Page	Chapter	Line Number	Comment	Reviewer
0	0	0	Please see cover letter to U.S. Fish and Wildlife Service dated January 14, 2022, for explanation of the comment codes provided	1
0	Table of Contents	2	Page numbers and Table of Contents needs to be updated.	1
0	1	30-47	W1, W2, W3, W9- The science presented at U.S. Fish and Wildlife Service's Longfin Workshop indicated significant uncertainty around whether the longfin smelt qualified as a DPS. The report should reflect that uncertainty and should summarize the new genetic data and ocean current data. The acknowledgment of uncertainty in the discussion of the referenced data would be helpful, as the draft report currently cites no papers or other documents to substantiate the DPS.	1
1-2	1	-	W1, W9- This section should discuss how data quality and uncertainty is considered within the context of resiliency, representation, and redundancy. Similarly, the section should explain how gaps in scientific understandings are addressed.	1
3	1	129	W1, W9- The introduction should discuss how uncertainty and gaps in scientific understandings are addressed.	1
3	2	132-34	W1, W3, W5, W6- The report should explain the assumption that information from other longfin smelt populations such as that inhabiting Lake Washington is directly applicable to the Bay-Delta population. The draft report (lines 30-47) discusses the finding that the Bay-Delta population is distinct from more northerly populations and that these populations inhabit very different environments. The report should explain the value and uncertainty in extrapolating information from an area where longfin reside to another area where longfin reside.	1
3	2	136	W1, W9- This section needs to identify uncertainty and knowledge gaps and describe how they are addressed.	1
4	2	167	W1, W9- This section needs to identify uncertainty and knowledge gaps and describe how they are addressed.	1
5	2	184-208	W1, W3, W5, W6- The discussion on lines 30-47 should be linked together with any additional new information and analyses, to provide a better discussion of the best available data and most current analyses used in support the finding that the Bay-Delta population qualifies as a DPS.	1
5	2	215-21	W2, W6, W10- The San Francisco Estuary Institute and others have developed detailed maps and documentation on changes to wetlands within the Bay-Delta that are more detailed and current and should thus be included in the discussion rather than relying on only The Bay Institute 1998 as the basis for the assessment. In addition, the discussion of wetland habitat should be expanded to discuss the current and planned efforts to restore wetland habitat and its functions that affect longfin smelt and other species including enhanced zooplankton prey abundance. Occupancy by longfin smelt and enhanced zooplankton production in existing wetlands should be discussed, including as a result of the south Bay salt pond restoration program. Of potential importance is Lewis et al. (2019), which reported that all life stages of longfin smelt were found at relatively high densities in tidally-restored salt-pond and adjacent habitats in Alviso Marsh and the Napa River.	1
6	2	229	W1, W9- This section needs to identify uncertainty and knowledge gaps and describe how they are addressed.	1
7	2	304-05	W5, W8, W9- In addition to the SWP and CVP, the report should acknowledge and discuss all of the other water diversions that affect inflow to the Delta, in-Delta flows, and Delta outflow, which necessarily include upstream tributaries and the Bays.	1
8	2	311-17	W3, W6- The discussion of spawning distribution should be expanded to include other bay tributaries (e.g., Napa River, Petaluma River, Coyote Creek, etc.) where there is evidence of spawning activity (e.g., collection of gravid adults, post-spawned adults, young larvae). This discussion should also be expanded to include results of surveys as part of the South Bay Salt Pond restoration effort.	1
8	2	336	W1, W3, W5, W6- The report should discuss (1) observations of longfin smelt spawning in captivity and in the Lake Washington/Cedar River studies, and (2) potential uncertainties in the application to the Bay-Delta population (see comment lines 132-134). As noted above, it would be helpful if the report included an expanded discussion of the uncertainty associated with extrapolating life history information from other populations and smelt species to the Bay-Delta. [Comments on lines 132-134 replicated for ease of review: The report should explain the assumption that information from other longfin smelt populations such as that inhabiting Lake Washington is directly applicable to the Bay-Delta population. The draft report (lines 30-47) discusses the finding that the Bay-Delta population is distinct from more northerly populations and that these populations inhabit very different environments. The report should explain the value and uncertainty in extrapolating information from an area where longfin reside to another area where longfin reside.]	1
10	2	387-97	W1, W5, W8- Discussion of diel movement patterns should also be discussed later in the report while discussing longfin smelt detectability in long-term monitoring surveys.	1
11	2	414	W1, W2, W3, W6, W8- The draft report speculates on a relationship between Delta outflow and growth, providing no data or graphics depicting confidence and variability in the relationship or a comparison of observed and predicted growth. The discussion should be expanded to describe the data and methods used to estimate longfin smelt growth rates from the field surveys, uncertainty and knowledge gaps associated with the relationship and points of speculation.	1
11	2	414-24	W1, W2, W3, W4, W5, W6, W7, W8- The discussion is vague and no data on these relationships is presented. The draft report says growth rates are reduced during periods when outflow is high (it is not clear if periods are years or seasons within a year). Growth was reported to be highest when temperatures were warm and conductivity was low. Since no data are presented and there is no indication of the strength of these relationships (or uncertainty) this discussion is difficult to assess. These appear to be model results rather than empirical relationships (an expanded description of the data, assumptions, and methods used in the analysis would be helpful in assessing these results). Presentation of the results in graphic format would help clarify the strength and level of uncertainty in these results.	1
11	2	425	W1, W2, W3, W5, W6, W8- The draft report presents no data or results showing a relationship between age-0 growth and outflow or the levels of uncertainty and variability between the environmental covariates and length-at-age. For example, the report could use the findings of Lewis et al. (2019), which show that otoliths exhibit daily increments that can be used to estimate the age, growth rate, and hatch date of individual fish, and to produce temporally-resolved chronologies of otolith chemistry. This type of data could be used to examine the relationship between growth in different areas of the San Francisco estuary and with different environmental conditions.	1
11	2	434-47	W1, W2, W5, W8- The draft report fails to adequately explain findings that appear to conflict with modeling results. The report should include a robust description of the models and should address the uncertainty associated with all findings rendered based on model output. There should be discussion of the finding that growth is negatively correlated with outflow. See also response to lines 414-424 [Replicated here for ease of review: The discussion is vague and no data on these relationships is presented. The draft report says growth rates are reduced during periods when outflow is high (it is not clear if periods are years or seasons within a year). Growth was reported to be highest when temperatures were warm and conductivity was low. Since no data are presented and there is no indication of the strength of these relationships (or uncertainty) this discussion is difficult to assess. These appear to be model results rather than empirical relationships (an expanded description of the data, assumptions, and methods used in the analysis would be helpful in assessing these results). Presentation of the results in graphic format would help clarify the strength and level of uncertainty in these results.]	1
11	2	437	W1, W2, W3, W8- The draft report discusses density dependent mortality but provides no discussion of the mechanisms that would result in density dependent relationships when population abundance is low. The report should provide support by adding graphs of trends in CPUE, which is not discussed here but is discussed in the section summary. It would be helpful and strengthen the report to include an expanded discussion of the information to support or refute the role of density dependent mortality on the status and trends in the longfin smelt population.	1
11	2	438	W1, W3, W7- The report should discuss results of using otoliths to support analyses of longfin smelt age and growth as well as locations of successful rearing habitat. Recent work has provided empirical evidence of growth rates using otolith analysis (i.e. Lewis et al. 2019). These growth rates as well as empirical evidence of successful rearing habitats should be added to the report.	1
12	2	470	W1, W2, W3, W5, W6, W8- No data or graphs are presented to support the discussion of a relationship between the geographic distribution of yolk-sac larvae and X2 location and therefore there is no depiction of variability and uncertainty in the proposed relationship. Data and graphs should be included, or the absence of such information should be explained.	1
12	2	479	W1, W3, W5, W8- The draft report finds that the presence of yolk-sac larvae downstream of Suisun bay is influenced by spawner distribution, however, results from Lewis et al. 2019 found that "we cannot reject the possibility that a significant fraction of Longfin Smelt found recruiting to the northern and southern SFE in 2017 and 2019 originated from upstream habitats and were dispersed by high outflows." The report should explain what appears to be a distinction, reported by Lewis et al. 2019 between local spawning and dispersal downstream.	1
14	2	Figure 2.4	W1, W2- The draft report references the low vs. high abundance years. The report should define those terms. Also, an expansion of the discussion or the data, assumptions, and analytical methods used should be included in the report.	1
20	2	608	W1, W9- This section needs to identify uncertainty and knowledge gaps and describe how they are addressed.	1
20	2	608	W2, W8, W9- The report should discuss the observations of high mysid production in the south Bay restoration areas as well as the overall trends over time in mysid densities and other key zooplankton (longfin smelt prey) in those areas of the estuary supporting larval, juvenile, and adult longfin.	1
20	2	629	W1, W2, W6, W7- Presentation of maps showing the sampling locations for each of the surveys should be included as well as an expanded discussion of the spatial and seasonal coverage of each of the surveys with the lifestages of longfin smelt. Information on the size-specific capture efficiency of each of the sampling gear should also be presented. Since the distribution of sampling stations in the FMWT are fixed, the indices of relative abundance from one survey and year to another based on variance in the geographic distribution of longfin smelt in response to differences in the salinity gradient and other factors should be discussed as a source of uncertainty.	1
21	2	654	W1, W6, W9- The discussion of the surveys should not be limited to contact with longfin smelt. The report should discuss the effectiveness of each sampling gear in retaining various sizes of smelt (size-specific gear collection efficiency).	1
21	2	665	W1, W6, W9- The discussion of the FMWT (and all other sampling methods discussed in the report) should include sampling bias based on size-specific net retention, vertical distribution in the water column of longfin smelt within the pelagic zone sampled by the midwater trawl, and the geographic distribution of longfin smelt downstream of the region where sampling occurs and how those factors contribute to underestimates of longfin smelt abundance, distribution, survival, etc.	1
21	2	680	W1, W2, W7, W9- The draft report notes that the existing fishery surveys are limited to providing only relative indices of abundance but should also address why there are no quantitative population abundance estimates (with confidence intervals) for the various longfin smelt lifestages each year. Despite the declining trend over time the interpretation of current conditions for the species would be different if the population of spawning adults was 100,000 vs. 1,000,000 or more. The report should discuss efforts to develop quantitative population estimates in the future. How does the lack of sampling in near-shore coastal waters impact uncertainty in developing population abundance estimates and development of a lifecycle model for longfin smelt?	1

Page	Chapter	Line Number	Comment	Reviewer
22	2	699	W1, W2, W7, W9- The current monitoring surveys are limited to reporting relative indices of abundance while examples of other fishery-related species status assessments have provided detailed quantitative estimates of population abundance. The report should acknowledge the data gap and any uncertainties associated with it. See comment regarding line 680 above. [Replicated here for ease of review: The draft report notes that the existing fishery surveys are limited to providing only relative indices of abundance but should also address why there are no quantitative population abundance estimates (with confidence intervals) for the various longfin smelt lifestages each year. Despite the declining trend over time the interpretation of current conditions for the species would be different if the population of spawning adults was 100,000 vs. 1,000,000 or more. The report should discuss efforts to develop quantitative population estimates in the future. How does the lack of sampling in near-shore coastal waters impact uncertainty in developing population abundance estimates and development of a lifecycle model for longfin smelt?]	1
22	2	699-708	W1, W2, W7, W9- The discussion of the adequacy of various existing field survey programs should be expanded to include uncertainty in quantitatively assessing the population status when the surveys do not sample the temporal or geographic distribution of all longfin smelt life stages. Results of a comparative analysis of survey results (abundance indices or actual population estimates) between the various surveys (e.g., Bay Study otter trawl compared to Bay Study midwater trawl, Bay Study compared to FMWT, etc.) should be included to show the level of variation among surveys. The report should discuss, for instance, that the Bay Study using MWT and Otter trawl cover the greatest geographic and seasonal distribution but miss the 20mm and SLS size distribution. The level of uncertainty introduced by the lack of routine surveys in near-shore coastal waters on indices and estimates of abundance should also be discussed as should mid-channel surveys vs. shoals and near-shore sampling. The draft report notes that the existing fishery surveys are limited to providing only relative indices of abundance, but the report should also address why there are no quantitative population abundance estimates (with confidence intervals) for the various longfin life stages each year.	1
22	2	710	W1, W9- This section needs to identify uncertainty and knowledge gaps and describe how they are addressed.	1
25	2	820	W1, W4, W6, W7, W8- An expanded discussion and presentation of information on trends in key zooplankton prey densities and species composition should be included to support the later discussion of step changes in the population and the declining trends in longfin smelt abundance related to prey limitations and effects of non-native filter feeders.	1
25	2	836	W1, W9- This section needs to identify uncertainty and knowledge gaps and describe how they are addressed.	1
25	2	845-50	W1, W2, W4, W5, W6, W7, W8, W9- The draft report discusses the consistency in the slope of the relationship between winter-spring outflow and indices of abundance in the FMWT. Absent from the draft report is a discussion of data, and their impact on estimated population abundance, derived from the Bay Study surveys and Delta outflow. Although the slope of the apparent relationship has remained relatively constant, the report should discuss that the intercept has changed substantially over time. The report should also consider the potential underlying mechanisms that may cause the relationships, as well as the implications for the step declines that have been observed in the relationships. This discussion would benefit from graphics showing the relationships from each of the surveys across each lifestage rather than the FMWT alone. The discussion of the flow-abundance relationships should also be expanded to discuss the apparent lack of a population response in recent wet years when Delta outflow has been high.	1
26	2	874-75	W1, W2, W4, W5, W6, W7, W8, W9- Results of recent wet year surveys indicate that longfin smelt abundance has continued to decline under previously favorable outflow conditions. The report should acknowledge that and explore the factors and mechanisms that account for this recent change in the flow-abundance relationship. The draft report discussion should be expanded to include environmental factors that are correlated with Delta outflow (e.g., floodplain inundation, downstream zooplankton transport, larval smelt dispersal, low salinity habitat area and quality, etc.) and the potential underlying mechanisms for potential relationships among these and other factors and longfin smelt production, growth, and survival. For example, it has been hypothesized that the introduction and rapid increase in clam abundance and biomass of the Asian clam, a benthic filter feeder, has altered the food chain (the clams remove zooplankton that longfin smelt prey on from the water column) and that may affect longfin smelt production, growth, and survival. Furthermore, this passage is referencing the post-2002 abundance decline, which was explored by Thompson et al. (2010). They found that "An unexplained (by the covariates considered) step decline in 2002 for all species … [is] consistent with a hypothesis that simultaneous, abrupt declines in abundances of multiple species are more likely to have been caused by a common but unknown factor than by different factors for each species." This passage should acknowledge Tompson et al. (2010) and discuss its significance to longfin smelt.	1
26	2	876	W1, W2, W4, W5, W6, W7, W8, W9- Although the slopes of regression relationships between Delta outflow and longfin smelt indices of abundance have remained similar, the report should discuss the changes to the intercepts and the hypothesized mechanisms underlying the step changes that have been observed. A graphic presentation of the data (both FMWT and Bay Study) would add to the discussion. See comment above regarding potential factors and mechanisms affecting longfin smelt abundance. [Excerpt of above comment replicated here for ease of review- The report should acknowledge that and explore the factors and mechanisms that account for this recent change in the flow-abundance relationship. The report discussion should be expanded to include environmental factors that are correlated with Delta outflow (e.g., floodplain inundation, downstream zooplankton transport, larval smelt dispersal, low salinity habitat area and quality, etc.) and the potential underlying mechanisms for potential relationships among these and other factors and longfin smelt production, growth, and survival.]	1
27	2	898-90	W1, W3, W5, W8, W9- Speculative: "During the winter and spring, larval and juvenile longfin smelt could be vulnerable to entrainment by water export operations." The draft report does not provide support or justification for this conclusion or any analysis regarding how SWRCB's Decision 1641 controls are working. This information needs to be included in the report, or the absence of such information should be identified, and associated uncertainties should be addressed. The discussion should also include description of management of seasonal exports to reduce the risk of entrainment and acknowledge uncertainty in the performance and effects of a number of management actions for longfin smelt that have not been evaluated to date. Species status assessment guidance and framework require FWS to specify level of certainty in the analyses and data supporting findings. For instance, in 2020 the longfin salvage was about 2,680; findings like this should be addressed. An expanded discussion and assessment of the effects of regulatory constraints that currently exist and the level of certainty (or how this will be addressed in the future) would help strengthen the report.	1
27	2	905-09	W1, W5, W8, W9- The report should include a discussion of other factors that affect flow or hydrodynamics in the Delta, such as tidal actions, geomorphology, and in-Delta diversions. The report should also discuss the difference of effects from net flow versus instantaneous flow on fish.	1
27	2	909	W1, W2, W5, W6, W8- The report should discuss that there are no estimates for entrainment losses of larval smelt less than 20 mm and therefore the magnitude and importance of these losses to the population dynamics of the species are unknown. The discussion should also acknowledge the high level of uncertainty in assessing the effects of entrainment and salvage losses on the overall population dynamics and status and trends for longfin smelt as well as the absence of annual estimates of proportional losses associated with SWP and CVP export operations. No results from a lifecycle model of longfin smelt are presented to help put entrainment losses into a population-level context.	1
27	2	918-20	W1, W2, W3, W5, W6, W8- "Successful transition of longfin smelt from larval to juvenile stage … may be limited by food resources and could depend on increased food supply resulting from higher outflow." The draft report does not include data or analyses to support this assertion. This information needs to be included in the report, or the absence of such information should be identified, and associated uncertainties should be discussed.	1
27	2	926-30	W1, W2, W3, W5, W6, W8- "Longfin smelt prey abundance … may also rely on increased Delta outflow." The draft report does not include data or analyses to support this assertion. This information needs to be included in the report, or the absence of such information should be identified, and associated uncertainties should be addressed. Further, these conclusions do not identify what levels of outflow are adequate and do not state whether typical years are adequate for survival under current conditions. If available, data on the stomach fullness and condition factors of longfin smelt collected over a range of outflow conditions should be presented as support. In unavailable, description and explanation of uncertainty and unknowns need to be included.	1
27	2	929	W8- The phrase "continuous wet years" is confusing and should be revised.	1
28	2	941	W1, W4, W6, W7, W8- See earlier comment on report pages 25-26 regarding presentation of a discussion of trends in zooplankton densities that are important to longfin prey over time and space within the Bay-Delta. [Excerpt of referenced comment replicated here for ease of review- The draft report discusses the consistency in the slope of the relationship between winter-spring outflow and indices of abundance in the FMWT. Absent from the report is a discussion of data, and their impact on estimated population abundance, derived from the Bay Study surveys and Delta outflow.]	1
28	2	945-52	W1, W4, W6, W7, W8- See earlier comment on report pages 25-26 regarding presentation of a discussion of trends that are important to longfin time and space within the Bay-Delta. These comments apply to water quality as well and need to be addressed. [Excerpt of referenced comment replicated here for ease of review- The draft report discusses the consistency in the slope of the relationship between winter-spring outflow and indices of abundance in the FMWT. Absent from the draft report is a discussion of data, and their impact on estimated population abundance, derived from the Bay Study surveys and Delta outflow.]	1
28	2	964	W1, W4, W6, W8- There is a discussion of higher outflow yielding more habitat for longfin smelt, although on line 886 there is a discussion of high outflow compressing the salinity gradient. An explanation of how these two conditions affect each other is needed.	1
28	2	966-74	W1, W4, W6, W8- There is a discussion of higher outflow yielding more habitat for longfin smelt, although on line 886 there is a discussion of high outflow compressing the salinity gradient. An explanation of how these two conditions affect each other and water quality is needed.	1
28	2	977	W1, W5, W8- "Increased delta outflow often associated with increased cloud cover and precipitation." This is not always the case especially during periods of high snowmelt in the spring. In addition, water temperatures in the delta are largely driven by ambient air temperatures so the timing of the flows matters more for water temperatures than the amount of outflow. These contradictions and uncertainties need to be addressed in the report.	1
29	2	992	W1, W6- The text implies that longfin smelt only spawn in the south Bay and other Bay tributaries in wet years. Do longfin smelt not spawn in the south Bay in drier years? This needs to be addressed.	1
29	2	1003-04	W1, W3, W5- Provide citation for the following statement: "During very dry years, evidence of successful spawning is typically limited to the delta and Suisun bay/marsh." Lewis et al. (2020) found evidence of successful spawning in the South Bay during dry years although recruitment was limited. Clarify this distinction in the report.	1
29	2	1007-14	W1- W8, W9- The report should briefly discuss the inter-annual hydrologic variability in freshwater flows within the Bay-Delta and the feasibility of providing the recommended level of Delta outflow every year. Potential impacts of high Delta outflows each year on other species like cold water management for winter-run Chinook salmon spawning and egg incubation should also be acknowledged.	1
30	2	1015	W1, W2, W8- Figure 2.8 does not provide adequate context for the correlation between outflow and mechanisms that may affect recruitment. Even the figure caption claims: "if freshwater flows to the estuary remain sufficient [sufficient is undefined for each mechanism] then additional benefits may accrue to larvae and thereafter young juvenile fishes." Additional description should be included in the report.	1

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31	2	1034-37	W1, W2, W3, W4, W5, W6, W7, W8, W9- "The ecological mechanisms that have generated the correlation between freshwater flow and abundance have not received as much research attention as the phenomenon itself and as such may not be fully described and their relative importance has not been quantified." The report should provide additional explanation on lack of information and understanding of the potential mechanisms underlying the flow-abundance relationships and how the relationships may have changed over time. For instance: How does this data gap impact future management decisions and the level of uncertainty associated with a management action for longfin smelt? How confident is FWS in predicting the response of longfin smelt to a management action when there is a high degree of uncertainty in the underlying mechanisms and relationships?	1
31	3	1045	W1, W9- Each section in this chapter needs to identify uncertainty and describe how uncertainty and knowledge gaps are addressed.	1
31	3	1045	W1, W8, W9- In general, the draft report presents information on the same topic in several different sections. The entire discussion on a topic should be consolidated into one section. If that change is not made, inconsistencies and factors that impact the certainty or reliability of the topic need to be adequately addressed in the discussion.	1
31	3	1062	W5, W8, W9- The draft report focuses on SWP and CVP exports and entrainment with little or no discussion regarding the large number of other diversions upstream, in the Delta, and downstream bays. Additional discussion should be included to address those diversions and associated effects. For example, recent research (Hutton et al. 2021) showed that channel depletions consumed as much of 17% of total delta water use during a drought year (2015).	1
33	3	1117-20	W6, W8, W9- The report should include additional discussion of the stressors limiting and effecting longfin smelt and the linkage to evaluating the effectiveness of existing management actions in addressing these stressors and address questions such as the efficacy of existing regulations. The discussion should also identify data gaps; for instance, the ITP was issued in 2020 and there is scant discussion of data regarding the effectiveness of its implementation.	1
34	3	1142-44	W1, W6, W7, W8- "[I]t is likely that the species has been in decline … and will likely continue to experience record low abundances particularly during droughts." The report needs to provide additional discussion regarding the analyses, uncertainty, and bases for findings. Further, the report needs to discuss uncertainty with findings and trends, for instance the surveys resulted in long term decline, although the Bay Study Otter Trawl decline is an order of magnitude less than the Fall Midwater Trawl.	1
34	3	1159	W1, W5, W8, W9- The discussion of density-dependence should be expanded and include some explorations of the potential mechanisms acting on the population as this is a key unknown affecting the species' population dynamics. For example, Nobriga and Rosenfield (2016) suggested that density-dependence was occurring in the population but were unable to identify the mechanism. Interestingly, they "found no indication that freshwater flow moderated the survival of Longfin Smelt between age 0 and age 2, but we did detect evidence that survival during this lifestage transition is density dependent." Additional explanation is needed to address questions such as: at the current low levels of apparent abundance what data show that survival of longfin smelt is density dependent? What is the limiting factor leading to density dependent mortality? Discuss the underlying mechanisms that would result in density dependent population dynamics and response. See comment above regarding density dependent mortality. [Replicated here for ease of review: The draft report discusses density dependent mortality but provides no discussion of the mechanisms that would result in density dependent relationships when population abundance is low. The report should provide support by adding graphs of trends in CPUE, which is not discussed here but is discussed in the section summary. It would be helpful and strengthen the report to include an expanded discussion of the information to support or refute the role of density dependent mortality on the status and trends in the longfin smelt population.]	1
35	3	1175-79	W1, W2, W3, W5- The draft report provides no data or analysis to support the claim that the species may not have the resiliency to withstand drier conditions more than 50% of the time. This is ostensibly based on the continuously low abundance indices in recent years, which have been shown to be influenced by a number of factors (e.g. distribution) and do not provide a meaningful representation of the true population abundance.	1
36	3	1191-96	W1, W2, W3, W5- The draft report provides no data or analysis to support the claim that the species may not have the resiliency to withstand drier conditions more than 50% of the time. This is ostensibly based on the continuously low abundance indices in recent years, which have been shown to be influenced by a number of factors (e.g. distribution) and do not provide a meaningful representation of the true population abundance.	1
36	3	1201	W3, W4, W8, W9- The discussion of stressors needs to be more comprehensive, including potential direct and indirect effects of the large number of water diversions located throughout the Sacramento and San Joaquin watersheds.	1
36	3	1203	W3, W5, W8, W9- The report should be explicit that the effect of reverse flow on longfin smelt entrainment risk is limited seasonally to only the periods of the year when longfin smelt occur in the south Delta.	1
37	3	1218	W1, W2, W3, W4, W5, W6, W7, W8, W9- The draft report provides no data or analysis to support findings that Delta outflow has declined over the past two decades or that a decline in outflow has directly resulted in the decline of longfin smelt. Given all the factors that have been associated with changes in conditions in the Delta (e.g., the Asian clam expansion, numerous introductions of non-native zooplankton, macroinvertebrates, and fish, etc.), the report should discuss the high level of uncertainty that a decline in Delta outflow has caused the decline in longfin smelt. The report should also address questions like whether Delta outflow during February-June declined in recent years (e.g., over the past decade, etc.). This data and analyses need to be included, or, if unavailable, absence of this data and analyses needs to be addressed.	1
37	3	1222-28	W1, W3, W5, W8- Provide citation for the following statement. "out of all the species in the SFE, LFS exhibit the greatest correlation with freshwater flow, suggesting that freshwater flow is not only a driving factor of species abundance, but the most important species need." There is uncertainty associated with FMWT index of abundance (e.g. distribution shifts in dry years which would affect catchability) and the mechanism(s) behind the flow-abundance relationship have not been identified. As such, it is hard to draw the conclusion that reduced freshwater flows are the primary stressor for the species. This is also evidenced by the step-declines in the flow-abundance relationship following the Asian clam introduction and the POD.	1
37	3	1238-45	W1, W2, W3, W5, W8- The draft report discussion of a possible oil spill wiping out an entire spawning class is used to indicate low resiliency of the species (like other estuary species that would be affected by a catastrophic oil spill). Given the broad geographic distribution and multiple age class structure of the population the report should discuss the high level of uncertainty in this example. The draft report emphasizes the flow-abundance relationships and should acknowledge and discuss the fact that exposure to a catastrophic event like a major oil spill is independent and not at all related to the outflow relationship. This data and analyses need to be included, or, if unavailable, absence of this data and analyses needs to be addressed.	1
38	3	1288-97	W1, W2, W3, W4, W5, W8, W9- The discussion of colonization of the Bay-Delta by the non-native clams and the impact of this benthic filter feeder on zooplankton abundance that are the prey resource for longfin smelt should also acknowledge the introduction of the clam and their impact on zooplankton abundance are independent of Delta outflow. There is no evidence that Delta outflow management would be an effective method of controlling or reducing the impact of filter feeders like the clams on longfin smelt. The impact of the clams on zooplankton abundance may, however, be a major contributor to the observed step change in the intercept of the flow-abundance relationships as the estuary shifts from a pelagic ecosystem to a benthic ecosystem. The report needs to include additional discussion of these and other factors.	1
39	3	1304-25	W1, W2, W3, W4, W5, W8, W9- There is no evidence that Delta outflow management would be an effective method of controlling or reducing the impact of filter feeders like the clams on longfin smelt. The impact of the clams on zooplankton abundance may, however, be a major contributor to the observed step change in the intercept of the flow-abundance relationships as the estuary shifts from a pelagic ecosystem to a benthic ecosystem. The report needs to include additional discussion of these and other factors.	1
40	3	1329	W1, W2- The discussion of the step decline in the flow-abundance relationship should include a figure showing the data and correlations earlier in the text that could then be referenced as part of the discussion on page 40. The report should include both additional presentation of data and analyses relevant to a given topic and organization to consolidate topic areas to reduce repetition and redundancy as well as streamline the presentation of information. If the data, analyses, or graphs are unavailable, absence of this data and analyses needs to be addressed.	1
40	3	1447-48	W3, W5, W8, W9- It is extremely unlikely that a sturgeon would eat a longfin smelt at any stage of its life (i.e., larval to adult). This passage should highlight other known nonnative predator species such as largemouth and smallmouth bass, Mississippi silverside, nonnative centrarchids, and catfish.	1
42	3	1448	W3, W8, W9- The abundance and biomass of largemouth bass has increased substantially in the Delta in recent years. The potential effect of increased predation mortality on longfin smelt should be discussed in more detail. This should be discussed in context with the statement on lines 1432-33: "We are not aware of any low-salinity or marine predators of small fishes that have greatly increased in abundance over the past several decades." This passage is referencing age-0 fish so it is unclear why the predators would need to be low-salinity or marine species. LMB are an obvious example of a predatory species that has increased in abundance dramatically over the past several decades. The draft report details on page 43 why they believe LMB predation is unlikely; however, with predicted upstream movement of X2 over time and subsequent shifts in LFS distribution into the central and south Delta, LMB and LFS co-occurrence will be more likely.	1
43	3	1458	W3, W6, W8- The report needs to address that opportunistic predation by silversides and other fish and macroinvertebrates would be a factor for both eggs and larvae, not just larvae alone.	1
43	3	1479-85	W3, W6, W9- The report should highlight the fact that in addition to a lower abundance, longfin smelt are likely more vulnerable to predation pressure because of the regime shift in the Delta, which is now numerically dominated by nonnative species, many of which are predatory.	1
44	3	1496	W1, W2, W3, W5, W6, W7, W8, W9- The report needs to include adequate scientific analyses and support for assumptions. The discussion should describe the information used to conclude that predation by striped bass and other fish is not an important factor in the population trajectory of longfin smelt. Uncertainties and other factors should also be addressed. Striped bass, for example, co-occur geographically and temporally with longfin smelt throughout the longfin smelt lifecycle. Changes in water clarity and proliferation of SUV could be shifting predator-prey dynamics for longfin smelt and should be discussed in more detail.	1
47	3	1634	W3, W6, W9- There have been studies of fish entrainment at agricultural water diversions in the Delta. The discussion needs to include a description of the occurrence of longfin smelt found in these studies. In general, the report should include adequate discussion and documentation of key references to potential stressors on various lifestages of longfin smelt.	1
49	3	1681	W1, W4, W8, W9- Need additional description to explain the relevance of information. The draft report notes that a combined total of longfin smelt collected in salvage at the SWP and CVP export facilities between 1993 and 2007 was 1,133 fish. This number is hard to interpret. What is the relevance of this discussion? Has the number of smelt collected in salvage as reported been expanded to account for subsampling, etc. For context, how many longfin smelt were collected in the Bay Study sampling between 1993 and 2007?	1

