible there is only one occupied stream and a strong local population with a well-developed migratory component. This would rank as a "4" I guess using the table on page 78. A population without a migratory component would also rank "4."

My view is that "dispersal" connectivity (e.g., ability of fish to move among local populations or colonize unoccupied habitats) is distinctly different than "migratory" connectivity (e.g., access of fish to habitats that allow development of migratory individuals). I'll suggest these are important and distinct enough to be considered separately. Both measures of connectivity might also benefit from consideration of distances between local populations or distances from a local population to what is considered to be a migratory destination. Distance is part of criteria in Table 25, for example.

Maybe this is done partly from information on page 79. It was hard for me to tell if you were not counting the same things twice here in these tables (pages 78-79).

While I'm on page 79 – I suggest deleting considerations in Table 20 (population qualifiers). This seemed like it was a big can of worms. It is really that informative and reliable? How did you decide on these criteria and how is “known” ecological adaptation or even “probable” determined? Seems like a bunch of speculation.

Page 6. Good point about “inherent” probability of persistence. I’d call it “absolute” versus “relative” estimates of persistence. See:

Ralls, K., Beissinger, S. & Cochrane, J. 2002. Guidelines for using population viability analysis in endangered-species management. In: Beissinger, S., and McCullough, D., ed. Population viability analysis. Chicago, Illinois: University of Chicago Press, pp.521-550.

Genetic stability might be more accurately called genetic “integrity” – the focus here is on hybridization, but loss of variability due to small population size is a big factor. According to Table 29, over half of CRCT populations are vulnerable to loss of genetic variability due to drift (<500 adults) and most are less than 2000 adults. Given that the genetically effective size of these populations is likely much lower than the total number of adults, I would suspect a population of 2000 adults would still be vulnerable to drift. This is a natural condition in some populations, and the team would have to consider this in their evaluation – but its clear overall the range of this species has been severely fragmented. In contrast, while there are important risks of hybridization, this is not as widespread as threats from small population size and loss of variability within local populations.

Two main points here: 1) genetic drift appears to be equal or greater in terms of a threat to CRCT and 2) some additional thought is needed to consider the effective size (which is related to genetic drift) versus total number of adults (which is less strongly tied to drift).

For a good example of how small the effective size of local isolated populations can be, see:

Neville, H., J.B. Dunham and M.M. Peacock. 2006. Landscape attributes and life history variability shape genetic structure of trout populations in a stream network. Landscape Ecology 21:901-916.

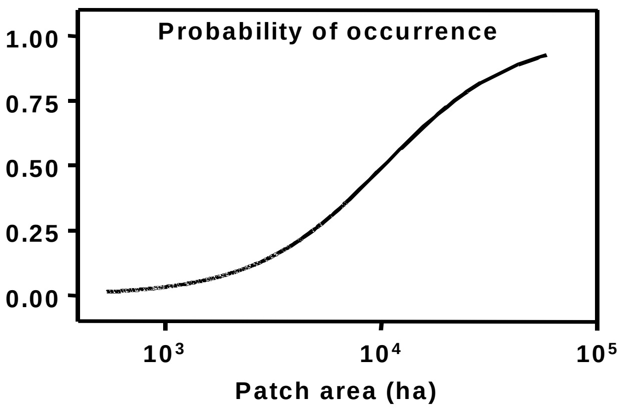
Nobody knows the true Ne/N ratio for inland cutthroat trout but the team might do well to consider some values from the literature. Some have assumed a 1/5 ratio (e.g., work by Hilderbrand and Kershner on “minimum stream lengths” – I can send a .pdf), but after the work on Lahontan cutthroat trout (see above) I suspect the ratio can be much smaller. So yes there is uncertainty here – and the ranks in Table 27 might be modified to consider this.

Page 7. See above. The parenthetical statement about “approximating and effective population” needs a lot more clarification. This is an important consideration.

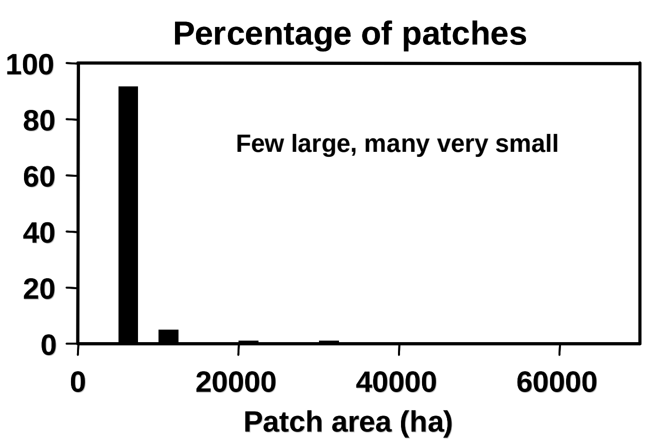
Page 9. What was the consensus process for excluding HUCs from historical habitat? If there is one person who disagrees, then is there not a consensus? I’m not used to consensus in my world. It’s a great goal if you can get there, but usually individuals have a range of views. Was consensus perhaps based on an overall weight of evidence from the group and not "trumped" by a single individual's call?

