From:	Cathy Marcinkevage - NOAA Federal <cathy.marcinkevage@noaa.gov>
Sent:	Monday, June 10, 2019 9:54 AM
To:	Maria Rea; Howard Brown; Garwin Yip; Barbara Byrne
Subject:	Fwd: FW: My few thoughts

Some specifics from Dave Hankin.

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From: **Michelle Havey** <mhavey@anchorqea.com>
Date: Mon, Jun 10, 2019 at 8:50 AM
Subject: FW: My few thoughts
To: Cathy Marcinkevage - NOAA Federal <cathy.marcinkevage@noaa.gov>
Cc: John Ferguson <jferguson@anchorqea.com>

Cathy,

Here are the thoughts Dave passed along last night:

Michelle Havey
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ANCHOR QEA, LLC

From: David G Hankin <david.hankin@humboldt.edu>
Sent: Sunday, June 9, 2019 5:07 PM
To: Michelle Havey <mhavey@anchorqea.com>
Cc: Rose, Kenny <krose@umces.edu>; John R Skalski <skalski@uw.edu>
Subject: Re: My few thoughts

I would not consider my comments below to be a "review" in any sense of that word, but please do relay these few possibly useful thoughts to the NMFS folks who are drafting the BiOp.

1. Analytical Approach.

I was generally OK with this section, but objected to:

(a) the exceptionally loose usage of the term "fitness", and the associated (b) implicit assumption that increased growth rate is directly associated with fitness (especially for salmon and steelhead). I found this loose usage specially surprising given the exceptionally strong genetics group at the NMFS Santa Cruz Ecology Lab (with whom I have worked for many years). First, the "bottom line" of fitness for an individual is the number of progeny that survive to maturity. In an age-structured population, one might argue that the associated performance attributes of those surviving progeny (age,size, and fecundity at reproduction) are also important components of "fitness". A huge number of factors ultimately determine fitness, but it can only be measured, in a relative sense, by comparing mature progeny production across reproducing adults. Although improved growth rate may lead to improved fitness in many teleost fishes, the relationships between size and timing of outmigration, the relationships between survival and time at outmigration (not just size), and the subsequent complexities of the effects of variation in size and timing of outmigration on subsequent age and size at maturation (in salmon) are exceptionally complex and do not lend themselves to a simplistic assertion that more rapid growth rate leads to greater fitness. Suppose, for example, that outmigration is triggered when juveniles reach a threshold minimum size. If water and flow management were to cause that size to be achieved, say, one month earlier (or later) than "normal", then smolts may enter the ocean at less than most favorable periods for survival, thereby cancelling the advantage of size that is normally observed *for fish that have the same outmigration timing*. For steelhead, the implicit assumption that more rapid growth leads to increased fitness is further complicated by freshwater residualization which appears to happen under situations where the juvenile freshwater growth rate is substantially enhanced (e.g., upper Klamath River). Thus, growth rate may affect expression of life history in steelhead.

It's fine to note, in the BiOp when water temperature or flows might lead to increased or reduced growth rates, but it is tough to know what to make of them and hard to consistently rate them as "+" or "-", I think.

2. Use of CalSimII modeling to infer "risk associated with stranding"

From the BiOp (Section 2.5.2, at pp 26-27 (quote below) and similar reasoning repeated elsewhere):

"Flows during the juvenile rearing period (July-December) average about 9,000 cfs downstream of Keswick Dam, which poses a stranding risk to juveniles when flows are reduced. The greatest risk posed by these operations would occur when December flows are reduced to 3,250 cfs. The risk associated with these operations is reflected in the proportion of years that Keswick flows in December would be no greater than 3,250 cfs. Assuming the initial operations reflected in Table 2.5.2-4, CalSimII modeling indicates that end of September storage is less than or equal to 2.2 MAF in about 20 percent of years (ROC on LTO BA Appendix D Table 3-2), and there is therefore a 20 percent probability in any year that December flows would be reduced to 3,250 cfs."

As juvenile fish are capable of moving when flows drop, one expects stranding of juveniles to take place only when flows drop so rapidly that fish cannot adequately respond quickly enough to the change in flow. I do not profess to have any idea of how rapidly flow would need to drop to strand juveniles, though I would suspect that it is in part related to the complexity of the cross-channel depth/velocity structure of a stream and no doubt an exceptionally complex issue.

I was, however, quite bother by the apparent inference in the above paragraph that stranding risk is somehow related to a reduction in flows from an average of 9,000 cfs to 3,250 cfs during December based on EOS storage in Shasta reservoir. It seems to me that the "risk of stranding" in these 20 percent of years should be related to the difference between the expected flow (prior to flow reduction) in those years that trigger the flow reduction and not the difference between the average flow of 9,000 cfs and the reduction to 3,250 cfs. John could explain this idea better than I can I am sure, but in statistical jargon, the risk of stranding should be more closely related to the conditional expectation of stream flow prior to those years in which a reduction in flow to 3,250 would take place and not to the unconditional expectation of stream flow prior to reduction (9,000 cfs). I would wager, without data, that the conditional expectation of flow prior to reduction is considerably less than 9,000 cfs as this would normally take place during years where rainfall and deliveries have not been adequate to maintain desired EOS storage.

3. In Section 2.5.2, it would be nice to know (a) the average escapement of spring-run Chinook spawning in the upper river, in the same habitat as winter-run Chinook, as well as (b) importance of this "population" to the overall spring Chinook ESU. That's probably in "some document somewhere" (that I am sure I was supposed to have read), but it would be handy to include also in Section 2.5.2 subsections dealing with spring-run Chinook. Although management of the upper river is very clearly driven by winter-run fish (because they have nowhere else to go unless Battle Creek eventually "comes to the rescue"), it seems to me that the spring-run fish might be much more adversely affected by upper river flow management. If this population is relatively unimportant, then this is no big deal, but I do not know.

4. Small matters, but highly objectionable to one unsuccessful reviewer.

A. In a document as filled with as many acronyms as this one (I've never seen anything remotely like it), it seems important that at least tables and figures always include definitions of acronyms. That was not the case in many instances and I had to just "guess" what table entries might be. That is, I think, quite crazy and should certainly be cleaned up for the final BiOp. It's a pain, but very easy to do, and it is obvious for the BiOp that length is not a concern.

B. I found myself referring to Wikipedia to see where "moreso" was coming from. My impression is that it is not yet "accepted usage" and it would be nice if different words might be used to convey a less vague notion of effect.

A daunting task to prepare the BiOp and a daunting task to review. My apologies for admitting defeat.

Dave