Page	Chapter	Line Number	Comment	Reviewer
49	3	1705	W1, W2, W3, W6, W8, W9- Additional model and scientific support is needed for these assertions and uncertainty needs to be addressed. Did the PTM model simulations assume that longfin smelt were represented as neutrally buoyant particles? How would this affect results for juvenile and adult smelt with swimming behavior? The report should present the empirical evidence that adult longfin smelt actively spawn in the San Joaquin River upstream of the export facilities. How representative are these model results for the entire spawning population? Are model results available for longfin spawning in the Sacramento River or Suisun Bay areas? What do results of those model runs show regarding entrainment risk?	1
49	3	1712	W2, W4, W6- Figure missing and additional description needed. The discussion needs include a graphical representation of the longfin smelt relationship between salvage and OMR reverse flows, which is cited as Figure 8 on line 1712 but not included in the draft report. Additionally, the report should describe how this relationship has been used as the basis of OMR management.	1
49	3	1736	W5, W9- The report should discuss the effects of existing regulations on longfin smelt, whether direct or incidental, for instance SWRCB D-1641.	1
50	3	1750	W2, W3, W5- The report needs to describe the data and technical basis for the conclusion that OMR reverse flows affect hydrodynamics in the upper Cache Slough. This conclusion does not seem to be correct or supported by available hydrologic simulation model results.	1
50	3	1757	W2- Figure 3.5 needs to be improved. It is difficult to read and the SLS densities do not appear to be included on the figure.	1
50	3	1764	W2, W3, W6- Proportional loss estimates of larval longfin smelt based on PTM simulation modeling for conditions prior to the 2008 USFWS BiOp are not representative of current operations or risk and are therefore misleading. The outdated data needs to be replaced with updated modeling results under current regulatory conditions.	1
51	3	1790	W2, W3, W4, W7- A comparison of the percentage decline between a single peak year and a single low abundance year is misleading as it overstates the magnitude of the population decline over time. A regression through the time series should be included to provide a better depiction and characterization of the population trend.	1
51	3	1793	W2, W3, W6- Figure 3.6 only includes data through 2018. The graph needs to be updated to include data through 2021 as the report should represent current conditions in the Bay-Delta. If the updated data or analysis are not available, the report should identify the data gap and acknowledge the associated uncertainty.	1
52	3	1795-99	W2, W3, W4, W7- A comparison of the percentage decline between a single peak year and a single low abundance year is misleading as it overstates the magnitude of the population decline over time. A regression through the time series should be included to provide a better depiction and characterization of the population trend.	1
52	3	1799	W1, W4, W5, W6, W7, W8, W9- The discussion needs to identify survey limitations of the FMWT on both seasonal and geographic coverage of the longfin smelt distribution. Further, the report needs to address the fact that the FMWT samples only the pelagic region of the water column during the day with earlier discussion in the report, see page 10 lines 387-397, that stated longfin smelt migrate vertically and occupy deeper water during the day where they would not be collected effectively using the midwater trawl.	1
52	3	1801	W1, W2, W4, W5, W6, W7, W8, W9- The report needs to address uncertainties with findings and data. The earlier text describes the outflow-abundance relationship producing higher juvenile and adult abundance in wet years (2017 and 2019 were characterized as recent wet years) while Figure 3.7 shows no major increase in abundance indices for these two recent wet years. The uncertainty and variability in the apparent relationship in recent years should be discussed in greater detail. If longfin smelt production and survival is now limited by the decline in prey densities would the population respond to Delta outflow as they did in the past? This discussion would benefit from consolidation with the earlier discussion of the flow-abundance relationships and how they have changed over time. The discussion of a lack of understanding of the mechanisms underlying the flow-abundance relationship and the factors that have contributed to the observed step changes in the relationships should also be integrated with this discussion.	1
52	3	1807	W2- The text states Figure 3.7, but the results being discussed are those in Figure 3.8. Figure number references should be checked and corrected.	1
52	3	1807-12	W2, W3, W4, W7- A comparison of the percentage decline between a single peak year and a single low abundance year is misleading as it overstates the magnitude of the population decline over time. A regression through the time series should be included to provide a better depiction and characterization of the population trend.	1
53	3	1815	W1, W2, W3, W6, W8- The draft report includes data from the Bay Study through 2018. The report should discuss the Bay Study results for longfin smelt abundance under 2017 and 2019 wet year conditions and whether the population respond to high Delta outflow conditions as predicted earlier. If updated data and analyses of the Bay Study is not available, the report should identify the data gap and acknowledge the associated uncertainty.	1
54	3	1824	W1, W6, W9- The limitations and uncertainties in each of the current surveys (e.g., size-specific gear collection efficiency, etc.) needs to be disclosed and discussed. The trends in abundance are all based on relative indices and not actual population abundance estimates. The report should discuss why after decades of monitoring, actual population abundance estimates with confidence intervals are not available for various lifestages of longfin smelt each year. This discussion can be consolidated with the earlier discussion of the same topics to strengthen the presentation in the report.	1
54	3	1824	W1, W4, W5, W7, W8, W9- The low estimates for 2017 and 2019 shown on Figure 3.10 should be discussed in greater detail. In particular, the report should discuss the uncertainty in the flow-abundance relationship currently and in the future. The report also needs to discuss whether the basic trophic dynamics effecting zooplankton as a prey resource for longfin smelt have been depleted by clam grazing, whether that depletion is now the dominant limiting factor on longfin smelt abundance and production, and whether the earlier flow-abundance relationship is still valid.	1
54	3	1827	W2, W3- Figure 3.10 needs to be updated to include data through 2021. If the updated data or analysis is not available, the report should identify the data gap and acknowledge the associated uncertainty	1
54	3	1828	W2- Additional modeling should be included. Given the temporal and spatial constraints of the FMWT, the report should include a graph, similar to Figure 3.10, comparing Bay Study otter trawl and MWT data.	1
55	3	1870	W1, W2, W3- Provide justification for excluding density dependence from the PVA analysis despite evidence that it is occurring (e.g. Nobriga and Rosenfield 2016), as well as its inclusion in the model described in Technical Note 4. Both of the population viability models included in the draft report have serious flaws and were considered in the report to be a “work in progress.”	1
56	3	1898-1900	W1, 3, W4, W5, W6, W7, W8- “[E]ven though it is most likely that population size is declining … we cannot exclude the possibility that mean growth rates are stable or increasing.” This finding seems to conflict with earlier statements in the draft report that the population is at historic low levels of abundance and will remain at these low levels. This requires further explanation to address questions such as: what data were used and what analysis was performed to support this statement, was the analysis done separately for age 1 and age 2 longfin smelt using the two gears from the Bay Study. Further, this information indicates the population may be declining or it may be increasing. The report needs to identify high degree of uncertainty in the current status of the species and projection of population level. The implications of this unknown and uncertainty need to be addressed throughout the report.	1
56	3	1906-09	W1, W2, W6, W7- The report should address how accurate the conclusion is that the quasi-extinction model is at 20% in the next five years and 50% by 2035. Likewise, the report should explain the degree of certainty and discuss how “quasi-extinction” level was determined, if data show significant uncertainty in how many longfin smelt previously existed and currently exist. In addition, given the discussion above regarding lines 1898-1900 there is a high degree of uncertainty regarding the population trend and status. The report should provide a robust and clear discussion of the data and analyses used in estimating “quasi-extinction” risk, level of uncertainty, potential sources of bias, whether analyses conducted use various data sets (e.g., age 1 from the CDFW Bay Study otter trawl, Bay Study midwater trawl, etc.), and whether there is agreement and convergence among all of the analyses.	1
56	3	1906-09	W1, W2, W6- In other SSA documents, results of a robust population-based Population Viability Analysis (PVA), based on estimated population abundance by lifestage, are used to assess extinction risk. The report should have a robust discussion of the reasons a PVA based on abundance estimates is not used for longfin, what uncertainties are associated with a PVA based on indices of abundance, including the uncertainties associated with the data underlying this PVA, and a description of the actions being taken to develop stronger lifecycle and demographic models for the species.	1
59	3	1951	W1, W9- The report should include a broader and more comprehensive discussion of key stressors and drivers for longfin smelt reproduction, growth, and survival by season and lifestage. For instance, Figure 4.19 indicates that water temperature, freshwater flow, food resources, and ocean conditions are the four most important factors affecting longfin smelt abundance. The report should include a robust discussion of each species need, the associated uncertainties and data gaps, and the likely associated impact on longfin smelt abundance.	1
59	3	1965-75	W1, W2, W3, W4, W5, W6, W7, W8, W9- “Freshwater flow is the most important species need.” Additional discussion is needed regarding this conclusion as data presented earlier and previous discussions in the draft report do not support this conclusion. A more robust analysis of the flow-abundance relationship is needed including analyses for various age classes and collection methods (e.g., age 1 from the Bay Study otter trawl) with data through 2021. The most recent data does not seem to show a major population response to seasonally increased outflow. Since the population appears to be declining despite outflows in recent years, factors other than flow may now be the most important limiting factors. All key factors and their importance need to be discussed along with the uncertainties associated with each.	1
60	3	2016	W1, W2, W3, W5, W6, W8, W9- Predation is not included here as a stressor despite it being highlighted as a concern in the Current Conditions section of the draft report and included as a factor that may affect extinction risk in the Conclusions. Although there is little available data on predation impacts to longfin smelt, it is likely a significant factor in the population dynamics of the species (given the highly modified state of the Delta fish community and the prevalence of invasive fish species) and should be considered for inclusion in the Stressors section.	1
61	3	2020-21	W1, W3, W7, W9- “Despite protective measures implemented under CESA, abundance and distribution continues to decline.” The draft report lacks and needs to provide scientific justification for this conclusion. Further, the report needs to identify and discuss the uncertainties associated with the lack of information on the mechanisms underlying the population trends. This data gap creates large uncertainties in the evaluation of the performance of actions implemented under ESA for other species and needs to be addressed in the report.	1
68	4	2280	W1, W2, W3, W4, W5, W8, W9, W10- The report needs to identify all assumptions and the scientific justification for those assumptions. Currently, the analysis of future climate change and Suisun Bay/Delta water temperatures assumes no future adaptation of thermal tolerance or behavior and habitat selection by longfin smelt, however, the draft report does not identify this assumption nor does the draft report include an adequate discussion of the supporting scientific basis. Additionally, the report needs to discuss how fish species continually adapt to dynamic environmental conditions. The potential for longfin smelt to successfully adapt to future climate change in the Bay-Delta should be discussed and identified as an uncertainty in the report.	1

Page	Chapter	Line Number	Comment	Reviewer
77	4	2442	W1, W2, W3, W4, W5, W8, W9, W10- The analysis of future climate change focuses very narrowly on species tolerance but does not include a wider discussion about how temperature increases may affect biotic habitat conditions. For example, trophic food webs will likely be affected as some prey resources are known to increase in abundance with warmer water temperatures and many of the invasive fish species in the Delta have higher thermal tolerances. This section should include some summary of how temperatures are expected to affect habitat conditions.	1
78	4	2478	W1, W9, W10- The report should present a broad range of future conditions and a robust discussion of future uncertainty. Further, the Service should identify whether the projected levels of development and changes in future water demand presented in the report are consistent with future planning by DWR and USBR.	1
78	4	2487	W1, W9, W10- The report should present a broad range of future conditions and a robust discussion of future uncertainty. Further, the Service should identify whether these projected changes in precipitation presented in the report are consistent with future planning by DWR and USBR.	1
78	4	2487-2501	W1, W4, W6, W8, W10- The climate change models predict a shift in outflow to greater Delta outflow in January-April and reduced Delta outflow in May-July due to a predicted shift from snowfall to rainfall. The report should include an expanded discussion of how this predicted shift in seasonal hydrology may affect longfin smelt spawning, larval development and transport, geographic distribution, food supplies, etc. The uncertainty in the future climate change scenarios should also be discussed and quantified using ranges of future predictions.	1
79	4	2501-02	W1, W8, W9, W10- The comparative scales of climate change graphs are confusing and potentially misleading. The information should be presented in a clearer manner, such as by comparing Y-axis of historical inflow graph with Y-axis of climate change prediction graph. Further, the effects on the species and uncertainty of future climate change scenarios, and range of those projections need to be included.	1
80	4	2530	W1, W2, W4, W5, W6, W7, W8, W9- See earlier comments regarding the changes in the flow-abundance relationship and the apparent poor response in recent wet years. [Replicated here for ease of review: The report needs to address uncertainties with findings and data. The earlier text describes the outflow-abundance relationship producing higher juvenile and adult abundance in wet years (2017 and 2019 were characterized as recent wet years) while Figure 3.7 shows no major increase in abundance indices for these two recent wet years. The uncertainty and variability in the apparent relationship in recent years should be discussed in greater detail. If longfin smelt production and survival is now limited by the decline in prey densities would the population respond to Delta outflow as they did in the past? This discussion would benefit from consolidation with the earlier discussion of the flow-abundance relationships and how they have changed over time. The discussion of a lack of understanding of the mechanisms underlying the flow-abundance relationship and the factors that have contributed to the observed step changes in the relationships should also be integrated with this discussion.]	1
80-81	4	2536-38	W1, W9, W10- The report should include an expanded discussion of the high degree of uncertainty associated with the response of longfin smelt to future climate change conditions. For instance, if longfin smelt move into the ocean as water temperatures rise and temperature is expected to rise under either climate change model, it is highly speculative to assume that later spring/early summer months with predicted dry conditions will affect longfin smelt, which may move into the ocean earlier in the year. The uncertainties of any assumption or conclusion need to be identified and discussed throughout the climate change analysis.	1
81	4	2548-49	W1, W5- The draft report states that "the projected decline in delta inflow beginning in early April could limit the duration of beneficial freshwater flow mechanisms that longfin smelt larvae experience." This is highly speculative especially without identifying what those mechanisms are. This statement should be modified to highlight that uncertainty.	1
81	4	2563-65	W5- It is unclear why there will be such a "steep flow dropoff" given the nature of water storage in California. Most major river systems are dammed which should provide a buffer against these types of flow drop-offs. The text should acknowledge this possibility.	1
87	4	2738	W5, W8, W9, W10- Explain/discuss why sea level raise will result in a substantial loss of longfin smelt habitat rather than colonization and vegetation propagation (wetland marsh plants) at higher channel margin elevations as water levels rise. With rising sea level, the location of the open water-wetland boundary will change but it is not clear that the wetland area will diminish. SFEI may have a better explanation and basis for predicting habitat changes in response to different levels of sea level raise that could be included in the report discussion.	1
89	4	2784	W2- Figure 4.16 is very hard to read, improve resolution of the image.	1
89	4	2794	W1, W2- "This suggests that salinity intrusion resulting from sea level rise will likely hedge on inflow." The report should include additional discussion of the data, assumptions, and modeling analyses used to support this finding. What would this mean for longfin smelt management, in light of previous prediction that increased rainfall / decreased snowpack will increase inflow early in the year? Can we just expect that longfin smelt spawning habitat would move upstream?	1
89	4	2799	W1, W3, W5- In addition to presenting the extreme case (X2 movement 11 km upstream), the report should present the most likely case of predicted conditions and the predicted range of responses to better characterize uncertainty in the predictions.	1
90	4	2804-05	W1, W2, W4- Explain what is meant by "on January 1, 2002, significant salinity increases are evident in the Delta." The below figure appears to be ambient salinity conditions, so it is unclear where the increases are from.	1
90	4	2817	W1, W10- The discussion of potential effects of sea level raise on longfin smelt habitat needs to address level of uncertainty in the estimates and inter- or intra-annual variability.	1
91	4	2858	W9- In addition to competing for prey with longfin smelt, silversides are thought to prey on longfin smelt eggs and larvae. Moyle (2002) discusses predation by silversides as a factor potentially accelerating the decline of longfin smelt.	1
93	4	2929	W6- Densities should be expressed as number per m2.	1
93	4	2937	W10- The report should discuss preventative measures like offshore ballast water discharges that have been adopted to help reduce transport of non-native species into the Bay-Delta, as well as the effects of these measures on the species.	1
95-97	4	2996-3027	W1, W2, W5, W6, W7, W8, W9- The discussion in the report needs to provide additional information on the development of this model and the basis and data used to develop each of the environmental covariates. The report needs to include a description of how the model was calibrated and calibration results (using data not included in the model development) and whether the model and model results have undergone independent peer review. The report also needs to include a discussion of the results of sensitivity analyses of key parameters and model relationships (i.e. how various factors effect outcome of the model), any autocorrelation among variables, a comparison of actual patterns observed in different field monitoring programs over a range of environmental conditions and results predicted by the model to assess if model results make sense biologically, and a discussion of whether there are any biases detected in the model or significant outliers between predicted and observed results. Further, the quantification of confidence intervals needs to be included for any model results provided in the report. The discussion should also include description, context, and uncertainties related to findings, e.g. of relative trends, etc.	1
99	4	3044-47	W1, W2, W5, W6, W7, W8, W9- "…the probability of extinction is roughly 50-90%. This could be argued as the 'best case scenario' as it is assuming that there are no changes to the extant habitat stressors in the system over time." The report needs to address uncertainties and questions such as: do data and modeling accurately support this; do data and modeling adequately account for the possibility that existing regulatory efforts could actually improve longfin smelt survival; and, based on uncertainties, is this really the "best case scenario." This fundamental finding of the draft report needs robust documentation and analyses, including further description of the model and incorporated assumptions and uncertainties. The report needs to identify and discuss the uncertainty associated with this extinction analyses. Characterizing the results as "rough" requires further discussion. The report should include quantitative population abundance estimates and a more rigorous PVA analysis that have been subject to independent peer review and comment.	1
100	4	3051	W1, W4, W9- In Table 4.4, predation is once again included as a future factor (despite not previously including it as a stressor) although the magnitude of risk from increased predation pressure is uncertain. It is unclear why increased predation stress would not at least 'moderately increase' the risk to the species.	1
100	4	3069	W1, W2, W4, W8- The draft report is currently lacking and needs to include a substantive discussion of the scientific analysis and justification for the conclusion that current April-June flows are insufficient to support longfin smelt productivity. The analyses performed on the relationship between April-June Delta outflow and hatching success, larval growth, larval survival, etc. that support this finding need to be identified and discussed in the report. The report needs to include the results of analyses of these relationships and an assessment of the mechanisms underlying how reduced April-June flows are adversely affecting population demographics of eggs, larval and early juvenile lifestages of longfin smelt. If this data or analyses are not available, the report needs to identify the data gap and the uncertainties associated with the unsupported assertion.	1
103	4	3135	W3- The Literature Cited should be updated to include only those references cited in the report text.	1
124	Appendix	4107	W1, W2, W5, W6, W7, W8, W9- Technical Note 1 - Population Viability Analysis: The report notes that this is a "relatively simple version" (4116) of a PVA. There are several issues with the underlying survey data, especially considering the inclusion of the FMWT which has been shown to not represent the population dynamics of the longfin smelt population very accurately. The report notes that the "abundance indices used here are relative indices of abundance, which means that although they are assumed to track changes in the population they do not have a direct correspondence to absolute population size" (4250-4252). For all surveys, the lack of known detection efficiency, lack of abundance estimates, and non-random selection of sites for all surveys serve to weaken the results of the analysis and assumptions that can be made. The report notes that "because of this, they are not the best data source for an analysis of population viability and the results of this analysis may be more applicable to evaluating the surveys themselves than the abundance of longfin smelt directly" (4252-4254). The model estimates population growth rates based on "simple unstructured population dynamics" (4158) that do not incorporate age or structure of the population or density dependence, as has been suggested in the draft report and proposed by Nobriga and Rosenfield (2016). As such, the model should be considered a work in progress that will be "updated as refinements are made to the data, analysis, and presentation" (4245). Furthermore, the quasi-extinction thresholds that were chosen "were not informed by considerations of when management could still be effective for managing population sizes" (4237-4238) and therefore may not represent the true trajectory of the population.	1

Page	Chapter	Line Number	Comment	Reviewer
171	Appendix	5405	W1, W2, W5, W6, W7, W8, W9- Any assumption or conclusion based on the model needs to also identify the high degree of uncertainty. Technical Note 4 - Population Forecasting Model: The model itself is a reasonable method for viability assessments but underlying issues remain with the data and analysis and conclusions are limited as a result. The report notes that the analysis is a “work in progress” (5585), which does not seem like something that should be relied on for determining species viability. Issues with survey data have been highlighted, including lack of known detection efficiency, lack of abundance estimates, non-random selection of sites, etc., which all serve to weaken the results of the analysis and assumptions that can be made. In addition, the model only used the most recent ten years of data (2005-2016) and did account for the full time series as the report notes that “it did not seem realistic to use estimated vital rates from early in the time series to make forecasts of the foreseeable future; it seemed more reasonable to sample the estimates for the last ten years of the time series” (5511-5512). As a result, the model reflects only a handful of years with poor conditions in the estuary and creates a “negative feedback loop with decreasing production and decreasing survival” (5560-5561). Furthermore, environmental covariates chosen to explain patterns in the data were selected haphazardly and “inclusion probabilities for covariates varied somewhat across the vital rates” (5551-5552), and the report notes that “it has been difficult to make reasonable predictions based on one (or a few) variables” (5598-5599). As such, the report needs to highlight the high degree of uncertainty associated with any conclusion about the factors driving population dynamics from this model. Further, conclusions from the model should not be used to derive insights into the mechanisms for increasing abundance.	1
6	2	248	Please clearly state if Longfin Smelt have multiple clutches in a season	2
6	2	248	I believe semelparous mean that the species has only a single reproductive episode (generally) in its life cycle. It is not whether the species spawn once a year or more frequently.	2
11		414	There was no model details to explain what was done and how juvenile and adult phases are differentiated.	2
		420	"We predict"? Is this a hypothesis or result from the model?	2
		423-424	I don't understand this sentence. Why report it then?	2
		426-432	What is the definition of length-at-age 0? I always thought of age-0 as any fish born that year so it's not clear what this paragraph is saying. There also isn't sufficient model information for us to understand what is being reported. I understand that the technical notes have the details, but these paragraphs contain so little information that it's not useful. I would at least describe what data or models were used, just as an abstract typically would.	2
		443-445	The phase "we predicted" should be rephrased or clarified. I don't understand whether they are describing the model or prediction/hypothesis of the authors.	2
		506	Doesn't this whole study use boosted regression trees? Why not just say Phillis et al. boosted regression trees showed that strongest predictors of juvenile….. In the first sentence of the paragraph instead?	2
		509-510	"lower salinity in more of San Pablo Bay" awkward phrasing and lower salinity in San Pablo Bay than dry years? Is that what this sentence trying to convey?	2
		525	What is this Bay Study fish distribution analyses referring to?	2
		526	young is referring to young-of-year I assume?	2
		539-541	Peterson and Barajas (2018) should also be noted here, since they found similar evidence, where when abundant, longfin smelt were more likely to be detected by midwater trawl (oblique tow) than otter trawl. Peterson JT, Barajas MF. 2018. An Evaluation of Three Fish Surveys in the San Francisco Estuary, California, 1995–2015. San Fr Estuary Watershed Sci. 4(2). doi:10.15447/sfews.2018v16iss4art2.	2
		548-549	This shallow shoal detections is a different idea. What is the citation? Is this still Steinke et al.?	2
		573-574	Is this correct? Is not possible even with otolith information? Or is this just referring to the particular study because such data wasn't collected?	2
		598-599	What is the significance of 15 psu? And is this a typo? >15 psu could also mean marine waters, so how does this support the theory of resident longfin?	2
		614-616	When does this shift occur?	2
		617-619	I would edit this sentence and the previous one to make your idea more clear (e.g. that the larger prey items are the mysids).	2
		633	"effectively" is subjective and dubious considering that most of these don't capture a good portion of longfin smelt full distribution in the estuary.	2
		652-653	The term "could" is not appropriate here. It certainly misses the early recruitment of these small longfin into the gear. I would argue that it's pretty clear from the data and Mahardja et al. (2017) paper on the 20-mm survey suggests that the 20-mm net is fairly effective for even small <20 mm FL Longfin Smelt, likely due to their high densities in the late 90s and early 2000s (and see supporting info for figure on what sizes of longfin the survey typically observe by day of year). Mahardja B, Young MJ, Schreier B, Sommer T. 2017. Understanding imperfect detection in a San Francisco Estuary long-term larval and juvenile fish monitoring programme. Fish Manag Ecol. 24(6):488–503. doi:10.1111/fme.12257.	2
		699-708	Why was EDSM not discussed at all? This is a highly intensive survey that can potentially provide high spatiotemporal resolution data for Longfin Smelt in the long-run. The catchability of longfin smelt by Kodiak trawl is something that is probably worth looking into as well.	2
		735-736	Finding from Feyrer et al. (2015) that age-0 longfin smelt is associated with North Pacific Gyre Oscillation should at least be noted here or somewhere else in the document. Feyrer F, Cloern JE, Brown LR, Fish MA, Hieb KA, Baxter RD. 2015. Estuarine fish communities respond to climate variability over both river and ocean basins. Glob Chang Biol. 21(10):3608–3619. doi:10.1111/gcb.12969.	2
		781-783	Why cite a statement from Stevens and Miller 1983 that contained no analysis when there are a few papers demonstrating the association between high turbidity and larval longfin smelt through occupancy models? See Mahardja et al. (2017), Grimaldo et al. (2017), and Peterson and Barajas (2018). Feyrer F, Cloern JE, Brown LR, Fish MA, Hieb KA, Baxter RD. 2015. Estuarine fish communities respond to climate variability over both river and ocean basins. Glob Chang Biol. 21(10):3608–3619. doi:10.1111/gcb.12969.	2
		814	I would suggest using the word "directly observed" instead of "detected". The current sentence seems to imply that there is regular egg sampling or monitoring, when in fact, it may be simply because there haven't been many studies that attempt to collect longfin eggs.	2
		825-833	You may want to list the typical range that you would see for this response variable (spawning migration window) rather than just listing the predictors (which is not incredibly relevant for the purpose of the SSA given that it's still being worked on and fluid as far as I know).	2
		850-852	You can actually cite the following paper instead of simply stating this as fact without supporting information: Tamburello N, Connors BM, Fullerton D, Phillis CC. 2019. Durability of environment–recruitment relationships in aquatic ecosystems: insights from long-term monitoring in a highly modified estuary and implications for management. Limnol Oceanogr. 64:S223–S239. doi:10.1002/lno.11037.	2
		913-915	Neomysis shows a positive relationship with X2 (higher in low flows) in post-clam period according to Kimmerer 2002.	2
		926-930	This food mechanism section could benefit from more nuanced discussion about the merits of this flow-food mechanism hypothesis. Kimmerer et al.'s (2019) paper explains the difficulty and complexity of understanding the relationship between prey items and flow, and in fact, indicate that some copepods would have lower productivity during wetter years because of current conditions. Kimmerer WJ, Gross ES, Slaughter AM, Durand JR. 2019. Spatial Subsidies and Mortality of an Estuarine Copepod Revealed Using a Box Model. Estuaries and Coasts. 42:218–236. doi:10.1007/s12237-018-0436-1.	2
		945-952	It should be mentioned again that there has been an overall decline of turbidity/sediment supply in the estuary as well, which may explain the change in intercepts over time for the longfin-flow relationship. Hestir EL, Schoellhamer DH, Morgan-King T, Ustin SL. 2013. A step decrease in sediment concentration in a highly modified tidal river delta following the 1983 El Niño floods. Mar Geol. 345:304–313. doi:10.1016/j.margeo.2013.05.008. http://dx.doi.org/10.1016/j.margeo.2013.05.008. Hestir EL, Schoellhamer DH, Greenberg J, Morgan-King T, Ustin SL. 2016. The Effect of Submerged Aquatic Vegetation Expansion on a Declining Turbidity Trend in the Sacramento-San Joaquin River Delta. Estuaries and Coasts. 39(4):1100–1112. doi:10.1007/s12237-015-0055-z.	2
		977-983	There is a preprint showing this exact relationship between flow and cooler water temperatures in spring. It also contains information that would be relevant to Figure 2.8 and its discussion of timing for the various flow-fish mechanisms. Bashevkin SM, Mahardja B. 2021. Seasonally variable relationships between surface water temperature and inflow in the upper San Francisco Estuary. EcoEvoRxiv. doi:10.32942/osf.io/rqbdk. https://doi.org/10.32942/osf.io/rqbdk.	2
		1027-1032	Some mechanisms are more plausible and well-tested than others.This point should be made somewhere. This section seem to treat all of them as equally valid.	2
		1038-1039	Re: wet years continue to have some positive influence. It is also important to note that while this is true, there has also been a general decline in Bay-Delta longfin smelt population over the time series. Delineating the different mechanisms behind this flow-fish relationship is important for the species instead of simply relying on the flow=good idea. For example, if turbidity is a strong driver in this flow-fish relationship, it should be a concern that sediment supply has been declining and submerged aquatic vegetation has been increasing.	2
		1038-1039	Re: Cite to a fig in the SSA that shows this. Yes, that would be ideal.	2
		1068	Is there evidence that reduction in flow is the primary stressor? What about other impacts such as invasive species and habitat loss from the channelization and land conversion of the Delta? I don't disagree that it's likely a huge impact, but water export has stabilized or declined since mid 2000s and it hasn't lead to a long-term increase in longfin population (flow reduction has also been around for longer than the POD). Labeling this as the primary stressor would require more supporting information than just what's in this section.	2
		1104-1106	Citation for this statement that these months are particularly crucial?	2
		1200-1204	Citation needed	2
		1222-1225	I'm not sure that this oversimplification is necessary nor helpful for the species. While I agree that outflow is a key driver and necessary for the sustainability of the population, this type of language implies that increased outflow is all we need, which I don't think the authors intend to say. The species having the strongest correlation with flow relative to other species doesn't indicate that it is the only or main factor that drive its population abundance. It's important to keep in mind that none of the studies cited so far have attempted to compare outflow with other stressors such as turbidity, food, invasive species numbers, or habitat availability; all of which have gotten worse for Longfin Smelt. The fact that there has been an overall decline in the population regardless of water year type is a point that was hinted at in earlier parts of this section and bears repeating here.	2
		1300	I would clean up the R code and spell out the species or taxa names on the figure (3.4)	2
		1368-1382	I think it's worth noting that there's a relationship between flow and temperature, which may be a partial driver to the flow-fish relationship. See: Bashevkin SM, Mahardja B. 2021. Seasonally variable relationships between surface water temperature and inflow in the upper San Francisco Estuary. EcoEvoRxiv. doi:10.32942/osf.io/rqbdk. https://doi.org/10.32942/osf.io/rqbdk.	2
		1432-1433	We don't monitor adult Striped Bass well in the estuary. They're anadromous and they could have had an impact on fish long before monitoring began. See: Nobriga ML, Smith WE. 2020. Did a shifting ecological baseline mask the predatory effect of striped bass on delta smelt? San Fr Estuary Watershed Sci. 18(1). doi:10.15447/SFEWS.2020V18ISS1ART1.	2
		1495-1496	I don't know if there's enough information to say one way or another at this point. What's the evidence to support this idea that striped bass predation is a non-issue? Adult striped bass can migrate into the ocean and back, and they have been around for a long time.	2
		1746-1748	I'm confused by this statement. What does nighttime migration have to do with entrainment? Pumping occurs regardless of time of day.	2