Page 12. When summarizing distributions for species like CRCT I think it is more accurate to report the distribution of sizes of local populations or occupied habitats. Overall statistics like total number of stream miles, total number of individuals, total percentage of historical habitat occupied, etc., can provide some information of limited utility, but there is very good evidence to show that local population persistence. So reporting this in preference to those broader, fuzzier statistics would be my preference. This I believe more accurately portrays the species' status.

Again an example from a subspecies I've worked on a lot (Lahontan cutthroat trout). Here's the predicted probability of occurrence in relation to patch size (suitable watershed area) – of course bigger is better:



Now here is the plot showing the size distribution of local habitats in that analysis:



It is pretty easy to see here that looking at each habitat individually can help to provide some perspective on habitat conditions. Like CRCT, Lahontan cutthroat trout occupy numerous habitats and if you add up percentages, etc., things might look better than this. However, this graph shows what the fish are telling us is their bottom line in terms of persistence. The "scale-area" issue is a big one now in conservation biology and I would be happy to send .pdfs of literature describing that, and papers on Lahontans that do the same. Also, I believe the work by Amy Harig and Kurt Fausch would be especially valuable here to make these points on a more local level (see also work by Mike Young and Paula Guenter-Gloss).

Related to above - I know the stated goal is to report "status" not analyze "patterns or causes" (page 15), but it is hard to report status without understanding what "status" is – you have to infer something about patterns or causes, and this is evident throughout this assessment.

Page 17. The trend in Figure 5 would best be displayed on a log scale. There is too much variability in the response to make any inferences on a linear scale. To deal with the variation you could apply regression quantiles if you had a hypothesis about some kind of maximum relationship. As-is, inferring a linear relationship based on the mean slope is a no-no.

Page 18. Determination of hybrid status is always troubling. I've always said the most important part of any genetic study is over by the time the samples make it to the lab. This means that sampling bias in the field can be the most important source of error – not genetic markers.

Now to step down from that soapbox, I think that something similar to Table 10 (see page 75) should be constructed to represent the quality of the field sample. Not just total number of fish sampled, but life stages (hybrids might be more common in juveniles, but not adults due to some kind of selection against hybrids) and spatial distribution (e.g., near a bridge or road, or random sample across the population?) at the very least. If this information was not documented, there should be an "uncertainty" score as well.

While I'm at it here I noted on page 74 (Table 8) that it would be informative to also record the date of last stocking for potentially hybridizing species. I suspect fish stocked a long time ago may be less likely to show an influence in terms of hybridization. However, if you follow Frank Rahel's analysis of the increasing trend in unauthorized stocking, this could become a moot point. Maybe there is a way to think about that as a distinct threat?

Page 44. Composite population health scores based on a weighted formula. Where is that formula? It may be in the report, but I could not find it. How were weights assigned and uncertainty dealt with? I didn't like the analogy with physician screenings. How many people really trust their doctor? Seriously, there is tremendous uncertainty in any diagnosis. A good example is how IUCN risk criteria have been modified to explicitly incorporate uncertainty into conservation ranks – or use of Bayesian Belief Networks (I can send .pdfs of examples).

Good here to emphasize the big problem of "smallness" for CRCT on a local level on page 45.

Page 50. Useful to index conservation actions, but I suspect there has been a lot of debate about their effectiveness as highlighted on the page – a difficult issue. I was similarly not able to adequately review work on Restoration and Expansion. I think it is a good idea to present a forward-looking view of status, however.

Page 55. Along the lines of comments above – consideration of future stocking of nonnative trout might be interesting here.

Page 56. Obvious comment – good summary of quality, but quantity might be way more important (see above graphs for Lahontans and my comments).

Page 58. Are the benefits of nonnative trout removal exactly the inverse of the complexity of removal? Maybe consider both?

Concluding thoughts –

Again I want to congratulate the team on this huge accomplishment. I have tried to suggest a few selected ways to improve this incredible product. As I wrap this up, I continue to be impressed by the volume of information and detail in this status assessment. I found it hard to follow in some places, due to so many pieces and my difficulty in trying to keep track of how they were all related.

This is a great foundation of information to have available – and no doubt good preparation for future "data calls." Another use of this might be to motivate a more systematic monitoring and evaluation program for CRCT that would fill in key information gaps for status assessments.

I found a lot more to like than I found to criticize here and I hope the comments and criticisms offered here are truly constructive. I would be happy to confer with individual team members or the team as a whole if they have questions or need clarification.

Jason Dunham

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