Page	Chapter	Line Number	Comment	Reviewer
		1757	Figure 3.5 is unclear. I understand the CVP and SWP salvage figures, but what are the figures to the right? SLS catch? If so why are they different between top and bottom?	2
		1965-1968	Again, I think this is a fairly inappropriate conclusion to make considering the stated impacts of POD factors and invasive clams. An apt comparison of the relative importance of longfin smelt drivers need to be made if one is to make such a statement (i.e., that it is THE biggest stressor) and there hasn't been one in this document so far. These sentences also miss the point that water projects have no control over the climate. A wetter year tends to lead to more outflow. To my knowledge, water projects don't typically control outflow so much so that a high precipitation year become drought-like because of environmental regulations that are in place (and vice versa, water projects can't release more water in a drought when there is none to release). I would recommend stating that it is one of the most important stressors, rather than making a claim (without a proper analysis) that it is the biggest one.	2
		1998-1999	I wasn't aware of any direct evidence of this ability (vertical movement). Is there a citation? Or is this just an assumption based on field data? This statement makes this seem like a known fact.	2
		2000-2001	This point of this sentence (stratification may not offer much refugs) stands, but there is certainly data that indicate that difference between surface and bottom water temperature can be much bigger than 0.3 C. This is apparent just by looking at the Bay Study data alone. I'm also not sure where in Vroom et al. that< 0.3 C is mentioned. Did you mean <0.2 C? That's what Vroom et al. mentioned, and that's on average (not maximum difference). Also, see my recently posted pre-print for more information: Mahardja, B.; Bashevkin, S.; Pien, C.; Nelson, M.; Davis, B.; Hartman, R. Escape From the Heat: Thermal Stratification in a Well-Mixed Estuary and Implications for Fish Species Facing a Changing Climate. Preprints 2021, 2021110401 (doi: 10.20944/preprints202111.0401.v1).	2
		2016	What is this table and how are these risks defined? What does the color mean? Why would temperature for adults be moderate risk considering climate change? This table seems so definitive that it needs further explanation or at least a caption of some sort.	2
		2118-2121	Note that we've demonstrated that warming has already occurred in the estuary (paper received minor comments and will be resubmitted in a few weeks or so): Bashevkin SM, Mahardja B, Brown LR. 2021. Warming in the upper San Francisco Estuary: Patterns of water temperature change from 5 decades of data. EcoEvoRxiv. doi:10.32942/osf.io/6u47y. https://doi.org/10.32942/osf.io/6u47y.	2
		2241-2243	Do you mean influenced by marine/ocean conditions? The Delta is also tidally influenced.	2
		2302	What is being referred to as "this timeframe"? Do you mean year-round?	2
		2351-2353	Is there any citation that describe in more details what this study entails?	2
		2354-2356	Is this a good assumption? What is this based on?	2
		2357	I would rephrase and say "somewhat consistent" instead. The figure alone is not quite convincing given that there's considerable variability from what I see.	2
		2435-2436	I know that this has been mentioned earlier, but I would make a habit of citing the proper reference/figure/data for a sentence like this.	2
		2537-2538	Citation needed for the statement that extended periods of high flow being important. I'm not aware of analysis showing that a particular hydrograph is better for longfin smelt.	2
		2563-2565	I'm not sure what this sentence is trying to convey. What does possessing the representation mean?	2
		2586-2592	Again, I think a proper comparison of the various drivers would be needed to make this statement that reduction in flow is the primary driver of longfin decline. How does that compare to invasive clams? Or reduction in turbidity? Or loss of wetland habitats? Reducing the drivers of longfin smelt population dynamics into a single dimension simply because it is the easiest to measure would not be helpful for ensuring species recovery. Especially when we know that there has been a general decline regardless of water year types.	2
		3050	What do the colors in the table mean?	2
		248	Semelparous does not mean they spawn annually. It means they spawn once in their lifetime.	2
		321	The usage of Lake Washington is confusing without the reminder it's the one in Seattle and not the one connected to the end of the Sacramento Deepwater Ship Channel that also has Longfin Smelt.	2
		420	Is there literature to support the prediction mentioned? If not, then this is just speculation.	2
		422-424	The sentence here is confusing. Is this for adults or juveniles? Is this another speculation, or is there literature? This need clarity or removal.	2
		433	This section does not read well. It has lots of predictions, unknowns, and perspectives. There is no literature cited in this entire section. Literature should be cited for some of the claims, such as the negative relationship between conductivity and growth	2
		464	Clarify what the 'upper estuary' means.	2
		480	When they say Bay Area Tributaries, are these tributaries to the SF Bay only? Is this trend seen in the Delta Tributaries as well?	2
		529	Does older age-0 fish mean fish that are closer to age-1?	2
		550	More speculation on what caused the change in longfin behavior. I would include the sentence, but remove the speculation on changed prey, or turbidity.	2
		569-574	This is confusing. They state that it's possible for some longfin to rear in the ocean for 3-years, however, no confirmed 3-year old longfin has been seen in the coastal ocean. This seems contradictory. Also, do longfin grow faster in the ocean? If this has been confirmed, which a source is cited, couldn't you state that longfin DO grow faster in the ocean instead of using such speculative language as 'expected'?	2
		614	A shift in diet is mentioned, but there is no information on when this shift occurs, and who it occurs for. It reads as though the whole species shifted their diets to incorporate introduced zooplankton. But then it also reads as though the diet shift occurs when a fish reaches a certain length. It's a bit confusing.	2
		633	These surveys don't capture smelt "effectively." It is my understanding that these surveys were designed to be the least invasive and as such miss detection of smelts in the Delta. Further below in the survey descriptions it calls out their limitations (line 661, or 674, for instance). The survey section seems to leave out EDSM. This may be due to EDSM focusing on Delta Smelt, but they still capture and track longfin smelt in their catches.	2
		897	Is this a true statement? I think a citation would help here.	2
		920	The Yolo Bypass can become inundated during other water year types, besides just wet. It only takes an overtopping event to become inundated, which can occur in any year type. There is construction underway to make the Yolo Bypass inundated at lower flows than currently. This all adds to the food web of the Delta	2
		1298	Figure 3.4. The names of the plankton should probably be spelled out. The species codes aren't clear. I'm not sure this figure is needed.	2
		2435	Is this true? A citation would help here.	2
		1556	Ammonia or Ammonium?	2
	31	1068-1069	A reduction from what? In all months? During the months from June to October the water projects increase flows above a "run of river". See Reclamation's 2019 ROC LTO BA modeling	2
		1080-1082	The statement on Upstream storage fails to mention human health and safety along with providing water for refuges.	2
		1092-1093	The statement uses a decade old reference to describe current conditions.	2
		1102-1104	The ROD did not include the Spring I/E, since Reclamation took a different approach for managing the anticipated take of steelhead. This approach resulted in similar protection, see NMFS 2019 BiOp.	2
		1108	Does not mention why changes are anticipated, which are "To address the review of agency actions required by Executive Order 13990 and to voluntarily reconcile CVP operating criteria with operational requirements of the SWP under the California Endangered Species Act"	2
		1110	Since the BiOps did not include Longfin smelt, "additional" should be deleted from sentence.	2
		1119-1120	"All of these regulatory measures" is a vague and confusing statement. The text above makes no mention to measures.	2
		1122	What model?	2
		1170-1171	Please provide a Reference for statement	2
		1200-1202	This statement needs a supporting citation/ reference.	2
		1213-1215	Please clarify "the influence of the water project operations". What is this in reference to?	2
4	1	43	Change the word "found' to 'concluded'. The former term makes sound like an actual study on the breeding and spawning probabilities was determined in the field.	3
9	2	185	The genetic analysis at the time of the 12 month finding had difficulty differentiating. Is this in reference to the more recent Saglam et al paper? This section of the chapter is about the 12-month finding. Recommend removing the reference of genetics analyses and when the genetic discussion is further discussed mention how that contributed to the current designation of the DPS.	3
8	2	174	Grimaldo et al 2020 should be included in the some of these citations. Grimaldo, L., Burns, J., Miller, R. E., Kalmbach, A., Smith, A., Hassrick, J., & Brennan, C. (2020). Forage Fish Larvae Distribution and Habitat Use During Contrasting Years of Low and High Freshwater Flow in the San Francisco Estuary. San Francisco Estuary and Watershed Science, 18(3).	3
9	2	191	It is stated in Garwood (2017) that longfin were caught in areas outside of the Gulf of the Farallones such as Monterey Bay. These extents should be acknowledged in this statement. Although recent surveys have not found continued occupancy of these areas approximate to the SF Bay in the north suggesting either recent loss or the occupancy of these locations are highly stochastic.	3
9	2	198	It is noted that the southward movement of Longfin smelt from the northern reaches is "unlikley". the paper by Saglam et al (2021) suggest there is some genetic exchange therefore it may be better to change the descriptor of "unlikely" to rarely.	3
9	2	205	It is not clear how this statement about biological significance and truncation are vital for the species viability. Suggest including the lines of evidence in regards to the three Rs.	3
9	2	208	Cite the 1996 DPS policy	3
9	2	217	Define 'upper estuary'. I am assuming this is east of Carquinez. Otherwise an 'upper' estuary could also include the upstream locations of the SF Bay tributaries.	3
9	2	219	Is this for the whole Bay-Delta or just the Suisun Bay- Delta?	3
9	2	222	It should be acknowledged that inputs from the San Francisco Bay Tributaries are more than just Alviso Slough. Add the inputs of Napa, Sonoma, and Petaluma Rivers.	3
10	2	233	Should include the other reaches of freshwater other then the Delta. As such there is the rest of the freshwater reaches of the tributaries of the SF Bay.	3
10	2	243	This section continues to ignore the contribution and occupancy of the SF Bay Tributaries. Why only mention the Delta. The Bay-Delta DPS occupies more than just the Delta. See Grimaldo et al 2020 and Lewis et al 2019. These are not recent phenomena as there were collections of larvae in the 80s in the SF Bay. Add the ranges of the other tributaries and not just focus on the outdated conceptual model of an obligate Delta distribution for early lifestages.	3

Page	Chapter	Line Number	Comment	Reviewer
10	2	248	At the LFS workshop in August, Tempel and Burns mentioned that there was evidence of multiple clutches and that his analysis of the hatch dates were not conclusive about the multi-brood lifehistory strategy. Is this information not being introduced into this section? Should include more citations on the semelparous nature of the species and include that the examination of a more iteroparous lifehistory strategy may be possible given the evidence and that it is a current line of inquiry. Suggest moving the discussion on line 274 up to this paragraph or to reference that discussion here to be more complete about the developing research on their lifehistory strategy.	3
10	2	251	How is 'low-salinity' defined? Add an additional statement after the mention of low salinity to include the results from Hobbs et al 2010, Lewis et al 2019 and Grimaldo et al 2020 concerning the ranges that they have evaluated.	3
10	2	254	This is really out of date. Why are only the Suisun Bay/Delta reaches discussed. It seems oddly myopic given the recent studies by Grimaldo et al (2020) and Lewis et al (2019) as well as older studies in the 80s collecting larvae in the SF Bay. Garwood (2017) also found evidence of larvae in the bays of estuaries up and down the coast. By not include the rest of the range in the SFE this synthesis seems biased toward the Delta and will likely result in spurious conclusions. The goal of this synthesis is to provide a baseline for effort throughout the range of the Bay-Delta DPS to protect and support the species. By ignoring the rest of the estuary there will be many activities that will not be properly evaluated for their costs and/or benefits to the listed species.	3
10	2	261	It appears that presentations are being considered for the inclusion in this synthesis. Is that correct or was this an accident to linclude Tempel and Burns (2021)? Does this constitute as best available science? Do those presentations meet the criteria for best available science as they were presented in the workshop? Provide the precedence for this kind of inclusion. It will be important for if and when the reviewers of this document provide additional lines of evidence that may be from works in progress like presentations or draft manuscripts.	3
11	2	265	Instead of a communication, why not just examine the data. It should be readily available and the information should be provided as part of this SSA. Also it appears the same lower range of maturity is reported and cited in the next statement. Just use those citations instead of a personal communication.	3
11	2	292	Add the information from Lewis et al (in process) manuscript as well as their presentation at the August workshop on hatching data analysis using otolith data. Also this SSA should include the analysis by Baxter (2021) that incorporates both montly hatching densities of YSL as well as incubation duration. This should have references to be considered robust. A personal communication is not sufficient and may not satisfy public review.	3
11	2	295	Temple and Burns (2021) presented their work on temperature an timing. This should be referenced. Also did their findings support this 14oC threshold. Many species of fish rely on both the time of year and temperature therefore it should included the discussion of timing and temperature as the current efforts to sample in the December would be better informed by this SSA if that were included.	3
11	2	302	Delta outflow and inflow are mentioned but it seemed like there was going to more discussion on the flow and spawning relationship. Why not include the discussion of the spawner/recruit relationship with Delta outflow in Nobriga and Rosenfield (2016).	3
12	2	309	Seems out of date to not include the SF Bay tributaries especially just after citing Grimaldo et al (2020). This paper suggests spawning year round in those reaches as well regardless of dry or wet year. Therefore include the brackish to fresh water reaches of those tributaries in this sentence.	3
12	2	313	The Hobbs/Lewis lab found larvae in dry years in those tributaries. This is counter what this statement is suggesting. This seems to be a repeated issue within this SSA. What is the reason for the exclusion of these habitats in these narratives up to this point. Provide more lines of evidence for the focus on the Delta. If there are robust reasons for this focus then why not provide it otherwise this SSA appears biased and may be undermined too easily if challenged.	3
12	2	319	This whole paragraph ignores both Hobbs et al 2010 and Grimaldo et al 2017. Which is odd since Jim Hobbs is a co-author on this document. Hobbs et al 2010 suggested that spawning occurred from fresh to brackish areas. Are you suggesting that the Delta is brackish? When did this occur? How are the results in Hobbs et al 2010 explained? In addition, Grimaldo, expressly tried to test this relationship and found that spawning is likely occurring in brackish reaches. How is that explained by upstream migration. In addition, Lewis et al (2020) acknowledged the brackish spawning lifefhistory. Why is this being ignored? Provide the reasoning. Referencing a landlocked species that is genetically distinct will not be sufficient in light of the other lines of evidence. Hobbs, J. A., Lewis, L. S., Ikemiyagi, N., Sommer, T., & Baxter, R. D. (2010). The use of otolith strontium isotopes (87 Sr/86 Sr) to identify nursery habitat for a threatened estuarine fish. Environmental biology of fishes, 89(3), 557-569. Grimaldo, L., Feyrer, F., Burns, J., & Maniscalco, D. (2017). Sampling uncharted waters: examining rearing habitat of larval Longfin Smelt (Spirinchus thaleichthys) in the Upper San Francisco Estuary. Estuaries and Coasts, 40(6), 1771-1784.	3
12	2	342	May be better to reference Tempel and Burns (2021) as there is something that can be actually observered. A personal communication is harder to defend.	3
13	2	354	Figure 2.3 does not seem to support this statement on the hatching rate declining 'rapidly'. Perhaps provide alternative relationships with temperature from other species. Perhaps compared to either Delta smelt or Wakasagi.	3
13	2	366	Provide the citation for the CDFW Tucker Trawl sampling in 1994. I am assuming this is Bennett et al 2002?	3
14	2	375	This paragraph is all over the place. It starts with a topic statement on the Advection Theory of distribution for Lonfin smelt but then drifts off into multiple topics. Better to split this up and to be clearer on the subject of each paragraph as there was too much being conflated which would confuse the information being presented. As a reference document you would want this information to be clear.	3
14	2	384	There are several hypotheses that were posed by Wim Kimmerer about the fate and transport of longfin smelt larvae. Those should be better explored as there is an increasing amount of evidence suggesting that local production and retention of those larvae in the tidal brackish regions of the SFE are a signficant lifehistory strategy. Grimaldo et al (2017, 2020) provide those lines of evidence. And there is also analyses by Wim and Ed Gross about alternative hypotheses. Why is this SSA only discussing the Advection Theory? This focus appears biased and may lead to spurious conclusions. Suggest expanding this section or at least providing more discussion on the lifehistory strategies suggested by Wim (which is in the LFS Science Plan that the LFS Technical Team has adopted. If they find it important to further study than this SSA should as well.	3
16	2	458	The tracking of X2 by larvae as reported by Dege and Brown (2004) was found to be subject to sampling frame bias as identified in Grimaldo et al (2017). Dege and Brown (2004) could not track the center of distribution properly because as the flows increased the distribution as described by the survey was skewed to far westward to be an accurate representation of the distribution of the population and the center of distribution of the species. It is not to say that the larvae do not center around the low salinity zone but that the way X2 is being used in this context represents only the direction up into the Delta and ignores the distribution into the San Francisco Bay Tributaries.	3
16	2	463	The conclusion that the distribution of larvae is "largely reflecting" the advection of the larvae is ignoring the conclusions by Grimaldo et al (2017 and 2020). Production in the low salinity zone was detected by these studies and should at the very least be acknowledged as a significant contribution to the larvae abundance. How much is still debated as the Advection versus the Local production has not been resolved.	3
16	2	482	Make sure to use the most recent report covering the subject that is being cited. There is at least another report for the DWR contract that was produced in 2019. Suggest reporting this instead of Lewis et al 2018. Lewis, L., Barros, A., Willmes, M., Denney, C., Parker, C., Bisson, M., Hobbs, J., Finger, A., Auringer, G., Benjamin, A. 2019. Interdisciplinary studies on Longfin smelt in the San Francisco Estuary.	3
16	2	491	You forgot to mentioned that the same was true for freshwater rearing. Include this into this statement by adding it to the end. Otherwise this omission seems very odd.	3
17	2	500	Tobias and Baxter is now published. Add the year to the citation and update in the bibliography.	3
17	2	508	The figure does not seem to show habitat in the West Delta. Use a better resolution of the figure to show this. Otherwise remove West Delta from this statement.	3
17	2	509	Moderate flows also showed habitat in the South Bay. Add South Bay to this sentence about Moderate flows.	3
19	2	520	Did Levi report the work by Malte Willmes on Oxygen isotopes? It shows the use of more saline habitat and should added as a citation for this section if allowable.	3
19	2	524	Cite this statement or include a graph showing this temporal relationship.	3
19	2	525	Add Merz et al (2013) to this statement regarding the distribution throughout the estuary. Merz, J. E., Bergman, P. S., Melgo, J. F., & Hamilton, S. (2013). Longfin smelt: spatial dynamics and ontogeny in the San Francisco Estuary, California. California Fish and Game, 99(3), 122-148.	3
19	2	532	The study by Grimaldo et al may not be definitive but it does support the results from Quinn et al 2012. It could be suggested that the day night patterns from Lake Washington may also be evident in the SFE. Add the Quinn et al (2012) citation and the discussion as to how Grimaldo et al supports those findings.	3
19	2	541	How was it determined that LFS "more frequently" inhabit deeper water. It appears that it is not more frequent in the cited paper. Is this perhaps being considered in a relative manner? If so than add 'relatively" before "more frequently".	3
19	2	543	The statement about 'downstream shift' suggests there is a change in distribution over time but then adds that it was in contrast to what was "previously thought". So which is it? Was there a downstream shift or was it an incorrect assumption? Provide the evidence of this relationship if the former.	3
19	2	545	This citations seems to be referencing an analysis but the citation is just data. Where is the analysis that shows this result.? This information is from the Prop 1 study I conducted with USGS and ICF. We did not collect enough longfin smelt to make definitive statements. It is unclear how the conclusions listed in the paragraph were reached given the low numbers of longfin smelt collected. Provide the analysis and cite the report instead. Young, M., Feyrer, F., Valentine, D. 2020. Factors Contributing to the Fine Scale Distribution of Fishes in San Francisco Estuary. Prop 1 Final Report. CDFW,	3
21	2	564	It is likely that work from UC Davis can be used to help inform these statements that are attributed to Baxter 2011a, personal communication. Refer to the Lewis et al 2019 report previously provided when appropriate to meet the 'best 'available' standard.	3

Page	Chapter	Line Number	Comment	Reviewer
24	2	613	Are the slides available for review? What are the criteria for inclusion into this document? Does it include presentations that are over 10 years old? Is there no report that can be referenced? As one of the objectives of this SSA is to be a reference for other documents its references must be accessible and up to date. Delete Slater 2009, slide 45 and consider including the reports by Jungbluth et al as part of their reporting for Prop 1. Reach out to Hildie Spautz or Wim Kimmerer. Also Junbbluth et al 2020 is publicly available. Jungbluth, M. J., Burns, J., Grimaldo, L., Slaughter, A., Katla, A., & Kimmerer, W. (2020). Feeding habits and novel prey of larval fishes in the northern San Francisco Estuary. bioRxiv.	3
24	2	622	Slater 2009 is not publicly accessible. Need to provide a citation that is or do the analysis with the data from Slater in this report.	3
24	2	632	How was 'effectively' determined? It was noted earlier that a significant portion of the larval smelt move out of the coverage area of the Smelt Larva Survey. Can this still be considered "effectively" capturing longfin smelt? It would be more accurate to state that the surveys cover the distribution of the species on a macro scale across the SFE and covers the temporal distribution from a biweekly to monthly scale.	3
24	2	641	The geographic limitation is noted in Grimaldo et al (2017) and further explored in Grimaldo et al 2020. These can be cited.	3
26	2	692	What is the size range that this survey is able to sample for Longfin smelt?	3
24	2	632	There seems to be a gap in the size covered by the surveys from 30-40 mm. Which survey was used to cover that size range? Was it the Summer Townet? Is the Summer townet not relied on for longfin smelt status and trends?	3
26	2	720	To know if 9oC was optimal then the results of less than 9oC would have to be tested as well otherwise remove the inference of it being an optimal temperature.	3
29	2	827	As part of the review all papers that are not published should be made available for review such as Nguyen et al 2021, in prep.	3
29	2	850	Should add the results and findings by Tamburello et al 2016 as it directly relates to this relationship and found that the relationship is getting weaker despite having a similar slope. This may be important to note so that anyone using this document can manage expectations as the uncertainty in the response due to a management action may have more error than these previous studies describe. Tamburello, N., Connors, B. M., Fullerton, D., & Phillis, C. C. (2019). Durability of environment–recruitment relationships in aquatic ecosystems: insights from long-term monitoring in a highly modified estuary and implications for management. Limnology and Oceanography, 64(S1), S223-S239.	3
30	2	870	The effect of including the step decline to account for the clam invasion was also evaluated by Tamburello et al (2019). Their conclusions should be included here as it supports the statement, updates the analysis, and test whether the step change should be included.	3
30	2	874	Recommend reviewing Tamburello et al (2019) as it seems to provide a lot of support and is the best available science in several of the cases in this paragraph. The change they note seems to be after 2005 where they found that the relationship with spring X2 becomes increasingly less reliable.	3
30	2	899	Should add "in the Delta" right after "longfin smelt" to make sure that it is clear which distribution of the LFS is being discussed. Or are you implying all of the LFS throughout their range is "vulnerable" to entrainment?	3
30	2	918	Flow was tested in Nobriga and Rosenfield 2016 and did not support this statement about flow improving survival from larvae to juvenile. The mechanisms are likely much more complex or something else entirely. The discussion on this point in the paper should be included here as this statement is not supported. Provide citations supporting the flow/food/survival relationship.	3
30	2	921	Unfortunately the conclusions by Sommer et al 2004 have not been shown to occur when artificially done using flow actions from agriculture return water. This statement about floodplain and production transport from wet years oversimplifies this relationship giving the false assumption that it would always work that simply. It would be best to inform any future manager relying on this document to know that there are limitations to this relationship as it appears that not all flows are the same. Cite the reports on North Delta Foodweb Action. Also there is the paper by Frantzich et al (2021) Frantzich, J., Davis, B. E., MacWilliams, M., Bever, A., & Sommer, T. (2021). Use of a Managed Flow Pulse as Food Web Support for Estuarine Habitat. San Francisco Estuary and Watershed Science, 19(3).	3
30	2	934	Why only wet years? This statement is ignore the recent studies showing plenty of larvae caught in drier years under two different efforts by Lewis et al (2019) and Grimaldo et al (2020). I am confused why this keeps getting left out. Do the larvae found during dryer years not need prey items?	3
30	2	948	Baxter et al 2007 and Hobbs et al 2006 do not test for sense of smelt but hypothesize what would the mechanisms for foraging for prey. Perhaps you are thinking of another study. Cite that study instead otherwise remove the citations and reword this statement to suggest it as a hypothesis.	3
30	2	951	Steinke et al 2021 is a data report not an analysis. Include the analysis showing this from this data. Young et al (in process) is a draft manuscript based on the final report analyzing this data. It will be shared. Young, M., Feyrer, F., Valentine, D. 2020. Factors Contributing to the Fine Scale Distribution of Fishes in San Francisco Estuary. Prop 1 Final Report. CDFW,	3
30	2	951	Refer to Bever et al 2018 regarding Delta outflow and turbidity in the San Francisco Estuary. During the Fall period turbidity is mostly governed by wind. Therefore Delta outflow would not necessarily improve turbidity in the Fall. Bever, A. J., MacWilliams, M. L., & Fullerton, D. K. (2018). Influence of an observed decadal decline in wind speed on turbidity in the San Francisco Estuary. Estuaries and Coasts, 41(7), 1943-1967.	3
32	2	956	The "dilution solution" has been long abandoned as a viable action for addressing contaminants. Delete this very outdated relationship. It grossly ignores the many issues that would not result in a reduction of toxicity. For example, higher flow increase mobilization of contaminants (Greenfield and Davis 2004). Dilution of contaminants also does not necessarily reduce toxicity as low concentrations of pyrethroids resulted in endocrine disruption that could lead to multigenerational impacts (Brander et al 2016a and 2016b). Brander, S. M., Gabler, M. K., Fowler, N. L., Connon, R. E., & Schlenk, D. (2016a). Pyrethroid pesticides as endocrine disruptors: molecular mechanisms in vertebrates with a focus on fishes. Environmental science & technology, 50(17), 8977-8992. Brander, S. M., Jeffries, K. M., Cole, B. J., DeCourten, B. M., White, J. W., Hasenbein, S., ... & Connon, R. E. (2016b). Transcriptomic changes underlie altered egg protein production and reduced fecundity in an estuarine model fish exposed to bifenthrin. Aquatic Toxicology, 174, 247-260. Greenfield, B. K., & Davis, J. A. (2004). A simple mass balance model for PAH fate in the San Francisco Estuary. Oakland, USA: San Francisco Estuary Institute.	3
32	2	961	Delete the last statement in the paragraph as it is usually false. As stated previously increasing outflow will increase loading of contaminants. The mass balance is a lot more complicated than is being described. Dilution would be possible if the loading was insufficient to match the increase in volume or loss of the contaminant. Neither of which is certain.	3
32	2	966	Do you mean Delta outflow or outflow in general? It seems Delta outflow because only Suisun and San Pablo are discussed. It would be best to be explicit to make sure there is no confusion. The South Bay sees very little impact from Delta outflow but High outflow from its watershed may have significant impacts on its localized LSZ.	3
32	2	971	This statement only mentions Suisun Bay region why not the other regions such as San Francisco Bay. Local tributaries during wet years would also expand spawning habitat. Managing agencies will need to know that there are local interactions that can be done to contribute to the habitat and not just Delta Outflow. Otherwise those same agencies can continue to move to zero freshwater outflow as they improve their recycling of their treated waste and stormwater. The suggested expectation is that freshwater from these tributaries don't matter as long as Delta outflow is supported. Maunder et al (2015) found that this was not the case.	3
32	2	976	This section on Extended spawning season seems to be ignoring the significant inputs from the San Francisco Tributaries. Add a few lines acknowledging their contribution and how high outflow would improve the spawning season.	3
33	2	989	X2 is being conflated with the low salinity zone. As a metric it is the measurement up the axis of the estuary into the Delta while the LSZ can be found throughout the San Francisco Bay into the tributaries as well as the Delta and is more strongly associated with spawning locations. Locations in the tributaries in San Pablo and South Bay could hardly be considered associated with X2.	3
33	2	1003	Lewis et al (2019) does not support this statement that spawning is limited to the Delta and Suisun. That report as well as Grimaldo et al (2020) found newly hatched larvae in San Francisco Bay during the dry year of their sampling. This sentence should be deleted as it is false.	3
33	2	1007	This analysis contains errors and this discussion does not report the significant uncertainty in these predictions. In an analysis by Phillis et al (2019) the effect of outflow was too small to detected given the uncertainty.	3
33	2	1015	The mechanism "avoid toxicity" is not addressed above in the document. The suggested mechanism is to reduce contaminant exposure. Toxicity is a given as the entire system is considered impaired as is noted in this document. Therefore it should changed to reduce toxicity. Also this is still a hypthesis as I mentioned mulitple times that contaminants do not merely dilute with flow. It is much more complicated than that.	3
34	2	1016	Why does this only focus on Delta outflow. These mechanism could be applied to Outflow in general as hypothetical mechanisms. Replace Delta outflow with General Outflow. Although then entrainment would not be included. Perhaps have two graphics or an asterisk by entrainment noting this is for Delta outflow mechanisms.	3
34	2	1023	In relation to the previous comments, replace X2 with the LSZ as it would allow for the graph to apply more generally to the all the habitat for longfin smelt SFE DPS. This is supposed to be a reference document to help inform managers that may impact LFS in all of their habitat not just in the Suisun Bay/ Delta.	3
35	2	1032	The loses to recruitment mentioned in this statement forgets about the local production lose as the habitat through out the San Francisco Bay is saltier therefore the spawning and nursey habitat would retreat into the tributaries and east Suisun Bay. This was mentioned in both Grimaldo et al 2017 and 2020.	3
35	2	1034	This statement about not quantifying the relative importance is not entirely correct. Maunder et al 2015 quantified some of these mechanisms. Also some flow relationships with the individual factors have been explored such as with turbidity and prey.	3
35	2	1039	There is an accidental inclusion of an editorial note to "Cite to a Fig in the SSA that show this". Make a correction.	3
35	2	1041	The correlation with Delta outflow still requires more evaluation as was just noted in the paragraph the citation of Maunder et al 2015 that was provided in the previous comment actually suggests that flows from the Delta are interchangeable with Napa river outflow which would invalidate this statement. Cite to Nobriga and Rosenfield 2018 to provide some counter points to Maunder et al 2015 and discuss these relationships as an ongoing form of study in regards to mechanistic relationships.	3

Page	Chapter	Line Number	Comment	Reviewer
35	2	1039	High outflow years are actually characterizing wetter years. These years have inherent properties such as what was mentioned previously in the document about temperature that is not replicated with outflow manipulation alone. Change the term High outflow years to wet years as it is more accurate and would not give the false impression that such relationships can be replicated with just outflow.	3
35	3	1068	The Maunder et al (2015) model tested this and did not find that the Bay Area Tributaries were to a lesser extent related to Longfin smelt abundance. This statement needs a citation but there also needs to be a discussion regarding the Maunder et al (2015) model. otherwise this is not the best available science. Also as the relationship between flow and abundance are indirect effects at best Delta outflow is a tertiary stressor. Reduced flow is not a stressor in the traditional sense. The effects attributed to flow are stressors on Longfin smelt. Instead relabel 3.1.1 as Stressors related to reduced freshwater flow.	3
36	3	1073	There seems to be something missing here. Would not any flow event regardless of severity be related to the "natural hydrologic cycle" or the be related to the inflow/unimpaired flow relationship? What is being said here?	3
37	3	1122	The findings from the analysis attributed to Rosenfield and Lewis (2021) is actually in contrast to the source of the analysis by Phillis (2019). This needs further exploration as the primary issue that Phillis (2019) was pointing out was that any analysis that was being done on this data was highly uncertain and that the signal was smaller than the noise. The "greater than 10%" part of the statement exaggerates the precision of the modeling. Reporting this information like this is not the best available science as it ignores the uncertainty that is even evident in the figure cited. Any reference to these "findings" such as the 20% decline is most likely spurious. The Rosenfield and Lewis (2021) analysis completely miss what is essentially an evaluation of the precision of the flow relationship as was discussed by Phillis (2019). The Phillis (2019) conclusions should be included as part of the discussion. Recommend this paragraph be deleted as it not best available science as there are better analyses like Kimmerer 2002 and Nobriga and Rosenfield 2018.	3
38	3	1152	Remember, Longfin smelt can spawn throughout the estuary fresh to low salinity regions of the estuary not just the Delta. Need to include what was happening to the rest of the population in the other locations. Add that what was occurring in the Delta was likely also occurring in SF Bay and Suisun Bay.	3
38	3	1154	The pattern in the figure from the Eakin (2021) analysis does not show the catch being "consistent" prior to 2014. It wildly fluctuates. But then is less so after 2014. Is that what is being described. Perhaps change the term "consistently" to "highly dynamic " or something like that. perhaps reword to be more accurate to what is being depicted in the figure. It shows that there was relatively more fish and the catch data was relatively more dynamic between 2009-2014 vs 2015-2019. This finding is very interesting since the earlier period has one wet year and the rest being dry while the later period had two wet years with 2017 (the wettest) having some of the lowest catch data.	3
39	3	1170	This paragraph has a lot of speculation. It would be good to show examples of how multiple years of poor recruitment would impact a short lived species. This would strengthen the discussion on this point and be less speculative.	3
39	3	1170	The increase in frequency is relying on a comparison of unequal sample size. Therefore this should be carefully discussed as this could lead to spurious conclusions. Also there weather predictions that have been published regarding climate change that could be relied on to provide a more robust discussion on this point. Cite and discuss work like those from Swain et al (2018), Diffenbaugh et al (2015), or Mallakpour et al (2018). Although these papers seem to suggest that it is not an increase in frequency as much as an increase in extremes. Swain, D. L., Langenbrunner, B., Neelin, J. D., & Hall, A. (2018). Increasing precipitation volatility in twenty-first-century California. Nature Climate Change, 8(5), 427-433. Diffenbaugh, N. S., Swain, D. L., & Touma, D. (2015). Anthropogenic warming has increased drought risk in California. Proceedings of the National Academy of Sciences, 112(13), 3931-3936. Mallakpour, I., Sadegh, M., & AghaKouchak, A. (2018). A new normal for streamflow in California in a warming climate: Wetter wet seasons and drier dry seasons. Journal of hydrology, 567, 203-211.	3
40	3	1189	There have been periods of long droughts or at least long periods between wet years in the recent historical record. This paragraph seems to ignore that. What about those prior periods. There needs to be some discussion about why this most recent drought period in 2012-2016 was severe enough to suggest that Longfin smelt do not have the resiliency compared to those prior periods. There was a drought in the late 80s early 90s and there was a long period between wet years as well as the worst drought in the recent historical record in the 70s.	3
40	3	1202	When writing "studies of" it would be required to at least cite some of them. Suggest Kimmerer 2008 (since the statement is generic about entrainment risk) and Grimaldo et al 2009 since this SSA is about longfin smelt.	3
40	3	1197	The impression that only X2 need be managed is suggested heavily in this paragraph. This oversimplifies the inherent properties of a wet or above normal year to just a simple factor that does not actually capture those benefits. Temperature, local precipitation among other factors should also be discussed. To be more accurate, add to any mention of X2 that it was "due to wetter conditions".	3
40	3	1210	As both Dege and Brown (2004) and Baxter et al (2010) used data that contains significant spatial bias the term 'strongly' should be deleted.	3
40	3	1212	The association of larval distribution to X2 ignores the distributions in the south Bay and San Pablo. Replace X2 with LSZ to be more accurate and the statement being more transferable to the spatial context.	3
40	3	1215	Knowles 2002 is not in the literature cited section. Is it this "Knowles, N. (2002). Natural and management influences on freshwater inflows and salinity in the San Francisco Estuary at monthly to interannual scales. Water Resources Research, 38(12), 25-1". Hutton et al (2017a, 2017b, 2019) actually updates this information. Hutton, P. H., Rath, J. S., & Roy, S. B. (2017). Freshwater flow to the San Francisco Bay-Delta estuary over nine decades (Part 1): Trend evaluation. Hydrological Processes, 31(14), 2500-2515. Hutton, P. H., Rath, J. S., & Roy, S. B. (2017). Freshwater flow to the San Francisco Bay-Delta estuary over nine decades (Part 2): Change attribution. Hydrological Processes, 31(14), 2516-2529. Hutton, P. H., Chen, L., Rath, J. S., & Roy, S. B. (2019). Tidally-averaged flows in the interior Sacramento–San Joaquin River Delta: Trends and change attribution. Hydrological Processes, 33(2), 230-243.	3
40	3	1216	Cloern and Jassby (2011) aggregate too much of the data and obscure the dynamics in that part of the year. Better to use Hutton et al (2019) for a more accurate and robust analysis. Hutton, P. H., Chen, L., Rath, J. S., & Roy, S. B. (2019). Tidally-averaged flows in the interior Sacramento–San Joaquin River Delta: Trends and change attribution. Hydrological Processes, 33(2), 230-243.	3
41	3	1222	Maunder et al (2015) tested the importance of flow from the Delta and did not find it more important than Napa river flows suggesting it was actually the wetter year conditions over the whole habitat that was important.	3
41	3	1224	An analysis by Phillis (2019) did not find that freshwater flow was not the primary factor contributing to the decline as despite increases in flows the response of Longfin smelt was less and less. This was supports the work from Tamburello et al (2019) showing that the relationship strength was weakening. As well as Nobriga and Rosenfield 2016 that showed that juvenile to adult survival was not significantly affected by Delta outflow. Tamburello, N., Connors, B. M., Fullerton, D., & Phillis, C. C. (2019). Durability of environment–recruitment relationships in aquatic ecosystems: insights from long-term monitoring in a highly modified estuary and implications for management. Limnology and Oceanography, 64(S1), S223-S239.	3
41	3	1225	The lines of evidence regarding whether it is Delta outflow or Wetter year conditions should be better evaluated than this. The narrative in this SSA is not robust regarding the flow relationship. Correlation is not causation. Mechanisms must be explored and discussed in this section. You cant just pour water on a LFS and it will grow, there are considerations with transport, habitat expansion, etc. Perhaps at the beginning of this section it is made clear that it will only cover the non-mechanistic relationships as that will be explored later in the document.	3
41	3	1238	Where is this coming from? This SSA literally cites Lewis et al 2019, 2020, and Grimaldo 2020 showing spawning in locations outside of Suisun Bay and Delta during dry years. Add that the spawning habitat is restricted in the SF Bay, Suisun Bay, and Delta.	3
41	3	1240	The statement about the oil spill is grossly inaccurate as there is spawning in dry years outside of the Delta. Delete this statement or add nuance to the scenario. There are oil trains going over the Yolo Bypass all the time and there was an oil spill in Suisun Marsh in 2004 so the the danger if this statement were true than why are there no safeguards for those oil trains in case of an emergency. In 2017 there was a train derailment near Stockton during the storms. If that had occurred over the Yolo Bypass and also been one of the oil trains that would have been catastrophic.	3
41	3	1250	For larval diets you could cite Jungbluth et al (2020) and/or Barros et al (2020). Jungbluth, M. J., Burns, J., Grimaldo, L., Slaughter, A., Katla, A., & Kimmerer, W. (2020). Feeding habits and novel prey of larval fishes in the northern San Francisco Estuary. bioRxiv. Barros, A. (2020). Geographic variation in feeding success, prey availability, and prey selectivity of Age-0 Longfin Smelt in the San Francisco Estuary (Doctoral dissertation, University of California, Davis).	3
42	3	1293	Why use unpublished data? Is there no report for this? Why is this report relying so much on references that are not publicly available? How is this document supposed to be a resource?	3
43	3	1319	When stating "other authors" they should be cited. MacNally et al (2010) does not evaluate the relationship between flow and copepods or mysids. Include those citations that do.	3
44	3	1330	This is where it would be good to add the discussion that the results from Maunder et al (2015) and Tramburello et al (2018) suggest about the flow relationship. This would result in a transparent and improved discussion. Ignore their findings only highlights them for those who know they exist and provide a vulnerability to the conceptual model or any management actions developed from the current narrative in this document.	3
44	3	1332	Although Johnson et al 2010 states that it cannot conclusive determine what the toxicity is occurring it is clear in that there is toxicity occurring just not the extent or severity. The statement is misleading in that it seems to call into question if there is any toxicity. This document suffers from the survivors fallacy where the only fish that could be evaluated were those that survived therefore their is a likely under reporting of toxicity for the fish.	3

Page	Chapter	Line Number	Comment	Reviewer
44	3	1333	This paragraph would benefit from the discussion from Brooks et al (2012) and Fong et al (2016). The latter actually found that contaminants may have ad a significant impact on Longfin smelt. Brooks, M. L., Fleishman, E., Brown, L. R., Lehman, P. W., Werner, I., Scholz, N., ... & Dugdale, R. (2012). Life histories, salinity zones, and sublethal contributions of contaminants to pelagic fish declines illustrated with a case study of San Francisco Estuary, California, USA. Estuaries and Coasts, 35(2), 603-621. Fong, S., Louie, S., Werner, I., Davis, J., & Connon, R. E. (2016). Contaminant effects on California Bay–Delta species and human health. San Francisco Estuary and Watershed Science, 14(4).	3
44	3	1343	There should be a discussion on Weston et al 2014. In the study there was signficicant impacts on amphipods by stormwater. This could potentially impact longfin smelt juveniles throughout its range as the toxicity was not only detected in Cache Slough Complex but in Suisun Marsh (Weston et al 2015). There are many other watersheds feeding into the habitats of Longfin smelt that have not been evaluated. Weston, D. P., Asbell, A. M., Lesmeister, S. A., Teh, S. J., & Lydy, M. J. (2014). Urban and agricultural pesticide inputs to a critical habitat for the threatened delta smelt (Hypomesus transpacificus). Environmental toxicology and chemistry, 33(4), 920-929. Weston, D. P., Chen, D., & Lydy, M. J. (2015). Stormwater-related transport of the insecticides bifenthrin, fipronil, imidacloprid, and chlorpyrifos into a tidal wetland, San Francisco Bay, California. Science of the Total Environment, 527, 18-25.	3
44	3	1359	This section has almost no citations. There are a lot of statements in this section and citations would only strengthen those statements. Otherwise they appear to be hypotheses.	3
46	3	1432	There are a couple of studies on silversides (a fresh to estuarine species) that examine whether they are predators of larval fish and whether predation may be an issue for Delta smelt (Schreier et al 2015, Hamilton and Murphy 2018). These studies can be used to discuss the potential of predation on LFS. Silversides may be increasing in abundance (Mahardja et al 2016). As for marine, there is evidence of predation by some predators from Marine environments. Szoboszlai et al (2015) supplemental materials data has several incidences of Longfin smelt detections in birds and a detection in a mammal. Szoboszlai, A. I., Thayer, J. A., Wood, S. A., Sydeman, W. J., & Koehn, L. E. (2015). Forage species in predator diets: synthesis of data from the California Current. Ecological Informatics, 29, 45-56. Schreier, B. M., Baerwald, M. R., Conrad, J. L., Schumer, G., & May, B. (2016). Examination of predation on early life stage Delta Smelt in the San Francisco estuary using DNA diet analysis. Transactions of the American Fisheries Society, 145(4), 723-733. With the spatial and temporal overlap is very possible that silversides also prey on Longfin smelt. Although Maunder et al (2015) did not find much support for their predator variable. Hamilton, S. A., & Murphy, D. D. (2018). Analysis of limiting factors across the life cycle of delta smelt (Hypomesus transpacificus). Environmental management, 62(2), 365-382. Mahardja, B., Conrad, J. L., Lusher, L., & Schreier, B. (2016). Abundance Trends, Distribution, and Habitat Associations of the Invasive Mississippi Silverside (Menidia audens) in the Sacramento–San Joaquin Delta, California, USA. San Francisco Estuary and Watershed Science, 14(1).	3
47	3	1483	Cite precedence for increased vulnerability to predation when a species reaches low relative abundance. It would be important as it will stress that this is not just a hypothesis.	3
47	3	1487	Like the many times before this, when referencing some vague study or data, you must provide the citation. The "empirical information" needs to be cited. Is it the Nobriga and Rosenfield (2016). Make it clearer that it is.	3
48	3	1494	The diversion/water clarity relationship is overly simplified here. Anyone relying on this document to inform their management needs would be making an spurious conclusion that sediment loading, tidal mixing, and wind may play no role. Add "among other factors" after water diversions.	3
49	3	1534	Inland Silversides are a model toxicology species. Not sure how you would reword this sentence. Remove Silversides from the parentheses and add it to when "model" species are discussed later in the sentence. It is important to note that Fong et al (2016) also found a significant correlation with pyrethroid use data and abundance index.	3
49	3	1539	This paragraph needs more citations. Brooks et al (2012) and Fong et al (2016) are good to reference in this section. Also there are several USGS documents that show recent contaminant profiles in 2015-2016 (Jabusch et al 2018) and 2016-2017 (De Parsia et al 2019) Jabusch, T., Trowbridge, P., Heberger, M., Orlando, J., De Parsia, M., & Stillway, M. (2018). Delta Regional Monitoring Program Annual Monitoring Report for Fiscal Year 2015–16: Pesticides and Toxicity. Richmond, CA: Aquatic Science Center. http://www. sfei. org/documents/delta-pesticides-2016. De Parsia, M., Woodward, E. E., Orlando, J. L., & Hladik, M. L. (2019). Pesticide Mixtures in the Sacramento–San Joaquin Delta, 2016–17: Results from Year 2 of the Delta Regional Monitoring Program (No. 1120). US Geological Survey.	3
49	3	1593	A review of contaminant effects may be too extensive for this documents therefore it may be better to not focus so much detail on ammonia and selenium and instead concentrate on general contaminants and their effects on the species and its environment. It would be useful for a reference document to give good references that can help a manager know what to look for. It should be clear that both direct and indirect effects are a concern. Also what are the routes of exposure and the contaminants of concern. The latter is mentioned but for the most part only ammonia is discussed even though other nutrients may be an issue and there are so many other classifications of contaminants that should be discussed at least on a trait effect level such as endocrine disruption or neurotoxicity. The last paragraph in this section just lists these trait toxicities but there should have been context as whether such toxicities have been detected. Cite Fong et al (2016) on this point.	3
48	3	1504	This section has a lot of great up to date information and takes into account a lot of the nuance of contaimants. As a reference docuemnt thought there can be more provided such the dynamics of contaminant loadins, the issue with stormrunoff, regional imputs like rivers and sloughs as well as fate and transport of contaminants.	3
51	3	1634	Dr. Ron Kneib did an independent review of the Biological Opinion (2019) and found the discussion on agricultural diversions to be misleading and the interpretation of Nobriga et al 2004 was much more nuanced.	3
51	3	1636	Barker Slough diverts from Barker slough not Suisun Marsh.	3
51	3	1646	Provide citation.	3
52	3	1668	Cite an analysis that shows this.	3
52	2	1684	The term 'entrained' is being conflated for being entrained at the facility. Entrained into the south Delta is different. The distiction should be made especially since entrainment can occur in may other areas in the estuary as was already noted in previous sections. As a reference document it should be clear what entrainment is being discussed. it later noted that the definition is that 'entrained' fish are drawn into the louvres of the salvage facilities. this is conflated with the different definition from Fujimura 2009.	3
53	3	1714	The salvage data needs more context in the reporting. It is noted that true entrainment is likely higher later in the paragraph but this should be reported here before the numbers for salvage. Likewise the proportion of that salvage the population should be reported as well. Otherwise there is likely to be a mistaken inference as was done with the Sacramento Splittail when proportion was not accounted for and the species was found to be have salvage related to population dynamics as opposed to an impact by the water projects. This led to a costly listing and then de-listing effort.	3
53	3	1718	Fujimura (2009) is not validated with field observations. Therefore the modelled results in the paper should be coupled with the study on Delta smelt Castillo et al (2012) to provide more support of the estimates of survival. Although the Castillo et al (2012) study only has an estimate estimate for adults. The juvenile estimate was only one release and would not be much of an improvement from what Fujimora (2009) reported.	3
54	3	1745	As noted earlier, the longfin smelt may congregate around the LSZ but it does not always mean X2. You must add that there are other areas of LSZ in the estuary otherwise there will be confusion a to what areas of the estuary are important. This would greatly impair a manager by limiting their context of examining the rest of the SF Estuary and not just the west/east access into the Delta.	3
54	3	1749	Add "into the water projects" after "entrained" to make sure the reader knows what form of entrainment is being discussed.	3
54	2	1764	This analysis was updated in the 2019 BA and included in the 2019 BiOp in part. Why is it being ignored. The % were an order of magnitude smaller than this estimate from the 2009 analysis. Contradicting the 2020 ITP may not be a good idea at this point. Also the estimates in the 2009 analysis by CDFW failed to account for the significant sampling bias from the larval survey. This was noted in Grimaldo et al (2017). This whole paragraph should be deleted or updated with the analysis in the 2020 ITP.	3
55	3	1773	This previous stressors did not include dredging in which there is an actual BMP for protecting LFS. So that should at least be mentioned. Also an assessment of in water constructions has come up for at least two major construction issues, the bay bridge and the Delta tunnels. A brief discussion on those efforts and how they tried to reduce impacts from construction should be listed. Construction on wetlands are frequently required to evaluate their impact on longfin smelt. This would also constitute a stressor.	3
55	3	1781	Need to include why the SLS is not included. It was designed for Longfin smelt.	3
55		1788	As noted by Latour (2016a and b) The index is not a good way to use the catch data. There were several methods that were suggested for Delta smelt and would likely be appropriate for Longfin smelt. Latour, R. J. (2016a). Explaining patterns of pelagic fish abundance in the Sacramento-San Joaquin Delta. Estuaries and Coasts, 39(1), 233-247. Latour, R. 2016b. Statistical modeling delta smelt (Hypomesus transpacificus) survey data in the San Francisco-San Joaquin Delta, with reference to temporal and spatial autocorrelation. CAMT Final Report.	3

Page	Chapter	Line Number	Comment	Reviewer
55	3	1791	Although the declines are occurring, this analysis has an obvious temporal bias. Make sure to present the analysis how the temporal bias was handled so that when reviewed by others that it will not be too easily dismissed as biased. The recent period is dry when catch is generally low while the beginning is during a wet period when catch is generally high. Perhaps look at only dry or only wet years to show how even when it is grouped among like years the declines are still evident. Also don't use the index if possible as it does not have an error term.	3
56	3	1796	The FMWT is not effective (Merz et al 2013) at collecting Longfin smelt as it there is an obvious bias in the geographic coverage as well as the benthic coverage. The latter of which was identified by Nobriga and Rosenfield 2018 therefore the Bay study was used as well. Merz, J. E., Bergman, P. S., Melgo, J. F., & Hamilton, S. (2013). Longfin smelt: spatial dynamics and ontogeny in the San Francisco Estuary, California. California Fish and Game, 99(3), 122-148.	3
56	3	1808	Reference mistake. The figure being referenced is incorrect. It should be Figure 3.8.	3
56	3	1803	The declines being noted need to account for the temporal bias as was noted previously with similar recommendations for this analysis as well. Also it is dissipointing to see that the data is truncated for no reason giving the impression of hiding the analysis to support the conclusions. Please provide the whole data set for the analysis and be more transparent on the analysis. Otherwise this section is a vulnerability in the document as it is clearly biased. Figure 3.10 does a better job of illustrating the declining abundance of longfin smelt although it still relies on the index.	3
58	3	1834	When introducing a method, it is very important to cite how to do it and the precedence for using it. Otherwise its application would be considered arbitrary.	3
59	3	1849	As noted previously, using the index is not a good way to utilize the data from the surveys. Robert Latour, an expert on survey design and using survey data stated so in both his paper (Latour 2016a) and report to CAMT (Latour 2016b). Redo the analysis with the catch data instead.	3
60	3	1879	Why 2009-2008? Why was the data truncated? Provide the reason otherwise the data range is arbitrary.	3
62	3	1941	It was noted that the high variability detected in the analysis suggests there may be a increase in extinction probability "regardless of their initial population size". Why not then also recommend actions to reduce that variability. It would seem anything else ignores this significant issue of increased extinction risk with highly variable population.	3
61	3	1917	This Discussion insufficiently covers the results from the analysis. It does not put any of the results in the context of the assumptions made and address the potential for spurious conclusions. It also does not address the issues with geographic bias in the 20 mm and FMWT or the result from the OT showing an increase in growth. How is the latter explained?	3
62	3	1953	The analysis cited shows that there is some genetic similarities between the SFB DPS and the those further north, how then can it be stated that they are isolated. There is small geneflow occurring. This suggest they are not isolated but that geneflow is likely severely limited. If making a listing decision on this document it must be understood that geneflow was not found to be zero. Otherwise anyone relying on this document to inform a listing decision will have a fundamental flaw that was not highlighted enough for addressing.	3
63	3	1965	The conclusion that Freshwater flow is the most "important species need" is flawed as the document does not contrast the factor with habitat loss. Add the context of habitat loss into this conclusion. Flow was not found to be limiting the survival from juvenile to adults (Nobriga and Rosenfield 2018) and that the species is not likely to avoid extinction with flow alone (Phillis 2019). There are other limiting factors that are clearly very important. Remove the distinction of "most" important and replace it with "very".	3
63	3	1971	There is no support cited for this statement in the early sections of this document. Provide the loss of resiliency and redundancy that was noted in this paragraph. The last statement is clearly false as is noted in Grimaldo et al 2020 and Lewis et al 2019.	3
63	3	1983	It is not established that Longfin smelt have weak trophic adaptability. It is implied but not stated clearly. Add that to the earlier section on prey to tie this statement together.	3
63	3	1986	Loss of tidal marsh quality should also be considered. Lessons from Blacklock and Calhoun Cut have been revealing that just 'implementing' habitat restoration is only part of the solution. Without understanding of the impacts from Invasive Aquatic Vegetation there is a lower probability of having restoration provide benefits to Longfin smelt	3
64	3	2012	The scenario of mass entrainment in this paragraph is pure speculation and should be removed unless there was an event that can be cited to support this claim. Suggest softening the language such as change 'when' to 'if'.	3
65	3	2017	The table on line 2017 seems to be accidentally included as it has no details and is does not appear to be referenced in the text. Remove the table otherwise if inclusion was not a mistake then the table needs a title and description. It also needs a narrative in the text to describe what it is, how the risks were quantified, and what it is for.	3
65	3	2024	The impacts of the water infrastructure implementations prior to the 40s is speculative as it assigns all of the attribution of the historic declines to that event. This is unlikely as is stated previously the "cumulative negative changes" have likely brought the species to its current status. Delete the sentence about infrastructure as it is too speculative when the other parts of the analysis are far more robust and could be better examples to highlight.	3
65	3	2030	The conclusion that the three Rs are low is not readily apparent in the narrative leading this statement. Suggest providing a table listing the lines of evidence that related to any one of the three Rs. Also the three Rs are barely discussed in context in Chapter 3. Go over Chapter 3 and make sure to remind the researcher/manager reviewing this document that the three Rs are a core evaluation structure for how to use the information. Provide these details at the end of each stressor section or emphasize that more in the final section of Chapter 3. The weight of evidence needs to be clearly listed and discussed so the final statement can be regarded as logical and robust.	3
66	4	2051	The figure needs to add what the shaded area is supposed to mean in the title/description. Figures should be able to be reviewed without having to read all of the narrative. It is stated in the proceeding page that the shaded areas are what is being covered by Chapter 4. Then just add that statement into the title.	3
69	4	2167	Suisun Bay and the Delta are discussed but what about the SF Bay. Why not include that in this assessment as the title of this subsection is supposed to include this region as well. Much of what is described in this section could be more generalized to work the whole estuary. Calling out specifics for the Delta only highlights the bias in this narrative when considering climate change impacts for the estuary as a whole. Suggest adding more specifics for the SF Bay and its tributaries or be more general to have the statements apply to the estuary as a whole. If there is a need to be point out a disparity between regions then explicitly state it. Salinity intrusion into the estuary will also impact distribution of the LSZ in the tributaries of the SF Bay not just the Delta.	3
71	4	2237	Include the projections for the SF Bay in these subsections. It is odd that there is little consideration for this region. Is SF Bay not a critical habitat? Do LFS not use the SF Bay? The species obviously does and climate change will impact that region as well. The local managers and agencies relying on this document may get the impression that the SF Bay is not important or that for some reason it will escape climate change impacts. The reason for exclusion is not sufficient. Tidal influence is not a sufficient reason to exclude the analysis regarding temperature change.	3
71	4	2245	Grimaldo et al (2017) does not support this statement on page 8. The authors actually pointed out there was likely a bias in the larval distribution based on the surveys. They proceeded with the analysis to provide their findings but with the context that the bias was still there. Later surveys actually shows significant spawning in the SF Bay (Grimaldo et al 2020, Lewis et al 2019).	3
71	4	2250	The lack of mobility makes sense but not how it has been discussed in prior sections. The larvae were noted as passive bouyant particles yet such particles would likely be easily transported down stream out to cooler temperatures. Projections by Gross et al (2021) found that using buoyant particles would result in most of the fish leaving the system for the ocean. Some other mechanism for retention must be also occurring. Gravitational circulation was not sufficient to explain retention in the model. Then how do you rectify this discrepancy? If the fish are buoyant and passive how do they not move into the ocean if gravitational circulation does not explain it. Gross, E. S., Korman, J., Grimaldo, L. F., MacWilliams, M. L., Bever, A. J., & Smith, P. E. (2021). Modeling Delta Smelt Distribution for Hypothesized Swimming Behaviors. San Francisco Estuary and Watershed Science, 19(1).	3
65	4	2034	The fundamental flaws in Chapter 2 and 3 are perpetuated into Chapter 4. The whole section needs a fundamental rewrite to include the vast amount of missing information not covered in the prior section and the over expressed certainty in the conclusions from Chapter 2 and 3. Chapter 4 excludes much of the SF Bay in its projections for much of its analysis and contains out of date conceptual models. Multiple studies that would update the old conceptual models are not utilized and the information contained within this section would at best only contain part of the best available science. These flaws represent a vulnerability regarding the SSA as a support document for informing other regulatory or management decisions. Because of the need to fundamentally rewrite this chapter, the comments made in this section are much more limited under the assumption that this chapter will be changed to reflect the comments in the prior chapters.	3
73	4	2318	It has not clear that spawning in the Delta is essential to the population. Instead stating it is "essential" use the three Rs to illustrate why it is essential under the context of this SSA. For example how does spawning in the Delta fit into Resiliency, Representation, and Redundancy.	3
75	4	2336	Tobias and Baxter 2021 is now published.	3
75	4	2341	When citing a presentation it would be best to cite the report instead. Does not Miller (2021) have any report to accompany the presentation? Why are presentations that are not publicly available being cited?	3
76	4	2352	It is essential that any results be cited and that the report at the very least be accessible. Where is the report on the Longfin smelt Fecundity study?	3
85	4	2546	Why would the SF Bay not have suitable habitat? There is mounting evidence that it does possess suitable habitat. Is the assumption that the SF Bay is not habitat for larval Longfin smelt? This could result in no habitat restoration or management of current tidal habitat for the benefits of longfin smelt. Is the USFWS suggesting that it is not necessary to even consider this area for supporting Longfin smelt spawning and rearing? That is what is being suggested by the statement about the SF Bay not being 'likely' suitable habitat.	3
86	4	2586	Although there is a lot of evidence of the correlation between freshwater flow and abundance it cannot be said that it is the 'most' important species need. Physical habitat loss and the step change from the clam invasion could also be candidates. Remove the term 'most' from the sentence as the Current Conditions sections does not evaluate those factors quantitatively with flow to determine which is the 'most' important.	3
86	4	2602	Do not cite a slide from 2013. Not only is this likely out of date but there are likely much better resources that are published in the last 8+ years.	3
88	4	2635	This statement could be better supported if there was a viability or extinction risk analysis that incorporated the projected increases in consecutive dry years. This seems to be a big issue in this section. why not do that and add that as a quantified result for this SSA.	3
79	4	2394	This whole paragraph should include more citations as it is very speculative in nature. To show that it is not citations can help show how robust the conclusions may be. Use the citations already used in the previous chapters. Otherwise statements with 'likely' or 'potential' seem too speculative to be helpful for any manager or researcher relying on this document as a resource.	3

Page	Chapter	Line Number	Comment	Reviewer
79	4	2397	A resource or habitat manager in the SF Bay would ill served by this evaluation as there is little for them to know regarding what the climate change predictions will do to their region. The SF Bay is important spawning habitat. As was stated numerously the 20 mm and the SLS are spatially biased. They exclude habitats that would also be good spawning sites. What will the temperature predictions do to those cites?	3
80	4	2416	The statement about the capabilities of larvae to tolerate stress should be cited. The evaluations of Delta smelt did not find that larvae were the 'most vulnerable' how do you know that Longfin smelt are any different? Instead suggest that with the exception of Delta smelt, larvae tend to be more vulnerable to temperature changes.	3
80	4	2435	The pattern being noted hear was evaluated by one of the co-authors of this document. It would be good for you to cite it to support this statement. Tobias, V., & Baxter, R. (2021). Fewer and Farther between: Changes in the Timing of Longfin Smelt (Spirinchus thaleichthys) Movements in the San Francisco Estuary.	3
82	4	2479	Projected increases and decreases come with a lot of uncertainty and predictions are being continously updated. Recommend using more up to date citations and to also provide more citations for support. This will help if there are certain weaknessess in those individual analyses (which is likely) so that conclusions can be more robust and defensible.	3
82	4	2487	Teh adjectivews characterizing the dynamics of the predicted hydrology should be kept to a minimum as the error around those predictions are not provided. Statements with terms like 'substantial' or 'steep' may be descriptive but may not be accurate depending on the the uncertainty and the base frame or reference.	3
82	4	2498	The averaging of predictions confounds too much of the uncertainty to make precise estimates like '33%'. Suggest just stating that all the projections converge on increases in unimpaired flows.	3
84	4	2530	This use of the term productivity is inaccurate. It should be relegated to the increase in numbers from a previous state. As winter to spring period includes the larvae to juvenile survival it would not be accurate to say productivity as the juvenile state would be lower in abundance than the larvae state. Instead change 'productivity' to 'recruitment and survival'.	3
85	4	2537	Refer to the previous comment on the inaccurate use of the term 'productivity'. The use of the term makes this statement inaccurate unless there is additional discussion about how the year class would impact the productivity of the next years class.	3
90	4	2704	The information on tidal changes due to sea level rise needs to cited. Presumably this is all from the OPC (2018) document, then just cite it here as this is a new paragraph. This will help resource and ecosystem managers to know what to expect and what references to rely on for their needs. Also if need be to update those projections if need be.	3
91	4	2733	The marshes in the SF Bay are also important as they are relevant to the three Rs. Make sure to add them into this discussion.	3
92	4	2752	Citation for this statement on salinity intrusion. Gross, E. S., Hutton, P. H., & Draper, A. J. (2018). A Comparison of Outflow and Salt Intrusion in the Pre-Development and Contemporary San Francisco Estuary. San Francisco Estuary and Watershed Science, 16(3). Ghalambor, C. K., Grosholtz, E. D., Jeffries, K. M., Largier, J. K., McCormick, S. D., Sommer, T., ... & Whitehead, A. (2021). Ecological Effects of Climate-Driven Salinity Variation in the San Francisco Estuary: Can We Anticipate and Manage the Coming Changes?. San Francisco Estuary and Watershed Science, 19(2).	3
92	4	2740	Add the citation of DCS 2021 to the first sentence as well. Also should add the rest of the estuary to this discussion as LFS use all of it. Shirzaei, M., & Bürgmann, R. (2018). Global climate change and local land subsidence exacerbate inundation risk to the San Francisco Bay Area. Science advances, 4(3), eaap9234.	3
92	4	2762	This should also include diversion from throughout the SF Estuary including the SF Bay and the rest of the Delta. it only seems to focus on the water projects. There will be impacts to how in-Delta uses will use or divert the water such as the North Delta Aqueduct or Contra Costa.	3
93	4	2789	What will this mean for water supply? Will there be enough in the reservoir for maintaining salinity standards in the projections? Resource managers will need to know this. There is not an infinite amount of water and 4 or 7 km change on average is substantial. Is there even enough water to manage that change?	3
93	4	2796	This sentence is too generic and should specifically identify that X2 changes will impact spawning and rearing in the Delta and Suisun Bay but will have less impact in the SF Bay, especially the South Bay. That areas is largely unaffected by Delta outflow and is dependent on local flows.	3
93	4	2798	Use low salinity zone instead of X2 as the term X2 is mostly about the eastward extent of the 2 psu isohaline and does not account for the band of low salinity throughout the SF Estuary and its tributaries in the SF Bay and Suisun Bay.	3
93	4	2799	Replace the term 'preferred' with 'import'. It not known what spawning habitat is 'preferred'. Evidence of local production of YOY was evident in the 'preferred' habitat even during 2013 and 2014 (Grimaldo et al 2017). Both were dry years. In addition sampling in 2016 found local production in Suisun Bay and the SF Bay (Grimaldo et al 2020). This year was also dry and had been proceeded by 4 dry years with X2 moving ever more eastward.	3
93	4	2801	The term fitness is not used properly in this sentence. Although, longer spawning migration distance could have a fitness cost the way the sentence uses the term is incorrect. The longer migration distance may impact the three Rs with less representation by the upstream habitat by recruits.	3
94	4	2817	Provide citation. Was this Hobbs et al 2010, Grimaldo et al 2020, or Lewis et al 2020? It seems like maybe either Hobbs et al 2010 or Grimaldo et al 2017.	3
94	4	2819	This section seems to forget that there are tributaries in San Pablo and Suisun Bay that would still have low salinity habitat with substantial shift in X2.	3
95	4	2830	This statement is not supported by the text in this section. There were no citations to show that reservoir releases or export curtailments would abate salinity intrusion by climate change. Replace the section "if not sufficiently abated by increasing reservoir releases and export reductions (thereby extending already stretched water supplies)" with "under future conditions that incorporate climate change scenarios and consideration of changes in water consumptive use". Otherwise there needs to be an analysis or citation(s) that support this statement which this document does not do and would therefore not be defensible.	3
95	4	2826	Many of the statements in this paragraph are not substantiated by citations or a robust analysis therefore would not be defensible. Provide those citations/ analyses in this section on climate change to have a more defensible and robust conclusion. Also fitness is not used properly again. It is being conflated with health.	3
95	4	2848	This sentence is very nearly quoted from the cited manuscript. Suggest rewriting the sentence.	3
95	4	2853	There are no citations for any of these statements yet there are several studies that directly or indirectly apply to them. Do a quick literature search on predation potential and striped bass to get Grossman et al 2016 and Nobriga and Smith 2020. For silversides there are the citations by Schreier et al 2016 and Hamilton and Murphy 2018. As for SAV there is the Farrari et al 2014 and Zeug et al 2021 that could cited as well. Several of these citations were on other species but the implications of those papers still applies to Longfin smelt. Grossman, G. D. (2016). Predation on fishes in the Sacramento–San Joaquin Delta: current knowledge and future directions. San Francisco Estuary and Watershed Science, 14(2). Nobriga, M. L., & Smith, W. E. (2020). Did a Shifting Ecological Baseline Mask the Predatory Effect of Striped Bass on Delta Smelt?. San Francisco Estuary and Watershed Science, 18(1). Schreier, B. M., Baerwald, M. R., Conrad, J. L., Schumer, G., & May, B. (2016). Examination of predation on early life stage Delta Smelt in the San Francisco estuary using DNA diet analysis. Transactions of the American Fisheries Society, 145(4), 723-733. Hamilton, S. A., & Murphy, D. D. (2018). Analysis of limiting factors across the life cycle of delta smelt (Hypomesus transpacificus). Environmental management, 62(2), 365-382. Ferrari, M. C., Ranåker, L., Weinersmith, K. L., Young, M. J., Sih, A., & Conrad, J. L. (2014). Effects of turbidity and an invasive waterweed on predation by introduced largemouth bass. Environmental Biology of Fishes, 97(1), 79-90. Zeug, S.C., Beakes, M., Wiesenfeld, J., Greenwood, M., Grimaldo, L., Hassrick, J., Collins, A., Acuña, S. and Johnston, M., 2021. Experimental Quantification of Piscivore Density and Habitat Effects on Survival of Juvenile Chinook Salmon in a Tidal Freshwater Estuary. Estuaries and Coasts, 44(4), pp.1157-1172.	3
96	4	2884	Very interesting to hear about the issue with C caspia. Are there studies that can be cited as to why this species was highllighed over other invasiives? There are likely too many invasive to be thorough on this subject. Perhaps concentration on ecological traits of invasives that are most concerning regarding Longfin smelt and then identify some examples of invassive species that can exhibit those traits that have been identified in the estuary or as likely invasives.	3
103	4	3049	This table is difficult to interpret given the use of qualitative and subjective terms like 'substantial' or 'catastrophic'. The color coading should be described in the table description. Why is "potentially catastrophic' yellow when "moderate increase" is also yellow?	3
104	4	3068	There is year to year productivity therefore stating the flows in April and June are 'insufficient' is not supported by the data. In addition flows in the summer were evaluated in Nobriga and Rosenfield (2018) and were not found to be a major factor limiting the survival of LFS juveniles. Delete the last sentence.	3
105	4	3086	No analysis was provided that shows that management actions would abate the salinity intrusion projected in the climate change scenarios. Delete this statement.	3
105	4	3089	Do not ignore the contribution of low salinity habitat for spawning and rearing. This can also be impacted by freshwater flows not just the freshwater habitat. Also remind the reader that the freshwater spawning is needed to support the three Rs otherwise some may consider it an exaggeration without that context.	3

Page	Chapter	Line Number	Comment	Reviewer
145	TN 2	4729	Suggest providing the citations for the surveys methods like Honey et al 2004 among others. Honey K, Baxter R, Hymanson Z, Sommer T, Gingras M, Cadrett P. 2004. IEP long-term fish monitoring program element review. Interagency Ecological Program for the San Francisco Bay/Delta Estuary. IEP Technical report no. 78. Sacramento (CA): California Department of Water Resources. [cited 2008 Apr 24]. A	3
147	TN 2	4800	Cite the k-fold method.	3
149	TN 2	4888	The LFS MAST is not currently publically avialable. Thererfore it would be good to add another reference. Perhaps communication with the individual that provided the comment in the MAST or was it identified in the LFS Science Plan that was developed for the ITP? Reach out to Mike Eakin on the latter point.	3
9	2	189	The page designation for Stanley et al (1995) is incorrect. It has only pages 390-396. Was this supposed to be page 395? Also the authors only evaluated the Lake Washington to SF Estuary populations and found that the "two populations of S. thaleichthys were similar genetically" and "had no fixed allelic differences." They only determined the difference due to assumptions of isolation. Better to use Israel and May (2010) as it did find a genetic difference. Israel, J. A., & May, B. (2010). Characterization and evaluation of polymorphic microsatellite markers in the anadromous fish Spirinchus thaleichthys. Conservation Genetics Resources, 2(1), 227-230.	3
16	2	467	The citation Hieb and Baxter (1993) is not readily available even though it is a state document. Provide more citations that support this statement or provide the citation so that it can be accessible. Also Baxter 1999 found a relationship with Delta outflow not X2 during the months the larvae were collected. Delta Outflow and X2 are correlated but they are not the same thing. Change the term X2 with Delta outflow.	3
19	2	539	The statement that the FMWT favored the MWT after 2002 attached selectivity change that is not substantiated and is not supported in the analysis by Tobias and Baxter 2021. Delete this statement as the same author has updated the analysis. Tobias, V., & Baxter, R. (2021). Fewer and Farther between: Changes in the Timing of Longfin Smelt (Spirinchus thaleichthys) Movements in the San Francisco Estuary.	3
9	2	208	It needs to be clarified that the species is officially a Designated Population Segment. This SSA does not actually do that. The official designation must be registered. Provide that reference or remove the designation in this document as it will be misleading. Alternatively if this document is to be that registration of the SFE Longfin smelt as a DPS than that should be stated as part of its purpose.	3
37	3	1134	The volumes in the Rosenfield and Lewis analysis as well as the Phillis et al analysis use volumes that are likely outside the range of feasibility. The values were chosen as a way to provide context. That is lost when this SSA refers to those documents. The +5 or 10% of baseline are at the edge or beyond the current capacity for the watershed to provide. That should be stated more clearly as it would mislead anyone relying on this document for informing their ecosystem or resource management and would lead to spurious conclusions.	3
35	3	1055	When discussing stressors it should be noted each time which temporal and spatial framing is being used. Sometimes it was not clear and to make it more useful for readers it will be enormously helpful to report where and when stressor (when it is possible) effects should be considered.	3
35	3	1057	Only the stressors included in this statement are discussed. I was given the impression that other would also be discussed. How were these selected while others like dredging or in water constructions were not. For the former these efforts must account for impacts to longfin smelt. Is that no longer the case? As for in-water construction, the Bay Bridge construction and proposals for the Delta Conveyance project were supposed to consider impacts to longfin smelt. This document would have been a resource but either manager of those efforts would have been given the impression it is a non-issue. Make sure to highlight all the future impacts that may come from efforts that are known to be coming. In-delta construction like salinity barriers or coffer dams will continue to happen regardless if there is a Delta Conveyance or not. Also it seems that Technical Note 4 has identified ocean conditions, such as Upwelling in August as a factor. Why not include that impact? Ignoring that impact is why we have the Thiamine deficiency issue we had for Chinook salmon. The included analysis identifies ocean conditions as a potential stressor then it would likely suggest that you should include a discussion on that point in the Chapter 3. Otherwise why was it included in the analysis.	3
129	TN 1	4150	Does the max density occur in the first month of the 20-mm survey? If so, should note that the survey may have missed the max density of LFS for that cohort.	3
131	TN 1	4250	In discussion of the limitations of the abundance indices it should be noted that an underlying assumption is that catchability is constant through time. Environmental covariates that affect detection like water clarity varies over time; occupancy at the fixed stations also varies over time indicating large scale shifts in species distribution (e.g. Peterson and Barajas, “An Evaluation of Three Fish Surveys in the San Francisco Estuary, 1995–2015.”). Estimates of lambda and quasi-extinction probabilities will be sensitive to the assumption of constant catchability in the abundance indices even if the trend in abundance is correct. For example, if the true state of lambda is a constant 0.95 with no variance, but the estimate is 0.95 with a non-zero variance due to time-varying catchability, then quasi-extinction probability is going to be greater for the exact reason that is described at Lines 4207-4210. Peterson, J. T., & Barajas, M. F. (2018). An Evaluation of Three Fish Surveys in the San Francisco Estuary, 1995–2015. San Francisco Estuary and Watershed Science, 16(4).	3
136	TN 1	4306	Recommend using the function ‘countCDFxt’ in the popbio package to plot the bootstrap confidence intervals. Also, I don’t see the line for SFBS OT Age-1, the only index with a mean lambda greater than 1. Is it overplotted or not visible on the scale?	3
128	TN 1	4135	It should be stated that quasi-extinction is with respect to the survey. The surveys do not sample the full known distribution of the population; a range shift in the distribution of LFS to outside the footprint of a survey would be interpreted as extinction. For example, LFS are known to be distributed beyond the western edge of the 20-mm survey in wet years (Grimaldo et al 2017), which for these analyses will be interpreted as a smaller abundance index. Grimaldo, L., Feyrer, F., Burns, J., & Maniscalco, D. (2017). Sampling uncharted waters: examining rearing habitat of larval Longfin Smelt (Spirinchus thaleichthys) in the Upper San Francisco Estuary. Estuaries and Coasts, 40(6), 1771-1784.	3
158	TN 3	4977	The sharing of the R code from TN 1 was very useful. It was difficult to track what was done in the model without it. Can you provide that for TN 3 as well.	3
175	TN 4	5437	The sharing of the R code from TN 1 was very useful. It was difficult to track what was done in the model without it. Can you provide that for TN 4 as well?	3
176	TN 4	5465	I believe the error term on right of semicolon is meant to have the subscript N0 not N2. Also, are the error terms assumed constant or should there be a ‘t’ subscript as well?	3
176	TN 4	5469	I believe the error term on right of semicolon is meant to have the subscript N0 not N2. Are the error terms assumed constant or should there be a ‘t’ subscript as well?	3
175	TN 4	5427	In discussion of the limitations of the abundance indices it should be noted that an underlying assumption is that catchability is constant through time. Environmental covariates that affect detection like water clarity varies over time; occupancy at the fixed stations also varies over time indicating large scale shifts in species distribution (e.g. Peterson and Barajas, “An Evaluation of Three Fish Surveys in the San Francisco Estuary, 1995–2015.”). Estimates of lambda and quasi-extinction probabilities will be sensitive to the assumption of constant catchability in the abundance indices even if the trend in abundance is correct. For example, if the true state of lambda is a constant 0.95 with no variance, but the estimate is 0.95 with a non-zero variance due to time-varying catchability, then quasi-extinction probability is going to be greater for the exact reason that is described at Lines 4207-4210. Peterson, J. T., & Barajas, M. F. (2018). An Evaluation of Three Fish Surveys in the San Francisco Estuary, 1995–2015. San Francisco Estuary and Watershed Science, 16(4).	3
186	TN 4	5663	Provide the Credible intervals. Is the year to year variation in vital rates greater than the uncertainty in their estimate?	3
189	TN 4	5671	Please provide the Credible intervals. Is the year to year variation in vital rates greater than the uncertainty in their estimate?	3
189	TN 4	5673	Unclear if this is representing the range N simulations, or the mean and credible interval for a single simulation?	3
17	2	506	It should be clarified what is a result of the BRT and what is a product of the BRT predictions. Line 504-505 correctly reports the most important predictors determined by the BRT (i.e. given past catch data and corresponding environmental conditions, these are the predictors that do the best job of predicting out-of-sample catch). However, the maps described in 506-514 are a product of the BRT predictors and modeled salinity, temperature, and turbidity for select years and months (i.e. given the BRT predicted effects of temperature, salinity, turbidity, depth, etc.	3
37	3	1122	Need to clarify as to what the +SWP metric is. Is it mean SWP exports in all years? Or SWP exports in the specific water year types? For example, what volume exports is used for the Critical water years? Further, are the SWP exports consistent with existing pumping regs? E.g. Are the SWP exports used in the model for Critical years consistent with what is allowed/possible currently? For the recent years SWP has been under fairly low to just at health and safety standards.	3
37	3	1130	“subtraction of SWP exports alone” is a useful bookend to understand the realm of possible outcomes, but it is misleading to present it without also acknowledging the major, and perhaps disqualifying, tradeoffs that would have to occur (for instance, is it even possible to meet health and safety standards with other water sources?). Not to mention, the 30% increase in a Critical water year is only 60 additional units on the Age0 recruitment index, so subtracting all SWP exports would have massive consequences for water supply throughout the state during extreme drought and would garner few additional Age0 recruits relative to recruitment in any other water year (reducing NDOI by 10% in a Dry year still produces ~1.5 times as many recruits). Large percentages of small numbers are small numbers, small percentages of large number are large numbers.	3

Page	Chapter	Line Number	Comment	Reviewer
37	3	1126	10% of NDOI in an average AN and BN years is around 1.5 and 1 million TAF, respectively. That context is not clear in the material presented and it is misleading to characterize this change “as little as 10%”. Also, not sure if it is intended, but in Line 1127 BN and AN years are considered, but in Line 1128 D and AN are considered. I’m guessing they mean to consider D, BN, and AN?	3
37	3	1122	In general, this entire paragraph severely downplays the magnitude of change in outflow or water project operations necessary to produce the reported changes in Age0 recruitment. It would be helpful to know where these NDOI and water project operation scenarios lie relative to the Future Condition scenarios considered later in the SSA. Are they better or worse than the worst-case Future (hydrology) Condition considered? If increases/decreases in NDOI by 5 to 10% in AN and BN years is equal or worse than the worst-case future conditions being considered, then maybe those changes in NDOI should be characterized that way instead of minimizing them (e.g. Line 1126 reducing NDOI “by as little as 10%”).	3
97	4	2949	In discussion of the limitations of the abundance indices it should be noted that an underlying assumption is that catchability is constant through time. Environmental covariates that affect detection like water clarity varies over time; occupancy at the fixed stations also varies over time indicating large scale shifts in species distribution (e.g. Peterson and Barajas, “An Evaluation of Three Fish Surveys in the San Francisco Estuary, 1995–2015.”). Estimates of lambda and quasi-extinction probabilities will be sensitive to the assumption of constant catchability in the abundance indices even if the trend in abundance is correct. For example, if the true state of lambda is a constant 0.95 with no variance, but the estimate is 0.95 with a non-zero variance due to time-varying catchability, then quasi-extinction probability is going to be greater for the exact reason that is described at Lines 4207-4210.	3
3	1	1	Thank you for the opportunity to review and provide comments. I want to commend the USFWS for the inclusion of reviewers to provide transparency and foster collaboration. The co-authors had an enormous job to perform in trying to stitch together the many manuscripts, reports, communications, and presentations into this Species Status Assessment (SSA). As I understand the document is a review of the biology and ecology, the status, and an assessment of current and future threats of the Longfin smelt subpopulation from the San Francisco Estuary and will be a reference to “support all functions of the Service’s Endangered Species Program..” As a ‘partner’ reviewer, my comments were focused on providing constructive suggestions and edits to further improve the documents function in fulfilling its purpose. I want to apologize beforehand for any confusion in trying to interpret my comments. It was an odd system to provide those comments but I am available for further discussion and context for any of the comments provided. Although large portions of the subpopulations are covered there are significant gaps that are not covered both geographic and in regards to current and future threats to the subpopulation. As the SSA is much like a literature review it would be useful to include criteria for inclusion of references as it was difficult to determine what constituted as a reference as described under ‘best available science’. The inclusion of the technical notes for review facilitated a more transparent assessment of the viability analysis. More details are requested to further improve that transparency. Addressing the comments provided will go a long way toward improving the usefulness of the SSA to both resource and ecosystem managers regarding Longfin smelt in the San Francisco Estuary.	3
9	2	184-208	Awkward paragraph. Replace references to "small portion" of the population with "unknown portion" -- it is not clear that there is migration to or genetic exchange with northern populations in every year. I recommend simplifying & reducing redundancy. The major points are 1) Biogeographical, behavioral, ecological, physiological (size, swimming ability) evidence indicates the population is distinct, and 2) the population is ecologically significant and unique -- its loss would represent a significant truncation of the species' range	4
9	2	214	"historical island marshes" are a property of the Delta, not the Bay-Delta as a whole.	4
9	2	217	Need to define the term "upper estuary" -- seems like you're mainly talking about the Delta, though.	4
8	2	172	Longfin Smelt have been caught further upstream on the Sacramento, I believe. Also, on the San Joaquin, LFS have been detected near the confluence with the Tuolumne (see Rosenfield 2010 at p. 6)	4
49	3	1545-1546	I don't understand -- are you saying that ocean currents carry contaminants from northern California to the Gulf of the Farallones?	4
49	3	1547	There is no agriculture *in* Suisun Bay -- I think you mean "around the margins of"	4
50	3	1556-1595	Correlations between ammonium concentration (particularly, from wastewater treatment plants) and Delta smelt abundance have been questioned on methodological grounds (Cloern et al. (2012) wrote: "As a real example, Glibert (2010) inferred a strong negative association between delta smelt abundance and wastewater ammonium from regression of CUSUM transformed time series. However, the Pearson correlation (r .096) between the time series (Fig. 1) is not significant, even under the naive IID assumptions (p 5 0.68). In short, correlations between CUSUM-transformed variables should not be used as a substitute for analysis of the original untransformed variables" Limnol. Oceanogr., 57(2), 2012, 665–668). Also, the pattern doesn't hold up under scrutiny -- see Cloern 2021 here: https://escholarship.org/uc/item/1xz922jm (" Synchronous increases of chlorophyll and NH4 over the past 3 decades are not consistent with the ammonium suppression hypothesis....These five results are all consistent with an alternative hypothesis that the chlorophyll decline in Suisun Bay was largely a result of increased grazing losses to an introduced filter feeder and was unrelated to ammonium suppression of growth.")	4
51	3	1620-1634	Here and throughout Section 3.1 (and throughout the SSA, in general), I recommend focusing on topic sentences. W/R/T to agricultural diversions, you conclude that they are of little importance to LFS (agreed), so why not just say that up front and save your reader some time?	4
51	3	1636-1645	You're leaving it open for the reader to speculate/guess as to whether this is a potential major stressor on teh population. The scale of exposure * diversion volume seems seriously mismatched. Fish may die here, but it's extraordinarily unlikely that this has a had a major effect on LFS in the SFE historically.	4
51	3	1664-1666	You just said that larval fish salvage was unmeasured. So, you'll need to caveat this statement to say salvage of Age-0 is "believed to be" related to larval population size and larval entrainment.	4
52	3	1668-1672	This sentence is awkward; there are too many thoughts here and they are not completed.	4
52	3	1693-1694	Is there any evidence that any Longfin Smelt entrained in Clifton Court Forebay can/do escape? I think teh going assumption is that they all die, even if they are "salvaged".	4
53	3	1729-1731	You need to explain pre-screen mortality up front, not introduce it, somewhat apologetically, at the end of a paragraph. I would describe what is known about the relationship between salvage and direct entrainment-related mortality for other species. Just because studies haven't been done using Longfin Smelt does not mean we are not reasonably certain that the same relationship holds qualitatively for Longfin Smelt -- expansion of salvage for pre-screen mortality may be lower magnitude or it could be higher magnitude for LFS, but it's not zero and it's almost certainty not insignificant. Also, so far you have not mentioned indirect effects of entrainment (i.e., reduced survival for fish outside of Clifton Court forebay), although this is believed to be one of the largest effects of exports on migrating salmon and Delta Smelt. See for example, SWRCB 2017 at p. 3-47 citing the U.S. Department of Interior re: salmon entrainment-related mortality: “More important than direct entrainment effects, however, may be the indirect effects caused by export operations increasing the amount of time salmon spend in channelized habitats where predation is high (USDOI 2010, p. 29).”	4
53	3	1741-1743	This is speculative -- you haven't analyzed the potential effect of direct entrainment-related mortality. The impact of entrainment related mortality is related to the size of the overall population -- entrainment/salvage mortality must be compared to abundance indices. Therefore, I recommend comparing estimated entrainment of longfin smelt in any year to the FMWT (or Bay Study) abundance index in either the previous fall (estimates size of Age 0+ population) or two years prior (estimates size of Age 1+ population) -- or use an average of abundance indices from one and two years prior. This will reveal historically high rates of entrainment in 2012, 2013, and 2020.	4
55	3	1791	You refer to Figure 3.5, but I think you mean to refer to figure 3.6? Also, the caption for Figure 3.6 does not describe the inset. Also, are there more recent data available? Finally, given you state that the 20mm survey may be an underestimate in wet years, I think you should identify those years on the graph (e.g., 2011, 2017, 2019)	4
56	3	1798	Similar comment as for 20mm survey. I think you're refering to the wrong figure in the text (doesn't match the number of the actual FMWT figure). And, you need to describe the inset figure in the caption and use the most recent data available. I also recommend converting the y-axis to a log scale in order to capture the fluctuations over the past 2 decades.	4
56	3	1815	See comments re: previous sampling programs. Align Figure numbers with the text. Consider using log scale for the y-axis (or use the inset for that purpose).	4
58	3	1827	Figure 3.10 (the flow-Log(abundance) step change figure) is not referenced in the text and I don't think it's appropriate in this section. It's more relevant to your earlier description of flow-abundance "step-declines" (which I think are a bit antiquated at this point), but this figure is not as relevant to the question: "has the longfin smelt population declined?"	4
1	1	1	All of my comments will be submitted via email in a file that documents page, chapter, and line number. This is necessary -- I tried to use this web-based commenting platform and found it too cumbersome; it was interfering with the production of a quality review of the SSA.	4
4	1	18-28	The text at 49-51 (p. 5) makes this paragraph (on p. 4 at18-28) unnecessary. It is also confusing. That the SFE population of Longfin Smelt is distinct from other Longfin Smelt populations is no longer a scientific debate. The SSA should not re-iterate the Service's prior, flawed logic for finding that this LFS population was not discrete from other populations. That finding has been reversed, in part, because it ignored the best available science at the time, (Stanley et al 1995, stated: ",Ä¶[G]ene frequencies among these two populations of [longfin smelt] differed significantly, suggesting that current gene flow between them is restricted. This result, combined with geographic isolation between them suggests that the [San Francisco Bay-] delta population of [longfin smelt] warrants management as an isolated and genetically distinct entity. [Stanley et al. (1995, p. 390)]"). This section of the SSA should describe the Service's guidance on DPS and how evidence at the time of the 2012 determination mapped onto that guidance. The section should also describe (or at least allude to) the best available science (e.g., Saglam et al.), which confirms that the SFE population is discrete and distinct from other coastal populations of Longfin Smelt.	4

Page	Chapter	Line Number	Comment	Reviewer
4	1	12 -- 16	I recommend that the Service land on the smallest set of terms possible, define them precisely, and use them consistently. It makes sense to me that: "Delta" would be the legal boundary of the Delta, and that "Bay-Delta" would refer to Bay + Delta. (But choose either "Bay Delta" or "San Francisco Bay Delta") How you define "Bay" needs attention. "Bay" does not "encompass all waters west of Chipps Island". "Bay" should refer to pelagic waters ending at the Golden Gate on the west and Chipps Island on the east), regardless of salinity. You might expand this to include the marshes also, but you'll need to specify whether Bay is intended to encompass the marshes. I recommend that "San Francisco Estuary" not be used as a synonym with "Bay-Delta". Rather, I would use San Francisco Estuary to refer to all the fresh or freshwater-influenced (i.e, fresh & brackish) water under tidal influence -- this includes the Bay + Delta + the marshes + the Gulf of the Farallones. Longfin Smelt are found throughout this area, so having one term that captures that whole geographic range can be useful.	4
8	2	172	Longfin Smelt have been caught further upstream on the Sacramento, I believe. Also, on the San Joaquin, LFS have been detected near the confluence with the Tuolumne (see Rosenfield 2010 at p. 6)	4
9	2	217	Need to define the term "upper estuary" and use it consistently throughout the manuscript (or define the embayments and use those instead of "upper estuary" -- seems like you're mainly talking about the Delta, though.	4
9	2	214	"historical island marshes" are a property of the Delta, not the Bay-Delta as a whole.	4
9	2	184-208	Awkward paragraph. Replace references to "small portion" of the population with "unknown portion" -- it is not clear that there is migration to or genetic exchange with northern populations in every year. I recommend simplifying & reducing redundancy. The major points are 1) Biogeographical, behavioral, ecological, physiological (size, swimming ability) and genetic evidence indicates the population is distinct, and 2) the population is ecologically significant and unique -- its loss would represent a significant truncation of the species' range	4
10	2	238	Sentence describing the Practical Salinity Scale would be more appropriate as a footnote.	4
10	2	263	Was Wang certain of the function of the hardened anal fin (e.g., "sweeping fine sediments") or was this a presumption? I was not aware that we know that much about the specifics of Longfin spawning behavior or micr-habitat conditions. Throughout, the text should be revised to eliminate "just so" assertions about adaptation or do a better job of caveating them with words like "perhaps" "might be".	4
10	2	262	Change "undergo sexual dimorphism" to "are sexually dimorphic".	4
10	2	248-250	Awkward. Most Longfin Smelt die before spawning, well before they reach 2 years of age. Recommend rewriting to say that Longfin Smelt are semelparous and generally spawn at the end of their second year "although some individuals. may spawn...".	4
10	2	242	The phrases "Adapted to" and "governed by" are strong statements, requiring strong support. You could reference the habitats preferred by other members of this genus and family. But you could also just say that the "likely" require coldwater habitats and that their occurence in the SFE "appears to be limited to" habitats where temperatures are <22oC. The statement at 243-246 (re: movements of LFS in general) is consistent with a requirement for cold water, but it is not proof of that (migrations could be driven by many other forces).	4
10	2	233	Should also cite Rosenfield and Baxter 2007 and the Longfin Smelt DRERIP Life History Conceptual Model (Rosenfield 2010) here.	4
10	2	225-228	Recommend spelling out the recreationally important fish species (e.g., White Sturgeon, Striped Bass, etc.).	4
11	2	302	Sentence that begins "Two of the key hydrodynamic variables..." would be more appropriate as a footnote or in a different section.	4
11	2	287-292	Recommend removing reference to Moyle and Wang and their estimates of the spawning period because you're going to go with the better-documented method and estimates of Heib and Baxter, which are roughly consistent with Radtke. Inclusion of the other dates and approaches just confounds things. One point to emphasize here is that Longfin Smelt display highly variable life histories eith respect to timing (see Rosenfield 2010; DRERIP conceptual model, and also more recent otolith work by Lewis, Hobbs etc.).	4
11	2	276	"multiple stages of eggs" where? Do you mean within individual females? Please clarify.	4
11	2	267-268	See Figure 2, p. 33 of Rosenfield 2010 conceptual model for size-fecundity relationship in SFE. The sentence "Based on age-length relationships" does not follow from the previous statement. Seems like it wants to be its own topic -- after you describe the "broad range in mature fish". Also, many fish show wide-diversity in body size even among fish of roughly the same age -- i.e., differential growth rates. I would be cautious about inferring multiple spawning age-classes based just on differences in size among maturing fish.	4
12	2	328-335	Recommend referencing the phyologeny of this species. The reference to "mud" at 335 seems unwarranted. If you can't say anything specific about their likely spawning substrate (e.g., by reference to spawning substrates they use elsewhere or those used by congeners), I would not speculate -- the next sentence says that we don't know what substrates they use.	4
12	2	315	I would not describe the mutilple potential spawning sites or Longfin Smelt in the SFE as a "strategy". The ability to spawn in multiple areas certainly confers protection from localized catastrophes, but would not assert that this effect of individual behavior is a species-level "strategy". Be careful with this inference throughout. Individuals follow "strategies". The (valid) point you're making is that the presence of multiple strategies (behaviors, spawning locations, developmental schedules) has the effect of insulating the population from environmental variation. I also recommend pointing out that multiple individual life history variants (or a spectrum of variation) strongly suggests that different LH pathways have been successful (perhaps to differing degrees) in the past -- i.e., the lack of convergence to a single "strategy" tells us something about what has worked/not worked in the past.	4
14	2	407-412	I support the effort to define age-size cutoffs that allow discrimination between different year-classes of Longfin Smelt. (e.g., as in figure at line 4936 p 152). The data showing distinct modes of body sizes is strong indication that different year-class cohorts may be distinguished, in every month, by size. But, I don't think the monthly size categories are valid to analyze *growth* from month to month. This analysis and its results are not central to the purposes of this SSA. I would cut this discussion and the TN that describes patterns in intra-annual average length. If I understand correctly, the method for inferring inter-month growth relies on average length of Longfin caught in different months (TN 3, lines 5101-5104), after segregating different age-classes? But, this is not a closed population -- longfin are migrating in and out of the sampling zone of all of the fish surveys (including the Bay Study, but especially the EDSM and the FMWT) and one would expect that they may be migrating differentially based on body size. (the otolith isotope data demonstrate this size dependent migration, quite clearly). This is not a good method for inferring month-to-month somatic growth of longfin smelt given the (variable) migratory dynamics in this population and known factors affecting month-month distribution of the fish . For instance, if fish are continually leaving the sampling zone in the spring/summer (see Rosenfield and Baxter 2007; Rosenfield 2010), after they've reached a certain age or size, that would *look* like slow growth. Similarly, if fish return to the FMWT sampling region differentially based on size (with smaller fish remaining in deeper water to continue feeding), that would *look* like slow growth. Furthermore, the method for estimating temperature and salinity each month (TN 3 5054-5058 p. 159) ignores the distribution of the fish in each month and migratory dynamics between months. For estimates of growth rates in the field, I suggest relying more heavily on the Longfin otolith data -- those estimates are based on actual growth of individuals in the wild, rather than inferred from catch data. This would also allow one to look at the effects of migration (salinity) on growth rate. Finally, if you stick with this model and conclude that increased outflow = reduced growth (which may very well be correct, given how flow affects distribution of juveniles w/r/t water temperature), then you at least need to comment on why Souza 2006 found no outflow growth relationship -- what's the difference in the methodologies?	4
14	2	405	I would connect this discussion to the previous section reporting on larval Life History. Rosenfield and Baxter (2007) and Rosenfield (2010) observed a very long period of recruitment-to-the-net of juvenile Longfin (i.e., catches of YOY longfin continued to increase late into the fall. This suggests highly variable (and slow) growth from larvae to 40mm (the size at which Bay Study nets are believed to reliably detect longfin). This slow/variable growth has implications for when sampling data reflect juvenile recruitment (i.e., fall rather than spring or summer). Again, this is part of a larger story about life history diversity in this population.	4
14	2	378-381	Description of X2 should probably be a footnote, or saved for a different section. If the latter, you could just report Dege and Brown's findings with respect to "the 2ppt bottom isohaline (locally known as "X2")"	4
15	2	438	Bingo. Again, refer to Rosenfield and Baxter 2007 to see the very protracted recruitment of juveniles to the net (i.e., if hatching ends in April, why does the abundance of Age 0 fish *appear* to be increasing through December?) -- slow, variable growth explains that pattern (and also suggests that estimating growth from [inter-month delta in length of fish caught] might not be a good method.	4
15	2	434	You have not explained your expectations re: how the co-variates affect growth (especially as you seem to have measured growth). Not sure what the hypothesis is that would relate flow to somatic growth rate? Delta outflow may produce more food, but it also distributes the fish further downstream, into colder water (= slower growth).	4

Page	Chapter	Line Number	Comment	Reviewer
15	2	431-432	I'm confused about the statement here (lower temperatures, higher growth) as compared to the statement at 423-424 (higher temperatures, higher growth). Again, I think this discussion of intra-annual growth is not well-grounded and is not likely to be important to your listing decisions. I recommend eliminating most of it and replacing with what we actually know -- lab growth rates and inferences from otoliths (which, of course, only measure growth in fish taht survived). If you retain this analysis, then you need to explore the connections among these predictor variables. Higher outflows = lower salinities in the northern estuary (at least). And, higher Delta outflow shifts the distribution of larval (Dege and Brown 2004) and subsequent juvenile fish downstream -- where it is colder (because of ocean influence). See US EPA map project for effect of outflow on juvenile distribution of Longfin Smelt, portrayed here at Figure 15 (p. 48-49; https://www.researchgate.net/publication/308965834_San_Francisco_Bay_The_Freshwater_-_Starved_Estuary)	4
19	2	541-543	Change in ratio of catches between the two gears is important -- but, be careful with your interpretation of this pattern; it does not necessarily reflect a change in fish behavior (as suggested here) or a change in environmental conditions. Use of "more frequently" and "less available" implies that fish are moving to deeper water. It may simply reflect a declining population and the fish's well-documented "preference" for deeper water. The same pattern appears in Suisun Marsh --Longfin only occur in any numbers in Suisun Marsh when they are abundant/dense elsewhere. Finally, as noted at 543, the differential between MWT and OT may reflect a lateral shift, to the Central Bay, rather than a vertical shift everywhere. The OT may be more effective than the MWT in the deeper waters of Central Bay. Again, this may be because (a) the fish now migrate to Central Bay or (b) because they suffer differential mortality upstream or (c) because there are fewer fish now and so catches of any fish at all are more likely to occur in their prime habitat (which is downstream during the spring and summer of most years).	4
19	2	532	If this study of diurnal changes in distribution did not catch enough fish to produce results, I would simply mention that and move on. It's potentially confusing and not that helpful to discuss results that are effectively anecdotal, because of low sample sizes.	4
19	2	524	This sentence may mislead as it suggests that most Longfin Smelt are 40mm by May. That's not the case (see Rosenfield and Baxter 2007). I would reword to say that YOY longfin smelt *begin* to reach 40mm, and are thus detectable in Bay Study nets, by May of their first year, but recruitment to that net continues well into the fall.	4
21	2	560-576	I don't think we know what proportion of these fish migrate at what age -- the data you present from Levi Lewis re: O-isotopes are the most recent relevant data, but I don't think he has enough to suggest what fraction of the population is where, in any given season. Also, has Lewis approved use of these data? Also, I would not speculate on the reasons that the fish migrate (e.g., "escape warmer water or "gain access to food" -- because (a) we know that they don't all migrate (at least not at the same time) and (b) the costs and benefits of anadromy are probably numerous -- we wouldn't say Chinook Salmon migrate to the ocean to "escape high temperatures" (even though that's probably an effect of their anadromy) right?	4
21	2	560	As currently worded, you imply that Rosenfield and Baxter know (or said) that some Longfin spent their entire life in the Bay -- I don't think they say much about individual fish behavior. It's more accurate to say: "...in any given month, some fraction of the LFS population remain in the Bay, but an unknown fraction may be found in the ocean (Rosenfield and Baxter 2007); longfin smelt have been detected in the nearshore ocean off of San Francisco (Garwood 2017; City of San Francisco/CH2MHill...). Ocean migration appears to occur in the first year of life, but may occur at other times in the life cycle as well. Patterns in O-isotopes (Lewis et al. 2019; figures ___ used in this SSA) confirm high variability in migratory timing and duration among SFE longfin smelt."	4
21	2	556	See caption -- it may infer a change in the absolute number of fish, rather than a shift in the relative number of fish. An increase in absolute numbers caught in deeper water (i.e., higher catches in the OT on an absolute scale) might suggest a real change in condition/behavior -- but the relative shift could easily be an artifact of a change in overall abundance throughout the estuary.	4
22	2	589 and 603	Throughout the document: When presenting figures that the Core Team did not develop for the SSA, you should obtain permission and cite the source of the graphic in the caption	4
22	2	580-585	A) You say there may be 4 life history strategies, but then you only define three options. (B) these results only show when the fish migrated to water >6ppt -- as a result, they're not relevant to the preceding description of anadromy (as implies at 580). Whether these fish display 3 or 4 or more life history strategies as juveniles misses the point that they display highly variable life histories (in terms of the timing and distribution of different life history stages). I think that's a more important point for management and conservation than nailing down specific "strategies" (which, as you note (lines 585-587)) may only be us imposing categories on a continuous range of behaviors).	4
23	2	598	You need to ask Lewis if the O-isotope data "support the theory that a number of Longfin Smelt remain in the San Francisco Estuary for their entire lifecycle" (1) In part, this depends on what you mean by "SF Estuary" (see my earlier comment about definitions) (2) Almost all of the O-isotope traces displayed in Lewis et al. 2019 Figure 11 (p. 69) extend to >30psu. There are 1-2 that don't reach this level, but it's not clear whether the lower salinities suggested in those few otoliths are sufficient to conclude that these fish rnever migrated to the ocean. You'd have to ask Lewis or others how precise the O-isotope data are at high salinities (i.e., close to 32 psu). (3) "a number" might be interpreted to be "a sizable fraction" -- you might consider writing "a few Longfin Smelt (at least) remain in the Estuary...". (4) keep in mind that, depending on the season and year, marine waters extend well into the "Bay" (especially at depth) and brackish water can extend into the Gulf of the Farallones. Again, I emphasize that early and clear definition of geography (and distinguishing land/water boundaries (="Bay") from ecological conditions (="estuary") is critical.	4
25	2	676	"data are" plural, always. ("datum" is)	4
25	2	656-675	I don't know the basis for the claim that LFS do not "fully recruit to the [FMWT] gear until they are 60-77mm" and I don't think that's correct. Both Bay Study nets have the same minimum mesh size and it's the same as the minimum mesh size of the FMWT -- Rosenfield and Baxter (2007). Tobias and Baxter (in press) and R&B 2007 both use 40mm as the size at which LFS are reliably caught by both the Bay Study's OT and its MWT and the FMWT. Please correct or justify this claim with literature ("pers. comm" isn't going to cut it given the thresholds established in the literature.	4
26	2	720	Yes. It is not uncommon for size at hatching to be inverse to incubation temperature. See my earlier comment re: inferring hatching size of fish -- it may be affected by temperature as much (more) than it is affected by spawn date.	4
26	2	716	This is a better way to discuss the fish's likely "adaptation" to cold water -- earlier in the SSA the statements are declaratory, which is inappropriate.	4
26	2	695-697	Throughout this section, the emphasis is on the "shortcomings" of each sampling program. You should emphasize that each of the sampling programs targets (intentionally or not) a specific component of the LFS population and that taken together, these surveys provide *excellent* coverage of the likely habitats of Longfin Smelt adults and juveniles. You note, appropriately, that larvae are not sampled through their full geographic or temporal range, but adults and juveniles are very well sampled by several of the surveys individually and by all of the survey's collectively -- Rosenfield and Baxter and Tobias and Baxter make this point. Furthermore, because of this fantastic coverage by multiple different sampling programs, we can explore different hypotheses about abundance and distribution of these fish -- e.g., the fact that we now detect more LFS in the Otter Trawl than in the MWT tells us something about the preferred habitat of these fish. Similarly, the lack of detection in Suisun Marsh, compared to periods when LFS were common in the Marsh informs us about a "range contraction", which is likely a byproduct of their greatly reduced numbers.	4
27	2	745-755	Throughout the SSA -- you should define terms like "yolk-sac larvae" and other stages by size (maybe you did this and I missed it). See also at lines: 781 I raise this point here because you're talking about YSL doing better in 2-4ppt water and this is consistent with Hobbs et al. 2010. BUT, Lewis's isotope data, that you show earlier, strongly suggests that the fish move to saltier water while they are still larval (maybe this is after "yolk-sac" absorption -- but it would be easier to tell if you defined the stages by size or just referred to larval size ranges when describing their preferences).	4
27	2	726	Detection of larval LFS at a given temperature range does not suggest that the same temperature range is appropriate for eggs -- strike that reference. Also, the temperatures where you find the larvae could have more to do with favorable salinity or turbulence -- at that life stage, the fish are found at or around X2 and you've suggested that this is because of salinity or other LSZ characteristics.	4
27	2	740-742	You need to provide more evidence that LFS spawning is restricted to temperatures ≤13oC. You're saying the fish will spawn at 16oC and that some survival (you don't say how much) occurs at 15oC. I understand the data indicate that colder temperatures (9-12oC) produce better results (in the lab) and you make that point well -- but I don't know what the evidence is for 13oC. Seems like there is at least some habitat value at 15oC (you should state the results at this temperature) and the fish's behavior suggests that there may be habitat value up to 16oC.	4
28	2	802-803	This point should be emphasized with greater explanation. After accounting for the effect of flow and spawning stock size on recruitment, Nobriga and Rosenfield (2016) found no evidence of a decline in juvenile LFS recruitment over the 50 year data series. Thus, there is no evidence that larval survival has been affected by shifts in the larval LFS prey base, beyond those that are explained by flow. Also, they did not detect a "step" decline in population productivity associated with the year of Amur clam invasion (see also, Thomson et al. 2010 and Maunder et al 2015). Clearly, longfin smelt post-YSL and juveniles need food, but the evidence that prey abundance for these life stages has been limiting productivity is not strong. (Nobriga and Rosenfield 2016 detected that survival from juvenile to adult seems to have declined -- it is possible that that decline could be related to prey availability (or increased predation), but the prey/predators involved would include those that occur well downstream of the Delta/Suisun. See also at lines 870-876.	4
28	2	793-893	This discussion of "dietary needs" is one sided -- it's all about food supply. You're also assuming that higher growth rates for longfin smelt might make them less susceptible to predation. But (a) growth is a matter of balancing food intake with metabolic demands, not just diet alone (e.g., more food does not necessarily mean better survival if the food can only be accessed in warmer environments) and (b) their ability to capture prey, their exposure to predators, and their metabolic demands depend on where they are in the estuary (temperature and prey/predator communities change a lot as salinity changes along the axis of the estuary).	4

Page	Chapter	Line Number	Comment	Reviewer
28	2	785-789	This is speculative and inappropriate for a section on Longfin Smelt "needs". As juveniles, these fish are strongly bottom oriented (Rosenfield and Baxter 2007; Rosenfield 2010). It's dark down there; turbidity is not likely to provide much additional cover. You suggest (without much evidence) that they may be surface oriented at night, but I would not expect turbidity to provide much visual cover at night (except perhaps on evenings with clear skies and fuller moons).	4
28	2	776-779	Define NTU in a footnote?	4
29	2	850-852	As you note at 854, Stevens and Miller 1983 studied Delta inflow -- you discuss this next, so you can strike that reference here. But, if you want to discuss Delta inflow separately, you could also cite Maunder et al. 2015. Re: Delta outflow. Be thorough and add citations to include Kimmerer 2002; Rosenfield and Baxter 2007, Kimmerer et al. 2009; Thomson et al. 2010; Mac Nally et al. 2010; Rosenfield 2010; Nobriga and Rosenfield 2016. re: "little change in slope" of the flow-fish relationship. Should cite Kimmerer 2002; Kimmerer et al. 2009; Nobriga and Rosenfield 2016	4
29	2	836	The terms "species needs" and "population needs" are offputting. The species and population don't "need" anything -- they're not individuals, but distributions of individuals. The prior section might be better titled "niche requirements". The "population needs" section might better be titled "Requirements for population resilience", since that's what you're really discussing here.	4
29	2	828	Tighten this up by presenting more of the results and identifying uncertainties (without speculating about possible mechanisms). For instance, I think you mean that the *onset* of migration is closely tied to the solstice. Is the onset of spawning also correlated with all these other factors, or is it the number of migrating females that is correlated with things like Secchi depth (and is it a positive or negative correlation?), temperature, etc? Help the reader understand what factors are related to the onset of spawning and, separately perhaps, the magnitude of the spawning run.	4
29	2	814-820	This paragraph needs to be reworked. We know fish spawn around the Bay margins (so the fact that "eggs have not been detected" does not need to be repeated). It seems like you're trying to establish that Longfin eat up through sapwning (unlike, say, Chinook Salmon). But it sounds like you're saying "fish need food throughout their life cycle", which isn't as informative.	4
30	2	878	This section should open with a sentence indicating that the effect of flow on LFS abundance most likely occurs in the transition between Adults and 40mm juveniles. Nobriga and Rosenfield (2016) found that flow was the *only* variable affecting this transition and found no flow effect related to the transition from Juveniles to Adults. This will focus your discussion of hypothesized flow mechanisms (i.e., they seem not to be related to survival from juvenile to adult).	4
30	2	883	"rearing" should be "larval rearing"	4
30	2	870-876	The SSA misrepresents Nobriga and Rosenfield's 2016 paper in the same way that it was misrepresented by DWR's EIR for SWP operations -- I would not rely on DWR's misrepresentation of this important paper. Nobriga and Rosenfield (2016) do not say what you say at 874-876. Their paper showed no significant change in the relationship between flow v. recruits-per-spawner over the 50 year data series. They wrote: "There was no evidence that the ratio we used to depict recruits per spawner has declined over time; thus, food web changes apparently have not impacted this life stage transition." (at 54-55). See also at p. 52 of that mansucript. Nobriga and Rosenfield did find evidence of a decline in survival from juvenile to adult (2 stage model); but they could not discriminate well between a 1991 step-change vs. a continuous decline (represented by the "Year" variable) -- they proceeded to model a 1991 step decline for convenience (because discriminating between the two different patterns of survival decline was not essential to their research question. N&R (2016) did not test for step changes in 2002 (perhaps you're thinking of Thomson et al. 2010?)). Key implications of N&R 2016 were (1) that the flow-abundance relationship has not changed w/r/t juvenile LFS recruitment and (2) repeated step changes are not needed to explain the FWMT decline (because continuous decline in juvenile--> adult survival is equally likely (see Table 5 "Step 2" and "Step 4" of that paper)). Note also that Rosenfield and Baxter (2007) found the same decline in juvenile-adult survival (post 1987-1991 drought), writing (at 1586): "The slope of the relationship between the FMWT age-class 2 index and the covariate (the previous year's FMWT age-class 1 index) differed significantly among time periods (ANCOVA, time period 3 age-class 1 index interaction: P = 0.018; Table 3). ...This analysis revealed that age-class 2 index values declined significantly from predrought levels to postdrought levels (ANCOVA, time period main effect: P , 0.001; Table 3)".	4
30	2	865-869	Thomson et al 2010 found evidence for step declines in 1989, 1991, and 2004 -- but not in 1987, as per Kimmerer 2002. This pattern of repeated step declines (and other "step declines" postulated subsequently) raises the question of whether we're just observing a continuing decline vector that is separate from the decline related to outflow. Nobriga and Rosenfield suggested that this continuous decline might be the case, even though they also found some evidence of a 1991 step-decline in juvenile--> adult survival. Also, Rosenfield and Baxter (2007) looked for changes in flow-abundance relationships pre, post and during the 1987-1993 extended drought -- they found that flow-abundance dynamics were the same pre- & post drought (parallel slopes), but not during the drought period.	4
30	2	862-865	The SSA is too credulous of the conceptual model that posits a longfin smelt step-decline related to step changes in LFS prey items. See my earlier discussion of Nobriga and Rosenfield 2016. A certain reading of Kimmerer 2002 gave rise to a conceptual model that Longfin Smelt declined in a discontinuous fashion "because of the Amur clam's affect on the food web". The evidence for this hypothesis is not strong, even in Kimmerer 2002 (i.e., he did not find a step-decline in most of the prey species he studied that paralleled the step decline in Longfin Smelt and he did not find changes in the flow-abundance relationship for other fish species with similar diets (he actually found an increase in the flow-abundance relationship for American Shad) -- these are necessary conditions for the generalized trophic cascade hypothesis that Kimerer 2002 explored). In fact, Kimmerer 2002 is the only paper I know of that "observes" a step decline coincident with invasion of Amur clam -- that may be because Kimmerer 2002 did not look at other years to determine if the "step decline" actually occurred earlier or later. Using his binary ("before/after") approach, one could find a "step decline" in many years during a protracted population decline. Subsequent analyses that looked at multiple years around this period have not detected a 1987 step-decline in Longfin Smelt (Thomson et al. 2010; Nobriga and Rosenfield 2016). Thomson et al. (2010) found evidence for step declines in 1989 and 1991, but not 1987. They also found no evidence that Longfin Smelt populations were strongly correlated with prey availability (Figure 6 at p. 1442). Mac Nally et al. 2010 found weak, but significant, NEGATIVE associations between LFS and their calanoid copepod prey, as compared to a very strong association with spring X2 (Fig 3a,b at 1425). Furthermore, Maunder et al. 2015 found no evidence for an effect of Eurytemora abundance on LFS; this was the one Longfin prey species that showed a 1987 "step decline" in Kimmerer (2002) -- without a linkage through Eurytemora, there really is no basis for assuming that any Longfin Smelt "step decline" (if there is one at all) is related to the food web, much less the Amur clam's effect on the food web. Furthermore, Mac Nally et al. (2010) looked for but did not find an association of Longfin Smelt with the estuary's invasion by Amur Clams	4
30	2	856-857	"best predictors" ... be careful. Stevens and Miller 1983 looked at a whole matrix of correlations -- it makes sense that the longer the flow averaging period, the better the correlation (more explanatory variables = higher R2). But they did not test which averaging period was best statistically (i.e., which averaging period provided the most information with the fewest variables). I think it's fine to say "highest correlation", but don't say "best".	4
31	2	897-909	re: this text and Figure 2.8, I expect that some parties may object to inclusion of entrainment as a mechanism related to decline of LFS overall or as a mechanism contributing to the flow-abundance relationship. Recent, unpublished, modeling by Kimmerer and Gross notwithstanding, I have not seen evidence that entrainment is not a significant negative impact to this population in at least some years. Although entrainment/salvage of Longfin Smelt juveniles/adults is low in wetter years, this is not true in all years. Entrainment has been high in some years in the 2000's as well. It is possible that entrainment-related mortality has an important impact on Longfin Smelt episodically (Rosenfield 2010). Also, larval entrainment has been estimated and is predicted to be very high under some conditions (e.g., Dry and Critically Dry years when there is no augmentation of outflow by reducing exports), especially given that those are years of low productivity for LFS (see for example, analysis in Chapter 4 (SWP operations without supplemental outflow) the 2020 Final EIR for SWP operations). The SSA should report historical salvage data and place those data in context of population abundance indices at that time. In fact, I think it would help to report the value [salvage divided by prior FMWT indices]. This value was relatively high as recently as 2012 and 2013 and has probably been higher in more the past few years as well. This won't "prove" that entrainment is or is not and important driver of subsequent abundance, but it will at least put salvage into context and demonstrate that (a) it is higher relative to abundance in drier years and (b) it has been historically high even in recent years, despite the 2009 ITP and 2008/2009 + 2019 federal BiOps. Here, the SSA would benefit from reference to recent findings re: the effect of Delta Smelt entrainment. Although there are differences between the susceptibility and magnitude of entrainment-related mortality between these two smelt species, the overall effect may be similar. w/r/t Delta Smelt Smith et al. 2021 recently concluded: "In a population in which recruitment success rates cannot sustain the population, no additional mortality is sustainable . . . No additional mortality can be sustained by the population, but that does not mean that entrainment mortality of 0 will result in its recovery (Smith et al. 2021 at p. 14).	4

Page	Chapter	Line Number	Comment	Reviewer
31	2	926-930	The SSA puts too much weight on food web as the driver of Longfin Smelt decline. While it was "widely believed" that historic changes in food production vs. flow have driven declines in Longfin smelt production vs. flow, the evidence to support this view is not strong. See comments above (e.g., re: lines 862-872). Note also that: 1) E. affinis are prey of larval Longfin. Nobriga and Rosenfield show no change in the flow--> juvenile recruitment relationshiip flow (i.e., no change in the flow-->larval success). Therefore there is little evidence that a step-change in E. affinis translated into a change in the flow-->survival relationship for young (<40mm) longfin. 2) Mac Nally et al 2010 find a negative relationship between copepod abundance and LFS abundance. Thomson et al.2010 finds no effect of density of presumptive prey on LFS density. Maunder et al. 2015 find no evidence of clam or Eurytemora correlation with Longfin Smelt abundance. 3) Kimmerer 2002 shows that mysid's developed a negative flow-abundance relationship in the post-clam years; but this patternwould tend to contradict the ongoing strong, significant, positive correlation between flow and LFS abundance. I'm not saying there is not a relationship between food and Longfin smelt abundance overall, but if such a relationship exists, it does not appear to occur in the early life stages, as "widely believed". If prey abundance has had an effect on Longfin Smelt abundance, it is more likely to be in the juvenile-to-adult transition (where success seems to have declined over time, Nobriga and Rosenfield 2016). The relevant prey items are likely to be those that are located downstream of Suisun Bay, because that's where Longfin seem to spend most of their time as juveniles.	4
31	2	920-924	The effect of Yolo Bypass flooding is speculative and not well supported. Not a lot of evidence that Longfin have access to food produced on the Yolo Bypass. Also, as you note, the bypass historically flooded significantly only in very wet years (i.e., as a flood control measure). But the Longfin Smelt flow-abundance relationship is log:log linear and shows no evidence of a threshold effect (i.e., flooding vs. no flooding of the Yolo Bypass).	4
31	2	898	Should cite Rosenfield 2010, which showed that entrainment decreased as Delta outflow increased, and that entrainment was inversely proportional to LFS abundance indices. (See Figures 9, 11 and 12)	4
32	2	977-983	Glad to see the SSA recognizing the relationship between flow and temperature of LFS habitat. The best available science (much of it recent) contradicts the received wisdom that Delta inflows don't affect Delta temperatures. Several studies show direct effects of flow on temperatures in the Delta -- Vroom et al 2017 (Vroom, J., van der Wegen, M., Martyr-Koller, R. C., & Lucas, L. V. (2017). What determines water temperature dynamics in the San Francisco Bay-Delta system? Water Resources Research, 53, 9901–9921. https://doi.org/10.1002/ 2016WR020062) should be cited as well as Bashevkin and Mahardja (2021; DOI: 10.32942/osf.io/rqbdk) Also, increased FW flows moves the relevant salinity habitat further downstream, where both air and water temperatures are cooler -- this is an indirect, but potentially important effect of flow.	4
32	2	966-974	This paragraph needs to be much more specific re: how flow would affect success of different life stages (ie., the mechanisms for eggs, larvae, juveniles, and adults are different and have differing degrees of support in the literature). The hypothesis that increased inflow increased volume of habitat for juveniles and adults was explored (albeit coarsely), by Kimmerer et al 2009. They found a small change in LFS "preferred habitat" (which was defined by a very limited number of variables), but this change did not match the scale of the abundance response. Kimmerer et al. 2009 concluded that increased habitat space (for young LFS) was unlikely to be the mechanism behind the well-documented Longfin Smelt flow abundance relationship. In general, the mechanisms behind the flow-abundance relationship are unresolved. The SSA needs only to report the uncertainty and status of the science. The relationship between Delta outflow and spawning habitat is unexplored (mostly because the details of LFS microhabitat are little known and unmapped), so this is a possible mechanism. However, all else being equal, available spawning habitat would not be expected to limit an ever-shrinking spawner population. The relationship between flow and larval success seems the most likely explanation of the overall flow--> juvenile recruitment relationship. The SSA should not struggle to resolve the mechanisms behind the flow-abundance relationship to the extent that this has not been resolved in the literature. Unnecessary speculation may bias future research and recovery efforts.	4
32	2	943-962	On the whole, this section is speculative. It's important to focus on the fact that the relationship of flow-->juvenile recruitment has existed, unchanged, since the early 1970's (at least) so any mechanism related to a water quality variable should show a consistent relationship with flow since that time period. Turbidity might play a role in survival of surface-oriented larvae (Thomson et al 2010 find a significant relationship), but given juvenile/adult preference for deeper water (at least during daylight hours), the purported benefits of turbidity is likely to be low for these older life stages. Also, high runoff events can produce increasing amounts/concentrations of toxins, at least initially.	4
32	2	949-952	I'm not familiar with this study, but I question whether the research detects "increasing" use of deepwater habitat or "less frequent use" of shallow water habitat. Rosenfield and Baxter (2007) clearly showed Longfin densities were highest in/over deeper water habitats. Does the study cited here show a change in that pattern?	4
33	2	1010-1014	This same analysis by The Bay Insstitute found the inter-generation change in LFS abundance index (index minus index 2 years earlier) was strongly, and linearly related to Delta outflow (i.e., this is not a threshold effect, but a spectrum between very negative population change and very strong population growth). SWRCB repeated this analysis and used it to identify flow thresholds that are related to population growth. (here: https://www.waterboards.ca.gov/water_issues/programs/peer_review/docs/scientific_basis_phase_ii/201710_bdphaseII_sciencereport.pdf; AT pp 3-53 through 3-55)	4
33	2	997-1001	Speculative. Increased spawning area would only increase juvenile recruitment if spawning area is limiting the population (and limiting it in a continuous way (i.e., each unit of newly available spawning area produces the same increment (in log:log space) of increase in eggs). This seems highly unlikely given (a) the consistency of the flow-->juvenile recruitment relationship in the face of (b) orders of magnitude decline in spawning adults.	4
33	2	994	This is another example of where it would be helpful to refer to specific sizes of larvae -- as noted above and by Hobbs et al (2010); *early* stage larvae are associated with low salinities, but the isotope and catch data show that later-stage larvae move quickly to saltier water.	4
33	2	990-992	Spawning in the South Bay during wet years was suggested by Moyle 2002 and Rosenfield 2010, based on detection of adults and larvae in that region only during wet years.	4
34	2	1027-1032	There are more recent citations to these mechanisms that should be included: 1) Here and throughout the discussion of flow/X2 related mechanisms driving the flow-abundance relationship, you should cite the *other* Kimmerer 2002 paper (Kimmerer, W. J. 2002a. Physical, biological, and management responses to variable freshwater flow in the San Francisco Estuary. Estuaries 25:1275,Âì1290) as he made a good effort to categorize the likely mechanisms. 2) Rosenfield (2010) evaluates most of the mechanisms you discuss here w/r/t Longfin Smelt. 3) SWRCB (2017; Final Scientific Basis report re: Delta outflows --see link above) covers the potential mechanisms driving the flow abundance relationships.	4
34	2	1015	Figure 2.8 is helpful. But, you need to define the difference between larvae and Juvenile. I recommend 40mm as a cutoff, because this is when the Bay Study begins to catch LFS reliably. So defined, the juvenile life stage is more likley to begin in May (or late April) as juveniles are rarely detected in Bay Study nets in April. Furthermore, the larvae are abundant long after juvenile detection begins -- See Rosenfield and Baxter 2007 for the very extended period of increasing juvenile recruitment to the net (through fall) -- this can only happen if substantial numbers of larvae are present long after April.	4
34	2	1022-1025	These lines tend to discount the findings of Vroom et al. 2017; Bashevkin and Mahardja; and Nobriga et al. 2020 (Coldwater fish in a warm water world: Implications for predation of salmon smolts during estuary transit): that flows into the Delta affect Delta temperatures. These papers indicate that there is a detectable flow effect on temperature, which may affect the survival of Delta fish species in the spring.	4

Page	Chapter	Line Number	Comment	Reviewer
35	3	1061-1062	This statement is unnecessary (you are discussing stressors here, not management actions) and may not be true. You should strike it. 1) Salvage relative to previous abundance indices has been historically high in several years following adoption of the 2009 ITP. See water years 2012, 2013, 2020 for example -- you should present salvage data relative to LFS abundance indices. 2) The management regime has just changed -- the 2008-2009 BiOps and the 2009 ITP are no longer in place, so any beneficial effect they had for Longfin may no longer be relevant. 3) re: Current ITP. "Refined Alt. 2b" of SWP operations (i.e., the preferred alternative reviewed in the Final EIR) indicates that much higher salvage of juvenile Longfin is anticipated in April-May of Wet, Above Normal, and Below normal year types (see Figure 5.3-44 at page 5-142 of the Final (2020) SWP DEIR, and Table 5.3-12 of the same document) 4) Although the SWP preferred alternative calls for April-May outflow augmentation, which is expected to reduce LFS entrainment in Critical Years, Delta outflow requirements for critical years were not attained in April 2021 (a violation of D-1641) and DWR and Reclamation have proposed to waive these requirements (as part of a temporary urgency change package) in 2022, regardless of water year type. In other words the "protections" said to be provided by the state's ITP have not been implemented, so they cannot be deemed effective. 5) The SWP EIR for Refined Alternative 2b does not analyze effects of larval longfin smelt entrainment in April or May, despite the high susceptibility of larval entrainment-related mortality (direct and indirect) in those months. again, augmented Delta outflow in April and May is assumed to reduce larval LFS entrainment effects, but this is not analyzed and the flow augmentation has not occurred and it is not reasonable to expect that it will occur in 2022 or beyond.	4
35	2	1037-1039	This sentence understates the effect of freshwater flow on the SFE Longfin Smelt population currently. For a given increase in flow, the proportional effect on the longfin smelt population has not changed. All analyses show that the slope of the flow(or X2) abundance relationship has not changed (Kimmerer 2002, Kimmerer et al. 2009; Rosenfield and Baxter 2007; Thomson et al. 2010). The slope shows the effect of changing flow (not the intercept, which may have changed). As discussed at length above, Nobriga and Rosenfield (2016) unpack the flow abundance relationship and show that it is driven by a relationship between flow and juvenile recruitment -- this relationship has not changed in 50 years.	4
36	3	1110-1120	The Final EIR for the SWP operations (2020) finds that LFS abundance is expected to decline 1-3% in all year types see Table 5.3-9 at p. 5-136. (the final column on the right is the result of an invalid statistical manipulation). The methodology applied is invalid and likely underestimates the negative effects of increased water exports permitted by both the SWP and CVP under the ITP. For instance, the method applied to arrive at the estimate of a 1-3% decline does not account at all for large increases in entrainment estimated by the EIR to occur in Wet, Above Normal, and Below Normal years. The ITP's proposed augmentation of outflow by reducing exports is not the same as the I:E ratio -- the former regulates monthly average outflows, but this may allow impacts that would not occur under the former I:E ratio requirement, which operated at a more granular timestep. Also, the flow augmentation did not occur in 2020 and the Projects are petitioning to waive Delta outflow requirements for April -- thus, any benefits of this provision of the ITP are not reasonably likely to occur in the future. At best, the ITP projects "no significant effect" of SWP operations on Longfin Smelt -- meaning, no change from the status quo. But the status quo for Longfin Smelt (and other native Delta species) is decline -- so even "maintaining the status quo" is likely to result in continued decline and increasing endangerment of Longfin Smelt.	4
36	3	1091-1093	There are more recent analyses of the effect of diversions (including but not limited to Delta exports), that are particular to the winter-spring flows that affect the Longfin Smelt population. 1) TBI (2016) Shows an average of 53% of winter-spring UIF is diverted/stored upstream (https://bayecotarium.org/wp-content/uploads/freshwater_report.pdf see pp. 10-13 (26-29 of the PDF) Note: the red numbers on the graph on p. 13 (p. 29 of the PDF) indicate the % of unimpaired flow that made it through the Delta as actual Delta outflow. 2) Reis et al. 2019 concludes that "more than half of winter-spring unimpaired flow is diverted or stored before it becomes actual outflow. (See Figure 3 (bottom panel). [https://escholarship.org/content/qt8mh3r97j/qt8mh3r97j.pdf] 3)Hutton et al. 2017. (this study disaggregates flow modification into individual months) Hydrol Process 31:2500,Äì 2515. https://doi.org/10.1002/hyp.11201 4) Gartrell et al. 2017. [Here: https://www.ppic.org/publication/a-new-approach-toaccounting-for-environmental-water-insights-from-thesacramento-san-joaquin-delta/]. But see the critique of Gartrell et al's interpretation of flow modification by Reis et al. 2019 (at p. 17).	4
36	3	1073	The implication that demand for water is driven by California's population growth is only true at the coarsest scale (e.g., centuries). Municipal per capita water use demand has dropped substantially since the 1980's such that cities like Los Angeles use the same or less water now than they did decades ago, despite substantial population growth.	4
37	3	1122-1137	This material needs to be presented, but the presentation needs to be significantly reworked. BACKGROUND: DWR presented analyses of SWP effects on Longfin Smelt in its EIR for the State Water Project Operations Plan. These included results from a computer code that was said to apply the Nobriga and Rosenfield 2016 model in order to compare effects of water management scenarios on Longfin Smelt. Comparing management alternatives was not the intended purpose of Nobriga and Rosenfield's model and several aspects of that model (including the assumption that density dependence is Ricker-like) could produce spurious results when used to contrast different flow volumes. More importantly, DWR's computer code did not properly analyze alternatives by comparing Longfin Smelt performance within different model-runs (i.e., such that differences between alternatives are related only to the alternatives, not to randomization of other model elements). Instead, DWR's analysis lumped all the variation in Longfin Smelt outcomes (including a 2-3 order of magntidue decline through time) into year-type bins (each of which represents huge variations in flow) -- and then concluded that changes in population under different alternatives were "small" relative to the high variation in the modeled population. This comparison is invalid and misleading. In fact, the underlying N&R (2016) model will predict higher LFS populations when flow is higher -- the result will be extremely consistent because flow is the driving variable in their model (for reasons that are well-documented in their paper). Rosenfield and Lewis did NOT conduct new modeling (as implied here). They simply identified fundamental flaws in the DWR modeling effort (such as compounding of non-flow related variances) and showed that, once some of the extraneous, non-flow related variance was eliminated, the original (DWR EIR) model outputs actually revealed disproportionately large changes in the LFS population relative to modeled changes in flow. Clearly, the SSA cannot ignore these findings because the DWR analysis is part of the (flawed) foundation for SWP ITP.This same analysis has popped up elsewhere (i.e., in the EIR for Sites reservoir) and it is likely to be put forward in comments regarding the SSA. But the SSA should not imply that Rosenfield and Lewis produced the computer code that (incorrectly) implements the N&R 2016 model. Lewis and Rosenfield showed, in a presentation to the Core Team, that the DWR model is seriously flawed in that it fails to actually compare the effect of flow alternatives in a statistically valid way (paired comparisons) and that it erroneously claims that the effect of increased Delta outflow on LFS is "uncertain". The Nobriga and Rosenfield model is not at all uncertain about the effects of increasing Delta outflow.	4
39	3	1170-1179	The discussion of drier hydrology over the past 20 years is interesting. But several studies have shown no statistical change in unimpaired flows over the past several decades (see for example Hutton et al. 2017 -- variance has certainly increased). However, there is a very strong trend of declining *actual* Delta outflow as a proportion of unimpaired flow -- this can be seen as a statistically significant decline in the %UIF reaching the Bay (Reis et al. 2019 Figure 3) and as an increased frequency of Critically Dry and Dry years (in terms of actual outflow) that is disproportionate to the change in frequency of year types in terms of unimpaired flow (Reis et al. 2019 Table 5).	4
40	3	1220-1222	Kimmerer 2002; Rosenfield and Baxter 2007; Kimmerer et al 2009; Mac Nally et al. 2010; Maunder et al. 2015; Nobriga and Rosenfield 2016	4
40	3	1202-1204	Grimaldo et al. 2009 & Rosenfield 2010.	4
40	3	1200-1202	This point also made by Rosenfield 2010.	4
40	3	1189-1196	I understand the point you are trying to make here -- population growth frequency has to at least match the life-spawn of the organism, otherwise the long-term time trend will continue. But, Delta outflows (and thus conditions that affect Longfin Smelt productivity) are not entirely dependent on something as coarse as water-year type. Humans have a good deal of control over Delta hydrological conditions that might affect LFS population growth -- water diversions+ storage account for almost half of Central Valley unimpaired flow in an average year (the drier the year, the higher this percentage; Reis et al. 2019; SWRCB 2017 scientific basis report; Hutton et al. 2017).	4
41	3	1224	Recommend unpacking this a little. Natural hydrology in CA is so extremely variable that it is sometimes difficult to detect trends. Freshwater flow has declined because of (a) natural hydrology (more frequent prolonged droughts) and (b) increasing diversion of the water that is available water (reducing the % of unimpaired flow that makes it to outflow). For (b), you should cite Reis et al. 2016 and Hutton et al. 2017.	4

Page	Chapter	Line Number	Comment	Reviewer
41	3	1247-1303	The stressor (if it is one) is the food supply, not the clams. Amur clams are just one potential mechanism for reduced food supply (e.g., decreased flows, increased competition, are others). This is a lot of text (too much) to dedicate to the food supply issue, in general, and the clam's putative role, in particular. As described at length above, there is not a lot of evidence that changes in food supply are driving declines in Longfin Smelt (at least not in the earliest life stages) and there's less evidence of a trophic cascade that affects Longfin Smelt stemming from the invasions of filter-feeding clams. Basically, you have Kimmerer (2002) making the inference, without testing it in a rigorous fashion -- and no one since then has found strong evidence of a Longfin Smelt step change in 1987 (not Thomson et al. 2010; not Mac Nally et al. 2010; not Nobriga and Rosenfield 2016; not Maunder et al. 2015), or an effect of clams on Longfin Smelt abdundance (Mac Nally et al 2010 and Maunder et al. 2015 looked but did not find a relationship), or a *positive* relationship between Longfin Smelt abundance and density of their prey (Mac Nally et al 2010 found negative relationships; Thomson et al. 2010 and Maunder et al 2015 find no relationship with prey items).	4
41	3	1238-1239	These terms need to be defined more clearly at the outset of the paper (perhaps a map to show the boundaries for the different geographic terms). In my book, West Delta and Susiun Bay are part of the San Francisco Estuary. All of the Delta, all of the bay, and parts of the Gulf of the Farallones are part of the San Francisco estuary.	4
41	3	1230-1232	I encourage you to frame this in terms of actual flow conditions rather than water year type. Presumably, these historically very abundant, widespread fishes did well in most years, pre-water development. What we know --what the data show-- is that they respond to actual flows; water year type is our construction and it's unlikely that LFS respond to such an abstraction.	4
41	3	1220-1228	This paragraph should be much higher up in this section (or perhaps Section 2). In general, I encourage the authors to focus on opening paragraphs with strong and clear topic sentences and opening sections with clear introductory paragraphs. Throughout the report, I find that the main point is buried in the middle of a paragraph and the introductory paragraph is located in the middle of the section. Simply reorganizing sentences/paragraphs will make for a more powerful presentation of the information already in the report.	4
43	3	1319-1321	Speculative. This assumes that food availability (for LFS larvae and early juveniles) is the variable driving the long term population decline. And this is starting to get circular. First you said --> food is declining, and that's what's driving the Longfin Smelt decline (see above re: how there's not so much evidence for that). Now you're saying, Longfin Smelt declined with increases in X2, so their food must depend on X2 position. Mac Nally et al. 2010, figure 3 shows Spring X2 is strongly and negatively correlated with spring copepods and with Longfin Smelt abundance. Longfin Smelt abundance is NEGATIVELY correlated with copepod abuidance. Also, Thomson et al. (2010) -- which included most of the same researchers, found no significant association between Longfin Smelt and food web indicators. Also, Maunder et al. 2015 find no support for a Eurytemora connection.	4
43	3	1310	"lowest on record" *at that time*	4
44	3	1359-1382	This paragraph is summarized by the last sentence. Much of the rest of the paragraph is hand-waiving -- we don't know that they've adapted to high temperatures in the Delta, migration seems to be part of their strategy elsewhere and may have nothing to do with temps. For the same reason, we don't know that they have the ability to adapt to higher temperature. what we know is (a) they have temperature limits and (b) temperatures are expected to rise. (a) + (b) = the spawning/incubation season of LFS may be constrained by temperature now and may be increasingly constrained in the future.	4
44	3	1327-1329	This (consistent slopes, but fewer LFS for a given X2) is a common, but very weird way of presenting these results. One would only be surprised by the change in intercept if one expected that the movement of water is the force creating Longfin smelt. If one thinks of flow as mediating survival between one cohort of LFS and the next, then what is important is that the slope of this relationship has not changed. For any given change in log-Delta outflow (or X2) the relative change in the Longfin Smelt population is the same now as it ever was. The change in intercept is (almost by definition) not flow related. This has been confirmed by Nobriga and Rosenfield (2016), who found no flow affect on juvenile-->adult survival (also see Rosenfield and baxter 2007).	4
45	3	1406-1408	The statement that LFS require "sandy shorelines" for spawning contradicts the premise of this paragraph, which is about "marshes". "Marshes" evokes vegetated shorelines. Yes, marshes may have sandy shorelines, but so do ... beaches.	4
45	3	1403-1406	The presence of larval Longfin Smelt in marshes does not demonstrate "the importance of these former inter-tidal wetlands". I recommend "suggests the possibility". For example, no one argues that Suisun Marsh is good habitat for adult Longfin Smelt -- however, Moyle's UC Davis marsh surveys finds some Longfin Smelt adults and juveniles in Suisun *when they are abundant throughout the estuary*. Presence does not equal persistence.	4
46	3	1386-1423	This section is weak. Available tidal marsh habitat does not vary on a year-by-year basis in a way that tracks LFS populations. Nor have there been "step-declines" in this habitat corresponding to major declines the lthe early 1990's and early 2000's,(as per Thomson et al. 2010). If anything, tidal marsh habitat has increased in the last several decades -- but there has been no hint of a change in the Longfin Smelt population dynamics that would support a hypothesis that they benefit from tidal marshes. I'm not saying tidal marshes weren't important historically or that restoration at a large scale now would not be helpful now, but the value to Longfin Smelt currenty of restoring this habitat is not at all certain. The SSA must acknowledge significant uncertainties in the relationship between tidal marsh availability and LFS abundance and the completely unknown effect of habitat "restoration" as mitigation for other negative effects. Although recent research has documented Longfin Smelt occurrence in marshes outside of the Delta-Suisun Bay region, there is no evidence that Longfin Smelt detected in these areas contribute to the adult population. Results of a preliminary otolith chemistry fingerprinting study concluded, "...of the adult fish that were classified with moderate confidence (e.g., 75%), nearly all appeared to have reared in the northern SFE ..." (Lewis, L., A. Barros, M. Willmes, C. Denney, C. Parker, M. Bisson, J. Hobbs, A. Finger, G. Auringer, A. Benjamin. 2019b. Interdisciplinary Studies on Longfin Smelt in the San Francisco Estuary. 2018-19 Annual Report for DWR Contract %23 4600011196. Prepared for: California Department of Water Resources & IEP Longfin Smelt Technical Team at p. 9 and Figures 17 and 18 at p. 75 of the PDF). It is not clear that Longfin Smelt found in shallow tidal habitats downstream of Suisun Bay originated in those habitats or reproduce successfully as a result of those habitats. For example, although researchers have detected substantial numbers of Longfin Smelt west of Suisun Bay, this occurred primarily during the exceedingly wet years 2017 and 2019 (Lewis et al. 2019). Even then it was not clear that the fish detected were produced in local marshes; Lewis et al. stated: ,"... it is valuable to consider whether, with high Delta outflows, it is feasible and probable that larval and juvenile Longfin Smelt found in high numbers in San Pablo Bay, and even Lower South San Francisco Bay, could have been transported from Delta and Suisun Bay spawning sites by currents, tides, and winds." (Lewis et al. 2019 at p. 6). Although researchers have caught pre-reproductive adult and larval Longfin Smelt shallow tidal habitats downstream of Suisun Bay and the Delta, they were circumspect regarding the importance of spawning and rearing in these habitats, stating that their value ,"remains unknown." (Lewis et al. 2019 at p. 2; see also at p. 6).	4
46	3	1412-1419	I agree that sandy (or even rocky) sub-tidal areas are probably important for spawning. But 1) we don't know that spawning habitat is limiting, 2) is there evidence of declines in this habitat that track empirical LFS population dynamics?, 3) you are arguing that SAV may make sandy habitat less valuable for spawning. But you're also arguing that "marsh" habitat is good for spawning when marshes are characterized by aquatic vegetation. And, 4) you're further arguing that SAV filtering out turbidity may reduce survivability (presumably) of larval LFS -- but marshes are generally depositional environments -- filtering out turbidity is what they do (in general). You'll need to rectify your comments which suggest that marsh restoration is good for Longfin Smelt with these other facts.	4
47	3	1487-1502	This is the best paragraph of the section on Predation and the only one you really need. I recommend starting this section with the sentence at 1435-1437 (re: speculative), then add this paragraph and then add a reference to Moyle 2002, which suggests that egg predation by Menidia (which occur in droves along beach lines) might be responsible for smelt declines (again, speculative, but at least it's a published speculation).	4
48	3	1524	data "are"	4
49	3	1547	There is no agriculture *in* Suisun Bay -- I think you mean "around the margins of"	4
49	3	1545-1546	I don't understand -- are you saying that ocean currents carry contaminants from northern California to the Gulf of the Farallones? This kind of speculation without documentation detracts from the strength of the SSA.	4
50	3	1556-1595	Correlations between ammonium concentration (particularly, from wastewater treatment plants) and Delta smelt abundance have been questioned on methodological grounds (Cloern et al. (2012 Limnol. Oceanogr., 57(2), 2012, 665,Äi668). Also, the pattern doesn't hold up under scrutiny -- see Cloern 2021 here: https://escholarship.org/uc/item/1xz922jm (" Synchronous increases of chlorophyll and NH4 over the past 3 decades are not consistent with the ammonium suppression hypothesis....These five results are all consistent with an alternative hypothesis that the chlorophyll decline in Suisun Bay was largely a result of increased grazing losses to an introduced filter feeder and was unrelated to ammonium suppression of growth.")	4
51	3	1664-1666	You just said that larval fish salvage was unmeasured. So, you'll need to caveat this statement to say salvage of Age-0 is "believed to be" related to larval population size and larval entrainment.	4
51	3	1636-1645	You're leaving it open for the reader to speculate/guess as to whether this is a potential major stressor on the population. The scale of exposure x diversion volume seems seriously mismatched. Fish may die here, but it's extraordinarily unlikely that this has a had a major effect on LFS in the SFE historically.	4
51	3	1620-1634	Here and throughout Section 3.1 (and throughout the SSA, in general), I recommend focusing on topic sentences. W/R/T to agricultural diversions, you conclude that they are of little importance to LFS (agreed), so why not just say that up front?	4
52	3	1693-1694	Is there any evidence that any Longfin Smelt entrained in Clifton Court Forebay can/do escape?	4
52	3	1668-1672	This sentence is awkward; there are too many thoughts here and they are not completed.	4

Page	Chapter	Line Number	Comment	Reviewer
53	3	1741-1743	This is speculative -- you haven't analyzed the potential effect of direct entrainment-related mortality. The impact of entrainment-related mortality is related to the size of the overall population. Therefore, I recommend comparing estimated entrainment of longfin smelt in any year to the FMWT (or Bay Study) abundance index in either the previous fall (estimates size of Age 0+ population) or two years prior (estimates size of Age 1+ population -- or use an average of abundance indices from one and two years prior. This is likely to reveal historically high relative entrainment in 2012, 2013, and 2020.	4
53	3	1729-1731	You need to explain pre-screen mortality up front, not introduce it, somewhat apologetically, at the end of a paragraph. I would describe what is known about the relationship between salvage and direct entrainment-related mortality for other species. Just because studies of pre-screen mortality using Longfin Smelt haven't been condicted does not mean we are not reasonably certain that the same relationship (orders of magnitude more pre-screen moprtality than actual salvage) holds qualitatively for Longfin Smelt. Correction/expansion ofLFS salvage to account for pre-screen mortality may be lower magnitude than it is for salmon and Delta Smelt or it could be higher magnitude, but it's not zero and it's almost certainty significant relative to salvage alone. Also, so far you have not mentioned indirect effects of entrainment (i.e., reduced survival for fish outside of Clifton Court forebay), although this is believed to be one of the largest effects of exports on migrating salmon and Delta Smelt. See for example, SWRCB 2017 at p. 3-47 citing the U.S. Department of Interior re: salmon entrainment-related mortality: ,ÄúMore important than direct entrainment effects, however, may be the indirect effects caused by export operations increasing the amount of time salmon spend in channelized habitats where predation is high (USDOI 2010, p. 29).,Äù	4
55	3	1791	You refer to Figure 3.5, but I think you mean to refer to figure 3.6? Also, the caption for Figure 3.6 does not describe the inset. Also, are there more recent data available? Finally, given you state that the 20mm survey may be an underestimate in wet years, I think you should identify those years on the graph (e.g., 2011, 2017, 2019)	4
56	3	1815	See comments re: previous sampling programs. Align Figure numbers with the text. Consider using log scale for the y-axis (or use the inset for that purpose).	4
56	3	1798	Similar comment as for 20mm survey. I think you're refering to the wrong figure in the text (doesn't match the number of the actual FMWT figure). And, you need to describe the inset figure in the caption and use the most recent data available. I also recommend converting the y-axis to a log scale in order to capture the fluctuations over the past 2 decades.	4
58	3	1827	Figure 3.10 (the flow-Log(abundance) step change figure) is not referenced in the text and I don't think it's appropriate in this section. This figure is not as relevant to the question: "has the longfin smelt population declined?" as it should be, because it confounds changes in the intercept of the flow-abudnance relationship (see my comments above) with declines in the population through time. Also, the caption states that there are two "known" step declines. (a) as described elsewhere, the step decline associated with 1987 is not well supported and (b) it is questionable whether describing the population dynamics by invoking a series of "step declines" is statistically valid or reflective of actual ecological processes. It is certainly not a helpful frame for considering listing or subsequent management of this species. Again, see Rosenfield and Nobriga re: the consistent relationship between flow and juvenile recruitment -- they disaggregated the Longfin decline into two vectors and showed that different forces were probably responsible for each vector.	4
145	TN 3	4713-4714	Need to rewrite this sentence: "Fish of all ages and sizes have been caught in the San Francisco Estuary". In context, I can infer what you mean (but it's open to misinterpretation). As a standalone statement, it makes no sense.	4
162	TN3	5170-5180	You need to dig into the assumption that you are catching larger fish proportional to their existence in the population. This is not a closed population and the sampling programs do not cover the entire distribution of these fish during many months. As described in comments above, differential migration based on body size (faster migration out of the sampling zone for large fish in spring, slower migration back into the sampling zone for smaller fish in fall) would make the proposed method of measuring growth (delta in size between months) untenable.	4
162	TN 3	5157-5159	Rather than assuming that size-at-hatch is consistent, you might want to evaluate whether your estimate of size at age-0 covaries with temperature during the assumed incubation season (e.g., temperature at Chipps or Fremont). Hatching size/condition is generally affected by (and inversely related) to temperature. But I don't think that this analysis is needed in the SSA -- I would suggest setting aside this entire Technical Note.	4
165	TN3	5276-5285	Here again, the analysis of growth fails to account for migration of the fish into ever saltier (and likely colder) water. I sense that "growth" itself is being used as an indicator of "success" or "fitness" of individuals. But, as noted by your selection of covariates, temperature and salinity affect growth rate -- all our studies show that these fish migrate to colder, saltier water as juveniles -- thus, we would expect growth to decline just about where you see it declining (age 6-7 months) because the fish are living in a different world (marine), not because their physiology shifts to produce intrinsically slower growth at ~6 months. If, in addition, fish catches in the summer disproportionately represent the relatively few longfin that remain in the hot and (now) salty Suisun Bay/Delta region, then your "growth" estimate is going to be skewed low.	4
158-174	TN3		This analysis is overly complicated and (as described elsewhere in these comments) may not be valid, unless and until the author can demonstrate that Longfin Smelt migration into and out of warmer/colder marine/brackish habitats is not size-based in a way that affects model outputs and interpretation. Also, "outflow" does not seem like a relevant variable for somatic growth because it would have its effect through temperature or salinity, which are already accounted for. Any effect of flow on growth through the foodweb, should be assessed using prey data (which are available). In the end, I just don't see this analysis as being relevant to the SSA's core purpose; nor is it suffiiciently explained or supported. Outflow may affect Longfin growth (through effects on fish distribution with respect to salinity, temperature, food, etc), but you have not established that "growth" represents "success" in the same way each year -- slow growth in cold water with abundant food may produce better survival/fecundity than faster growth in a warmer environment (see Bayliss' work on Centrarchid growth patterns for more on why "slow" growth is not always the same as "poor performance"). And, as mentioned, the temperature/salinity metric used here is not very helpful because it is not measured where the fish are, as they migrate from month to month (this could be addressed by using salinity/temeprature measures from the sampling programs that are weighted by fish catch density -- but even that assumes that the temperature/salinity where you catch the fish represents salinity and temperature they experienced in the previous month, when the growth occurred).	4
58	3	1821-1825	These lines should be the topic sentence for section 3.2 "current conditions". The point, which is well made by the graphs that follow, is that LFS abundance has declined A LOT and that this decline is apparent in all of the sampling programs. Rosenfield and Baxter (2007) showed the decline for the Suisun Marsh Survey as well. The consistency in the trend seen among the sampling programs is not simply confirming the overall pattern of decline -- each one demonstrates that the decline is detectable in a different part of their habitat and/or a different part of the fish's life cycle.	4
58	3	1834	It's not clear to me whether you used all the data in each data set, or restricted the analysis to more recent years (e.g., you used the 2009-2018 average to establish the quasi-extinction threshold; I understand why you would truncate the data for that estimation, but you don't describe the time series for the calculation of lamdas) Also, you describe mean lamda in terms of the 10-year growth rate (makes sense) and this may also lead to confusion as to the temporal exetent of the lambdas you calculated. Please describe.	4
60	3	1903-1904	This result is sure to generate confusion. It merits a little explanation of how the lamdas can be positive when the abudnance trend depicted in the relavant figure for the Bay Study's Otter Trawl (above) clearly shows a decline.	4
60	3	1895-1900	If you are trying to answer one question ("is the Longfin Smelt population declining?") using different (and somewhat non-independent data sets), it seems like you might be able to do a metanalysis of these results. What are the changes the 7 of 8 measures of the population show decline, if the population is not actually declining?	4
62	3	1921-1924	Again, the "step-decline" argument is not that strong. But even if there is a step-decline(s), Thomson et al. 2010 does not provide evidence that this is linked to the clam invasion (and neither does Nobriga and Rosenfield 2016). Sommer et al. 2007 is a description of the P.O.D. and exploration of potential relatoinships, but it does not isolate a particular change in "environmental condition" (that's an extremely broad term, so some kind of "change in environmental condition" is strongly implied -- but this sentence makes it sound like we know what's changed, and we don't). As described above, Rosenfield and Nobriga (2016) found evidence for a continuous decline in juvenile survival (their "year" variable) that was as strong as the step-decline variable.	4
62	3	1927-1930	I think this can be stated more clearly.	4
62	3	1940-1941	If I'm reading this correctly, improving population status and environmental conditions *quickly* is imperative.If so, I think you should emphasize that point.	4
63	3	1962	"… constrained options for impaired fitness…"= awkward. Please rephrase.	4
63	3	1965-1975	This description is clear and concise and the conclusion is strongly supported by the best available science (new and old) on this population. I encourage the SSA team to make the rest of the SSA as clear as this.	4
63	3	1977-1984	See above. Evidence for trophic cascades driving Longfin Smelt abundance is (a) not contemperaneous with the clam introduction and spread (Thomson et al. 2010; Nobriga and Rosenfield 2016) and (b) not strongly linked to the larval life stage (Mac Nally et al. 2010 (showing a linking to copepods that is significant, but weaker than the X2 relationship); Rosenfield and Baxter + Nobriga and Rosenfield 2016 (which show a decline success from juvenile--> adult, and consistent relationship between flow and juvenile productivity). Furthermore, Maunder et al. (2015) found that "... after including a flow covariate, support for Eurytemora [as a predictor of LFS] is lost and it is not selected in any of the final models" (at p. 107); this is significant because Eurytemora was the one Longfin Smelt food item to show a 1987 "step-decline" and thiswas Kimmerer's basis for concluding that a trophic cascade explained the Longfin Smelt's "step decline". If LFS are not tracking spring eurytemora populations, then the clam-->eurytemora-->longfin trophic cascade has even less support. It is possible that food web effects contribute to the decline in juvenile--> adult survival, but this would implicate different food resources (e.g., Mysids) and perhaps a different habitat (deep water and closer to the Pacific ocean).	4

Page	Chapter	Line Number	Comment	Reviewer
63	3	1986-1992	The connection between tidal marshes, longfin smelt, and decline of both needs to be better supported. If the marshes disappeared a century ago, we don't know what effect that had on Longfin. If we had information showing that tidal marshes are critical to Longfin today, that would be helpful, but I haven't seen that information presented here. We have emerging information about their presence in and use of tidal marsh habitats, but we don't know if those habitats are source/sink or better/worse than non-tidal marsh shallow habitats. Also, the comment about maybe there's not enough tidal marsh habitat to sustain them (a) assumes they *need* tidal marsh habitats and (b) ignores that populations were 2-3 orders of magnitude higher in the 70's and early 1980's and, if anything, there has been an increase in marsh coverage since that time. Also, you need to explain better how tidal marshes benefit these fish but vegetated shallow areas are not good for them. I'm not saying your wrong, but you need to provide better support for this claim.	4
63	3	1997	I think you mean "susceptible" rather than "prone"	4
64	3	2004	"Step decline" is not a potential driver of the population (i.e., not parallel to temeprature, food, flow and the other headlines in this section). I recommend calling this "The POD" and simply explain that multiple fish display a marked decline in abundance in the same year (Thomson et al. 2010; Sommer et al. 2007) and that the reason(s) behind this/these declines are unresolved. Basically -- we don't know what happened here. That's as clear and factual as you can be, spending more time on it does not help the SSA.	4
64	3	2010-2015	You have not provided any data or analysis of entrainment mortality. The text here doesn't seem to me to be incorrect, but it lacks support in the SSA. As described above, entrainment relative to prior FMWT index was relatively high in 2012 and 2013 (depending on how you measure "prior abundance" -- high relative to the period before the 2008/2009 BiOps and 2009 ITP. I'm fairly certain 2020 would show similarly large relative entrainment. More imporantly --> the regulations that you're assuming limit Longfin Smelt entrainment are no longer governing the system. And, the Projects continue to apply for and recieve Temporary Urgency Changes to water quality standards in Dry and Critical Years. This means that the position of Longfin Smelt spawning and the larval cohort are probably located closer to the export pumps than they were under the intended implementation of the 2008/2009 regime (i.e., without TUC Orders). So, I would not be so quick to write off the potential effect of entrainment on Longfin Smelt in the future.	4
65	3	2016-2017	I like this table, but (as described at length above) there is little/no evidence of a clam effect -- call it "extremely uncertain", at least. For larvae, there is little evidence of a food web effect that is not flow-related (i.e., increased flow leads to increased prey, just as it always has). Any food web--> larval LFS relationship does not appear to have changed in the past 50 years (and, again, no one that I know of who has looked for a relationship with LFS abundance and prey abundance has found such a relationship. There is some evidence of a food web effect on juveniles (i.e., the major decline in Mysid populations and the consistent flow related effect on Crangon populations; Kimmerer 2002) given the potentially (?) discontinuous decline in survival between juvenile--> adults. The "tidal habitat loss" may be a "chronic constraint", but there's not a lot of evidence that it is a constraint (see above) and the same "constraint" was present when Longfin populations were much higher). I would code this as a big ucnertainty.	4
61	3	1912-1916	This is a compelling figure -- clearly demonstrates that this population is at a high risk of extinction and deserves to be listed as ENDANGERED (as opposed to "threatened"). For example, each line shows >20% of quasi-extinction in less than 20 years. Lindley et al (2007) characterize this as a "High" risk of extinction, as would any raional fish population biologist. The SSA should cite Lindley, Steven, T. et al. Framework for Assessing Viability of Threatened and Endangered Chinook Salmon and Steelhead in The Sacramento-San Joaquin Basin. Vol. 5, Issue 1 [February 2007]. Article 4. http://repositories.cdlib.org/jmie/sfews/vol5/iss1/art4 --and related papers.	4
67	4	2086-2089	This sounds like: we've tried everything and nothing has worked. In fact, there has been remarkably little done to improve conditions for Longfin Smelt in any meaningful fashion. You need to be clear re: how frequently and aggressively these "protective measures" have been implemented. The export constraint mechanism of the 2009 ITP was implemented less than a handful of times (maybe 2x?) in it's 10 year history. Has the "export constraint to augment April May flow" provision of the current ITP ever been implemented? By contrast, in how many years have outflow provisions of the Water Quality Control plan been dramatically weakened via TUC Orders (2014, 2015, 2021, at least) or the outflow standards violated (2021, at least).	4
67	4	2091	Sorry to be redundant, but you keep claiming that the clam did significant harm to Longfin Smelt. There simply is not evidence to back this claim, despite multiple efforts looking for that connection. (see above)	4
69	4	2176-2185	The first half of the first sentence is correct -- the effects are uncertain. The "theorizing" about those impacts that happens in the rest of this paragraph is highly speculative (assuming mechanisms in an absence of evidence or analysis), not comprehensive of the possibilities, and not that helpful. I recommend minimizing this type of brainstorming as it fosters misinterpretation and, potentially, misdirection of efforts to protect these fish.	4
70	4	2214	Need to be consistent in your description of threshold temperatures for LFS spawning -- if there is uncertainty, say so. See your description at lines 740-742, and elsewhere. Also, the SSA should acknowledge the challenges of translating laboratory results to field results. There are several examples among Central Valley salmonids (winter-run Chinook Salmon incubation temperatures and Chinook Salmon and Steelhead juvenile temperature thresholds, come to mind) where laboratory studies created an inaccurate impression of temperature tolerance in the field.	4
70	4	2201-2219	This discussion needs to incorporate the temporal and spatial distribution of Longfin Smelt. Are those decadal increases of 2-3oC estimates of average temperature across the entire year? If so, what about the months when large numbers of Longfin Smelt juveniles are found in the Delta? Also, for all the SSA's focus on food as a driver of Longfin populations, I am surprised that there is not mention here (or in previous discussions of temperature) re: the effect of food on temperature thresholds. This is well known among Salmon (ie., temperature tolerance, which is generally around 20oC, increases to up to 25oC in the presence of ad libidum high quality prey -- that's why salmon juveniles are so successful on inundated floodplains, despite the high temperatures.	4
70	4	2221-2223	Please describe here why you think increased salinity intrusion will harm Longfin Smelt habitat? Yes, they are connected with "X2", which will shift east. But what if that correlation simply reflects a mechanism driven by outflow (e.g., transport/retention dynamics)? Also, recent laboratory studies (presented to the Core team by Lewis at the SSA symposium) indicate that Longfin Smelt eggs and larvae are more salinity tolerant than we previously undertsood. I don't necessarily think this is wrong, but you need to be explicit -- the SSA will form the basis of understanding for many decision-makers, you need to explain what is certain vs what is suspected, bu uncertain vs. what is just speculation.	4
73	4	2318-2320	The SSA continually refers to the "weak swimming ability" of Longfin Smelt as if that is an important limit on their ability to disperse or move down or upstream. This may be true, but how they behave with respect to currents (e.g., vertical migration to ride currents selectively) is probably more important than raw swimming ability. I think the SSA should acknowledge that and reference research on other fish species (e.g., Delta smelt) that are believed to use currents to facilitate migrations/dispersal.	4
73	4	2304-2327	The number of days of inhospitible temperatures is an interesting stat -- but I think it would be more relevant to the effect you are identifying to show how the window of suitable temperature conditions changes under climate change. For instance, it's not necessarily important if more days become inhospitable in August if the fish were already excluded from this area because of warm water temperatures in June (this affects interpretation of the numbers presented in lines 2366-2367, for instance). Is it possible to show the change in the span of days from first day over 20oC (or 22oC, or 7-day average>20oC -- doesn't matter) to last day over 20oC. This is just a suggestion- -- I would not delay finalization of the SSA to implement this suggestion -- but, I see that you did an analysis exactly like this for spawning in Figure 4.7. Given this, I think you can eliminate the graphcis and text that assess change in total number of days with temeperatures between x and y. The change in the window of days between onset of critical temperatures and end of critical temperatures is more important.	4
76	4	2350-2362	I like the intent behind this analysis and the graphical depiction in 4.7. Assuming your method for back-calculating spawning date is accurate, the figure suggests to me that (a)onset of spawning is not triggered by availability of temperatures ≤14oC and (b) spawning doesn't end when temperatures exceed 14oC (i.e., the grey bars extend beyond the orange line, sometimes for many days in about 1/2 of years) -- also, to the extent that these spawning windows are constructed based on catches of larvae, you are measuring spawning success here, not just spawning at all.	4
78	4	2384-2386	Speculative. *If* the fish have multiple clutches, that does not mean that a shortening window will constrain the number of clutches they can lay. It might, but the timing of clutches could be triggered by temperature itself. You'd made the general point that a shorter spawning season limits the population, either by limiting total number of spawners/eggs (you could also acknowledge that the effect might be simply to reduce the timing/variance in offspring Life Histories, which would also have important conservation implications) -- you don't need to wade into the potential impacts to a phenomenon that is uncertain (ie., in its timing, frequency in teh population, importance, or mechanisms)	4
80	4	2413-2419	This text should be moved above the paragraph that begins at 2394 -- this is a clear lead-in to the analysis that follows line 2394. AND, at lines 716-728 you describe temperatures between 8-12oC as being "ideal" for larval longfin smelt. I agree that it is important to study the period in which ideal temperatures may persist, but you should also identify a second threshold that is associated with detrimental temperatures (these migth be lower than outright lethal temperatures and instead reflect the temeprature range where survival/repriduction is severely curtailed. So, above you describe some success of larvae at temperatures up to 15oC, but the analysis here makes it sound like there will be zero suitable habitat for longfin smelt larvae at 2100. I think a more nuanced description would be more helpful.	4
80	4	2422	Love the heat map here! This is the kind of analysis that addresses the key question of this section -- "how will the season of [insert Life History stage] be truncated by rising temperatures?". As noted above, it's critical that you land on a consistent set of temperature limits for each life stage (by "set", I mean temperatures taht define a range of outcomes from optimal, to sub-optimal, to intolerable -- you can choose your own words for those categories). I know there's some variation in temperature estimates and that there is some uncertainty associated with translating lab results to the field --> you need to describe those challenges early on, and then choose your temperature criteria for each life stage/outcome level and use them consistently throughout the SSA.	4
80	4	2413-2419	Given this finding, I am interesteed in the SSA's take on the wisdom of "restoring" presumed LFS spawning or larval rearing habitats (tidal wetlands) in the Delta or around Suisun Bay. If the SSA suggests that habitat resotration is needed (see my cautions above, about the uncrertainties related to that kind of proposal), then this kind of analysis suggests limitations on where that habitat should be restored.	4

Page	Chapter	Line Number	Comment	Reviewer
85	4	2540-2553	My reservations about the hydrological projections aside, if you're going to project changes in flow pattern, you should also reflect on how those changes affect the SSAs view of efforts to "restore" habitat. In this Chapter, the SSA suggests that (1) temperatures are expected to increase in the Delta and Suisun in ways likely to limit the potential for spawning and rearing in this region and (2) flows are likely to increase in the winter in ways that may benefit spawning and rearing in the Delta and Suisun. For contradicting reasons, each of these outcomes suggests that proposals to build/restore more Longfin Smelt spawning/rearing habitat (e.g., "marshes") in the Delta/Suisun Bay are not well-supported (ie., either the area will be too hot to support spawning/larval development or there will be so much more flow into the Delta dueing the spawning/early rearing period that the extra flow will create the added habitat).	4
81	4	2443-2660	Maybe I missed it, but I don't see any acknolwedgement in this section that flows into and through the Delta are highly regulated by humans (e.g., Reis et al. 2019). Changes in natural hydrology are, of course, important. But the effect of these changes on the timing and volume of Delta outflow (i.e., on Longfin Smelt popuations) is mediated by operation of dams and diversions. After acknolwedging that more clearly (and apologies again if I missed it), the SSA should make reference (at least) to regulatory proceedings focussed on changing management of the Central Valley hydrosystem to increase freshwater flow through the Delta, to the Suisun Bay. The State Water Board has already changed flow standards for the San Joaquin River in 2018, and began implementation proceedings for those standards in 2021. Flows from these standards, once implemented, are not guaranteed to make it through the Delta; but the adoption of these standards demonstrates that it is possible to change these water quality standards in a way that results in significant flow increases. The State Water Board is currenly engaged in a multi-year effort to update Delta outflow standards and has indicated that such standards will provide greater inflow and Delta Outflow to protect species like Longfin Smelt and Delta Smelt (and other native fish species). In 2017, the Water Board issued a "Framework" document that outlined its intentions for and the need for Delta Outflow updates (https://www.waterboards.ca.gov/waterrights/water_issues/programs/bay_delta/docs/sed/sac_delta_framework_070618%20.pdf). This document signalled an intention to set minimum Delta outflows as a percentage of unimpaired flow. Obviously, this is not an adopted regulation but it is odd that the SSA completely ignores this ongoing regulatory process. Finally, I think acknolwedgement of the degree of human control over water flows in this system should temper the statement at 3045-3047, which suggests that your assumption (no change in exant habitat stressors) is the "best case scenario". There are clearly better case scenarios than the ones you analyzed in this future conditions assessment -- listing of this species under the ESA might actually increase the odds that future conditions are different (better) than those you have assumed for the future. (See also SWRCB 2010 -- Public Trust Flow Criteria Report; SWRCB 2017 "Scientific Basis Report in Support of New and Modified Requirements for Inflows from the Sacramento River and its Tributaries and Eastside Tributaries to the Delta, Delta Outflows, Cold Water Habitat, and Interior Delta Flows"	4
98	4	2949-3035	Regretably, I have run out of time to review this model description or the asociated Technical Note (TN4). I have skimmed the results however and suggest (as above) that you compare the probabilities of quasi-extinction to probabilities associated with "High" and "Moderate" level of extinction (e.g., Lindley et al. 2007 or the NMFS (2014) recovery plan for listed Central Valley Salmonids). From what I see of these results (and those presented previously, using the Dennis et al. 1991 methodology), the model outputs suggest that Longfin Smelt are at an **extreme** risk of extinction in the very near future -- this justifies a listing the San Francisco Estuary Longfin Smelt population as "endangered" rather than "threatened" and supports a neeed to complete that listing process and to develop protective regulations for this imperiled species **quickly**.	4
106	4	3133	In summary, information provided by the SSA is more than adequate to justify listing of the San Francisco Estuary population of Longfin Smelt as "endangered".	4
2	1	43-47	Page numbering is in error. 2nd page of Intro labeled pg 1. Ocean currents can facilitate northward movements alongshore during winter, but currents moving southward are either far offshore or periodically disrupted by eddys or by currents directed offshore.	5
	1	99	Do you mean at the "individual level"?	5
3	2	142	word choice: extends rather than exceeding	5
4	2	177-178	citation: Garwood, R. S. (2017). "Historic and contemporary distribution of Longfin Smelt (Spirinchus thaleichthys) along the California coast." California Fish & Game 103(3): 96-117. Also in CDFG 2009 Species Status Review	5
4	2	178-179	Citation: Miller, D. J. and R. N. Lea (1972). Guide to the coastal marine fishes of California. This is the original citation; Robert Lea the Monterey Bay collector. Also see Garwood 2017. Appendix 1	5
4	2	169-179	Paragraph does not match section header: it describes very little of the species range. For more complete range listing (see Dryfoos 1965, Moyle 2002; in California - Garwood 2017). The last sentence - Monterey Bay collection -- opens the discussion to other proximal catches (see Garwood 2017 for list) w/o follow-up. Consider a brief description of other catches in California or at least a little northward of SFE.	5
5	2	184	Big change in topic. Consider new section heading.	5
5	2	190-191	Correct interpretation of little southward genetic exchange, but there was some detected from south to north, so there may be some northward genetic support. Might be important in a warming climate. I see this is dealt with in subsequent sentences.	5
5	2	222	Reword. The statement "SF Bay is relatively shallow..." is arguable and depends on what you are calling SF Bay. Consider dropping the shallow part at the start and discuss the relative depths of embayments (i.e., south SF Bay, San Pablo Bay etc.)	5
6	2	224-226	Check SF Bay Study Tech Rept for a better list. Northern Anchovy, Pacific Herring, Top Smelt, Jack Smelt, Gobies are more dominant than most currently listed	5
5	2	209	New section -- Description of the SF Estuary? and ecology? What about temperature and rainfall/snowfall/snowmelt?	5
6	2	231-235	A fine point: maturing fish in the summer & fall prior to spawning are not found in freshwater nor even low salinities (<20 ppt) probably because these habitats are too warm to allow sufficient energy to develop gonads. Later in the year these fish will move to low salinities and freshwater to spawn.	5
6	2	254-257	Also spawning in Napa River, Sonoma Creek, Petaluma River, Coyote Creek/Alviso Slough . Citation: Lewis, L. S., M. Willmes, A. Barros, P. K. Crain and J. A. Hobbs (2020). "Newly discovered spawning and recruitment of threatened Longfin Smelt in restored and underexplored tidal wetlands." Ecology 101(1): 4 pages.	5
7	2	265	73 mm FL is the smallest female with maturing gonads I recovered. CDFG Reg 3 Trishelle Temple may know of smaller fish. they have been looking at LFS the past couple winters after my retirement	5
7	2	266-267	the fecundity (rounded) at size numbers reported are from the SF Estuary; graphed in CDFG 2009a but otherwise not formally reported.	5
7	2	274-281	fine point: based on macro-view of maturing/ripe female gonad, ripe eggs make up 75-80% of gonad weight for Delta Smelt; for Longfin Smelt it's more like 95%+. Eggs may develop serially such that some reach full size before others, but all are nearly the same size at the same time. I only looked at fish during the December through March spawning season. When I measured fecundity, we typically counted and weighted 3 alloquats (anterior, medial, posterior) and all were very close in measures of eggs per gram. Once eggs were separated for drying and weighing, very little material was left to develop into a second batch.	5
8	2	307	semi colon typically ahead of "however" rather than following.	5
8	2	313	Don't think this is the correct citation. No mention of Lathrop. Merz et al. 2013 pg 136 lists that range, but not in relation to dry years. Baxter et al. 2010 and CDFG 2009b SWP Effects Analysis both discuss shifting distributions of adults upstream in winter, moving higher in dry years, but no geographic limits discussed.	5
8	2	314	cite also: Lewis, L. S., M. Willmes, A. Barros, P. K. Crain and J. A. Hobbs (2020). "Newly discovered spawning and recruitment of threatened Longfin Smelt in restored and underexplored tidal wetlands." Ecology 101(1): 4 pages.	5
8	2	330	Consider removing sentence. I do not recall "mud' listed as a potential spawning substrate in any of the cited references (or uncited references). It does not provide additional information otherwise. The best examination of substrate vs egg number, Martz et al. 1996 did not note substrate finer than silt.	5
8	2	332-334	Consider broader substrate preference and additional citation. Based on their own work and that of others Martz et al. 1996 pg 18 concluded LFS preferred to spawn on sand and small gravel substrates in what they refer to as lower velocity areas.	5
8-10	2	339-397	Nicely summarized	5
141		250	Question statement about max length: I would like to confirm ID of any LFS approaching 250 mm FL (actually any >150). CDFW has LFS in the 130-150 mm range in its reference collection.	5
145		4873	Quote may be correct, but true range of mean lengths should be 50-70 mm FL	5
11	2	414-424	Analysis should also consider age class or species abundance: historically largest year classes (produced in high outflow years) had lowest apparent growth (e.g., 1982).	5
11	2	417-420	clarify conductivity/salinity ranges for both parts of the sentence	5
11	2	420-422	At some point in the age-1 growth period energy begins to be devoted to gonads and to changing salinities as fish stage prior to spawning.	5
11	2	414-424	Not sure what monthly periods were used to estimate growth. Knowing the period of months is critical to understanding what behaviors (migration) or life processes (gonad maturation) might be co-occurring. Clarity issue: parentheticals do not all directly follow the phrase they clarify, but should... particularly if additional requested information on conductivity ranges is included.	5
11	2	427-429	Many high outflow years exhibit persistent storms and relatively cool temperatures into spring and early summer, allowing continued and later recruitment, which will tend to suppress growth if year class mean/medians are used instead of modes (cohort group central tendency). High water also disperses food items. However, I think larval survival is improved and age-0 abundance increases are a big factor in slower growth.	5
11	2	436-439	Good to identify abundance (density dependent growth) and protracted spawning as potential factors. both appear to occur in wet years.	5
11	2	439-443	Good point. Also consider that all fish start at low conductivity values. Those migrating to higher values over time use energy to transition physiologically; those that stay put do not.	5
11	2	443-444	It would be good to put this growth phase into seasonal context: January thru June? That is before summer temperatures become challenging.	5
11	2	444-445	Again, a seasonal context would be good here. In the summer, age-1 fish are looking for the coolest temperatures available, but those temperatures are warmer than temps available the following November and December.	5
12	2	479-481	Also see CDFG 2009 LFS Status Review Fig 5 and associated text	5
13	2	500-502	Though hinted at, we do believe that age-0 fish also move to higher salinities in part due to temperatures upstream (also general dispersal and probably other reasons).	5

Page	Chapter	Line Number	Comment	Reviewer
13	2	504-511	Very cool!	5
13	2	506-508	Clause misplaced? I think "during March through July" should follow "...larval habitat availability". Measuring spawner abundance Mar-Jul wouldn't provide the best measures.	5
15	2	543	Figure 2.5 is cryptic. Not sure what was done to produce a residual and what a positive or negative result means	5
20	2	610-613	CDFW has a manuscript that shows Limnoithona tetraspina is often important diet component Jan-Mar or later. Contact Christina Burdi	5
20	2	637-644	Nicely summarized! Another point to make is the effective or targeted length range which is about 4-10 mm after which catch drops off quick.	5
21	2	651-654	Good point about missing February recruitment. Consider another definition of recruitment. In practical terms it is when fish reach sufficient size to be regularly retained by the gear.	5
21	2	655	No description of Summer Townet?	5
21	2	656-665	Fine description of current survey timing and targeted size range. Is it worth mentioning that the survey had run from August to the following March in the past? December - March catch data show and interesting story about age-0 and age-1 fish shifting distribution with changes in X2. Correct estimate for size range to recruit to the gear. Might be worth a mention that this occurs in December to February	5
21	2	667-680	Nicely written. Suggested change: Line 675. Fish do not FULLY recruit to the gear or are not FULLY retained by the gears until the size ranges listed. Many are caught that are much smaller.	5
21	2	682-690	Nicely written. Is it worth mentioning that the daily sampling is spread across 3 trawl lanes: north, central and south? The lanes produce varied catches.	5
22	2	699-700	A finer point: the July to September period is specific to most age-1 fish, but other ages also use the coast at other times of the year. Cite also Garwood 2017. See Gulf of Farallones summary CDFG 2009 Status Review pg 6 bottom	5
22	2	704-707	A better argument for use of BS and FMWT LFS data is that both surveys sample sufficient amount of the range of LFS during the period of sampling to calculate a representative index of abundance. This contrasts with other surveys that do not sample much into San Pablo Bay.	5
22	2	722-725	The sentence about O2 consumption, temperature and growth doesn't make much sense, unless fish are more active at 9 and 15 deg., but that doesn't make sense either. I would guess that fish are most efficient foragers at about 12 deg.	5
22	2	726-728	Be careful attributing what appears an optimal range for one life stage to another. In this case incubation is 2-5 weeks depending on temp, so spawning temps (a brief period/individual) are potentially very different than incubation temps and rearing temps.	5
22	2	729-731	The catch frequency of age-0 LFS drops off sharply at 20 deg, but fish (generally smaller fish) are still common about 21 deg. By 22 deg or above fish are only detected once and never at subsequent sampling in the area. Looking at data I would say 20-21 deg would be a range for extended tolerance. None above 22.	5
23	2	733	In general age-1 fish inhabit lower temperature water than age-0 fish, though both inhabit 16-18 deg water in summer and fall (see Baxter 1999 Osmerid chapter fig 8). Those moving to the coast are likely looking for slightly cooler temps.	5
23	2	736-742	Inclusion of substrate for spawning seems a little off topic. The spawning temperature discussion is accurate.	5
23	2	745-748	Seems reasonable based on otolith chem and previous FCCL salinity challenges for eggs and larvae. two big questions: 1. is high or low salinity tolerance heritable and a variable family trait; 2. when and how rapidly does salinity tolerance come into effect.	5
23	2	750-755	Trivia because not published. Bay Study E&L data show Yolksac larvae have been found in saltier water: Max avg profile salinity ws 30.1 ppt; 99th percentile 18.3; 95th 12.7 ppt. These larvae too likely flushed from a south Bay or San Pablo tributary and had not finished absorbing yolk/oil before salinity remixed.	5
24	2	771-779	summary is fine. LFS larvae do not rely on sight alone to feed. They apparently can use smell (reported in POD reports) and? to feed at night and in high turbidity. I suspect that low effectiveness at NTU<10 is from another cause.	5
24	2	781	surface orientation is only a general pattern for larvae <10-12 mm, before complete air bladder development. Larvae 12-19 mm occupy mostly the mid and bottom strata of water column. Also, emigrating salmonids could be a substantial predation source. But I never got access to any feeding study results.	5
25	2	814-820	The early portion of this paragraph seems out of place. Eggs and spawning should be secondary topics. This is the only paragraph where life stage is located by habitat. Only the last sentence seems pertinent.	5
25	2	824-833	Not all the predictors list will be in the final model. After the first two or so, remaining predictors provide insignificant added resolution and some have no logical biological basis.	5
25	2	850	see also: Kimmerer, W. J. (2002). "Effects of freshwater flow on abundance of estuarine organisms: physical effects or trophic linkages?" Marine Ecology Progress Series 243: 39-55. And... Tamburello, N., B. M. Connors, D. Fullerton and C. C. Phillis (2019). "Durability of environment-recruitment relationships in aquatic ecosystems: insights from long-term monitoring in a highly modified estuary and implications for management." Limnology & Oceanography 64: S223-S239.	5
25	2	850-852	Little change in slope but several changes in intercept (Kimmerer 2002, Thomson et al. 2010)	5
26	2	883	Add incubation as a life stage? Spawning can occur at one outflow level and hatching at a completely different level	5
26	2	889	you report 1-6 psu for the low salinity zone previously. May want to look for that citation and expand upon the definition to include 0.5.	5
26	2	889-895	Shoals of northern San Pablo Bay have freshwater sources from tribs and their depth is conducive to maintaining lower salinity throughout the shoal water column compared to adjacent channel. Also, eddy formed in northeastern San Pablo Bay helps maintain low salinity on northern shoal. See your Fig 2.4	5
27	2	907-909	Sentence a little cryptic: "... net flow goes against the pumps..."?? Might be quicker to say that as east side tributaries and the San Joaquin River flow meet and exceed south Delta export capacity, the region of negative flows diminishes to immediately around the SWP.	5
27	2	919	Correction: January through June. Still post-larvae present in May or early June of some years. See 20-mm Survey LFS length frequency for fish <20mm	5
28	2	966-974	To be fair, runoff associated with increased river flows carries its own set of contaminants: copper and rubber byproducts from roads and pesticides, herbicides and other chemicals from farms and communities... although much is associated with the "first flush", and less problematic afterward.	5
28	2	976	This would be particularly important to allow for second spawns (I am not yet convinced that they occur). Not sure fish would delay first spawns beyond a "normal" spawning period. Alternately, it is very important to extend the habitable period for larvae in tributaries. Probably dealt with for larvae in an upcoming section.	5
29	2	987-990	This sentence has too many disparate parts. There are two different scenarios for north and south bays. We presume that LFS adults stage near X2 and then at least some move upstream to spawn. South Bay is a different issue. Water temperatures in lower Coyote Cr. and Alviso Slough are generally to high due to treated sewage discharge. High flows both provide access farther upstream (Coyote Cr has little discharge normally; it's overwhelmed by sewage discharge) as well as drop water temperatures into the range for spawning.	5
29	2	1001-1003	Also, extended cool weather after flows keeps tributary temps from exceeding tolerance allowing more development before they must leave. Alternately, if spring temperatures are warm and weather clear, tributary temps can surpass the low 20 deg range in March or April, potential before all larvae are able to withstand salinities in the 20-30 ppt range in San Pablo or South bays.	5
29	2	1004	Current and substrate? LFS appear to choose locations with sufficient current to deposit sand or slightly larger substrate, and are not known to spawn in currents producing finer substrates or in lake margins (though the will spawn in tributary deltas within lakes, presumably within the tributary's influence) Martz et al. 1996	5
29	2	1008	Here I think the "during the winter-spring time period" belongs after with word "outflow" rather than the end of the sentence.	5
30	2	1016	Fig 2.8. Consider extending the larval period into the last quarter or third of December. Larvae periodically hatch during late December, and it's an important period for entrainment and recruitment. Otherwise, good summary of needs.	5
30	2	1027-1031	habitat expansion? Perhaps be more specific: expanded low salinity habitat and activation of several Bay tributaries as spawning and rearing habitat.	5
31	2	1032-1034	Perhaps list in temporal sequence and start with upstream shift in spawning location and increased risk of entrainment for spawners and then hatched larvae due to upstream location and reduced transport etc. Also decide whether to include other factors like food production and contaminants.	5
31	3	1066	Consider broadening the section title. Doesn't capture the topics covered.	5
31	3	1070-1073	This sentence doesn't make much sense to me. Perhaps point out: Water flowing out of the Delta and downstream, called Delta outflow, creates many of the benefits to LFS. Delta Outflow is the culmination of the natural hydrologic cycle modified by upstream diversion to storage and municipal/ag use, w/in and south Delta diversions etc.	5
32	3	1086	Change to: "south Delta" at end of line.	5
32	3	1087-1089	Add constraints to operations: water quality standards and protections for fishes can limit periodically control water management.	5
33	3	1116-1117	Clarification of sentence, though I'm not sure if it is a correct statement (see below). Clarification: The ITP terms and conditions are intended to avoid, minimize and fully mitigation negative effects of SWP operations on CESA fishes, not those of the CVP. I believe that the ITP places the burden on the SWP, but intends that some of the conditions will be met through joint operation, rather than solely actions of the SWP.	5
33	3	1119	Editorial comment. The last sentence draws a conclusion about measures which have not been fully implemented. Certainly, past measures implemented have not been sufficient to reverse the decline.	5
33	3	1122-1137	Not familiar with this information. Line 1128, what about below normal yrs? Comments: Doesn't converting these small percentages result in some big water volumes? Maybe those values should be supplied as well?	5
34	3	1145	Editorial: Suggest changing "recovered from" to "experienced". I'm not sure we recovered, except briefly in terms of water supply.	5
34	3	1149-1153	Good point! There appears to be a decline in densities (2014 for north and 2013 or 2014 for south) that did not reverse in 2018 (the only dry year subsequent to a wetter year). I suspect reduced south Delta survival is negatively feeding back on the portion of the population inclined to spawn there.	5
34	3	1149-1162	Comment: 2017 recruitment was good. Most would spawn in 2019, another wet year, in which LFS tend to spawn lower in the estuary. the dry winter of 2021 will be the real test of whether increased adult abundance and a high X2 will provide circumstances leading to increased larval numbers in the north and south Delta.	5

Page	Chapter	Line Number	Comment	Reviewer
34	3	1159-1162	The point of the last sentence is unclear. "...survival is likely density dependent..." conclusion: survival should be good because densities are low; " ... the population has already experienced multiple step declines.."; conclusion: not clear, but perhaps another is emanant leading to lower abundance (?); the final conclusion --"... the current pop may have difficulty rebounding..." -- does not necessarily follow. You already used the example of 2017 as a sharp rebound in abundance (not larval distribution in part due to wet year effect). Be careful how you discuss abundance vs distribution.	5
36	3	1189-1190	Correction: Most year-classes (year of hatch) need to spawn at all three age-classes (ages 1-3).	5
36	3	1189-1196	It's not as simple as even numbers of positive and negative growth years or one per life cycle. The magnitude of the changes is important. Wet years provide a big increase and that is more slowly winnowed down during successive dry years. This paragraph need some modeling support. Otherwise consider removing paragraph.	5
36	3	1200-1204	Cite Grimaldo et al 2009. Factors affecting fish entrainment...	5
37	3	1222	Most recent : Tamburello, N., B. M. Connors, D. Fullerton and C. C. Phillis (2019). "Durability of environment-recruitment relationships in aquatic ecosystems: insights from long-term monitoring in a highly modified estuary and implications for management." Limnology & Oceanography 64: S223-S239.	5
37	3	1225	Replace "survey" with "study"	5
37	3	1230-1231	Consider slightly reframing sentence in more specific terms: LFS have exhibited little or no ability to maintain abundance levels during dry years or something to that effect.	5
37	3	1232	Terminology change. I don't see drought years as events. consider changing "events" to "stressors". Also, presumably the dry year and warming temperature connection will be made in an upcoming section.	5
37	3	1238-1245	Good use of "catastrophic event". Include dilution effect? Size of Delta vs San Pablo habitat?	5
37	3	1250-1251	Eury dominance is true for spring in upper estuary. Limnoithona is more dominant in Jan and Feb larvae (CDFG unpublished) and other inverts are important at times in Napa River and San Pablo Bay.	5
38	3	1260-1264	Two points: 1. the abundance decline was a sharp substantial decline: a step decline. 2. Other zooplankton taxa invaded the estuary coincident with or in subsequent years to P. amurensis introduction.	5
38	3	1279-1280	A couple points. Streamline sentence by removing vague clause. Change to: "... and former abundance peaks were reduced in magnitude and shifted temporally after 1987." Remove the formally occurred clause. SECOND, one of the species whose peak abundance season changed was E affinis; peak abundance changed from summer to spring, AND its abundance is now outflow related (Kimmerer 2002; not sure if regression still holds). I suspect that E affinis abundance is controlled by clam grazing and competition which are both inhibited by high spring flows and cooler water temps that allow the E affinis abundance period to persist longer through the spring.	5
38	3	1288-1297	Several points to consider. Opossum shrimp is a common name for mysids, not for Neomysis mercedis. N mercedis abundance declined sharply after P. amurensis invaded and is now only a small fraction of the mysid population. Other mysids have been introduced and are now more dominant. Hyperacanthomysis longirostros (spelling??) is now dominant, but matures smaller and slimmer than N mercedis, thus like other zooplankton, mysid size has decreased (due to change in species composition, not growth potential).	5
39	3	1306	Clarification. Should be clear that you mean step declines in abundance.	5
40	3	1343-1348	Authors found a small number of toxic events over the study period, some of which were associated with presence of specific chemicals, but there was no pattern or persistence.	5
40	3	1357	Suggestion. Perhaps qualify this sentence to the SF Estuary population.	5
40	3	1362-1366	Again, I don't think many adult fish inhabit water even approaching 22 deg C, though a rare few inhabit that temp. Lab tests show larvae and small juvenile stressed by 20 deg C, but field observations show juvenile in summer periodically in habiting 21+ to 22.0 deg C, but almost never above that. Age-1 (adult?) mostly leave the estuary when water temps are >20 deg C., but again a few are present to 22 deg C. I suspect that maturing fish (true adults) require water temperatures in the 16-18 deg C range to develop quality gonads. I believe summer juveniles (age-0) are most tolerant.	5
41	3	1374-1376	Complete the sentence. "...caoturing more prey than would be needed in cooler water for maintenance and growth."	5
41	3	1376-1382	Needs a finer point. Bay Study OT age-0 LFS in July exhibited a mean salinity of about 25 ppt and 1SD reached 30 ppt (Baxter 1999) so some juvenile in July (if not sooner) can withstand full marine salinity. CDFG summaries of City of SF Outfall monitoring catches of LFS also shows both age-0 and age-1 LFS present in the Gulf of Farallones (CDFG 2009 Status Rev Pg 6). HOWEVER, warming temperatures early in spring may challenge young juvenile salinity tolerance. Tributary temperatures warm faster due to smaller volume, so these habitats may be first to go.	5
41	3	1398-1399	Additional citations: Brown and Michniuk 2007, Mahardja et al. 2017 Brown, L. and J. May (2006). "Variation in spring nearshore resident fish species composition and life histories in the lower Sacramento-San Joaquin watershed and delta." San Francisco Estuary and Watershed Science 4(1): 1-15.	5
41	3	1407	Correction - current text makes it sound like LFS spawn on Lake Washington shorelines. There is NO reference I know of that indicates LFS spawn on "shorelines". Martz et al. 1996, who reviewed previous work said specifically that LFS did not spawn on the lake shoreline. They did spawn in tributaries and tributary deltas at the lake perimeter. They may very well spawn on sandy riverine shorelines in the SFE, but they also spawn in deep riverine channels as evidenced by recently hatched LFS larvae in occasionally high density near the bottom. in such habitats (CDFG unpublished Tucker Trawl data)	5
42	3	1433-1435	So concentrating LFS makes them more vulnerable to predation? Perhaps a better angle is to argue that lower turbidity in dry years improves effectiveness of LFS predators. Off the top of my head I do not have literature support for varying turbidity in wet vs dry years, but believe there is in the 2010 POD report.	5
43	3	1450	and eggs and larvae are difficult to detect in diet studies because the do not persist long in digestive tracts.	5
43	3	1456-1458	include eggs in this sentence as well. Shrimp could be substantial egg predators. A potential DNA project because shrimp shred food.	5
43	3	1463-1464	The Striped Bass introduction sentence is not pertinent here. Consider removal. Possibly point out that adult numbers have been in decline.	5
43	3	1487	citation?	5
44	3	1492-1493	Agreed. The most likely predation sources have been (#1 and most likely) juvenile salmonids migrating while Yolk-sac larvae are present (Late Dec to early March) and #2 (and much less likely) Mississippi Silversides (offshore movement at night in Clear Lake). I could not get access to juvenile Chinook feeding data during lower estuary migration, but Merz found Chinook and Steelhead fed on larva in the Mokelumne. Merz, J. E. (2001). "Diet of juvenile fall-run Chinook salmon in the lower Mokelumne River, California." California Fish & Game 87(3): 102-114. Merz, J. E. (2002). "Seasonal feeding habits, growth, and movement of steelhead trout in the lower Mokelumne River, California." California Fish & Game 88(3): 95-111. Mississippi Silversides remain along the shoreline during the day except during high turbidity. This would result in higher MSS predation in wet years. Not enough night sampling data to check if there is offshore movement.	5
44	3	1520-1522	Add Selenium?	5
44	3	1528	Common name change: I think circa 2007 there was consensus among local taxonomists that the Silverside was Menidia audens, common name Mississippi Silversides. Purchased bioassay fish may be a different species. Those obtained from SFE are MSS.	5
45	3	1543	Livestock runoff as well through Denverton Sl.	5
45	3	1556-1581	New reference on ammonium issue: Cloern, J. E. (2021). "Use care when interpreting correlations: the ammonium example in the San Francisco Estuary." San Francisco Estuary and Watershed Science 19(4): 12.	5
47	3	1636	Barker Slough pumping plant is located in the north Delta and feeds water into the North Bay Aqueduct. No other facility on Green Valley Cr. Morrow Island and Goodyear Slough diversions are located on other sloughs, but in the same vicinity as Green Valley Cr.	5
47	3	1655-1657	This statement was true for adult LFS salvage historically, but adult LFS salvage has not happened in recent years, likely due to negative OMR limits..	5
48	3	1665	Hydrodynamics/the salinity field determines to some degree where adult LFS spawn. When X2 is well inside the Delta, LFS spawn well inside the Delta as well, and their larvae when hatched are more vulnerable to entrainment. CDFG 2009 SWP Effect Analysis pgs14-19	5
49	3	1700-1702	Actually, it's more like months larvae need to remain to reach 20mm (hatch at 6 mm and growth to 20mm @ 0.15 mm/day estimated from apparent growth (20mm Survey data; Souza 2007?) = 93 days. First large hatches occur in January; first juvenile salvage occurs in April, roughly 3 months hence.	5
49	3	1706-1708	PTM results in 2008 are as described, but they were also a substantial improvement in reduced SWP/CVP entrainment	5
49	3	1712	I think the reference should be to Figure 10 in CDFG 2009b.	5
49	3	1725-1727	This sentence takes some liberties. Estimated entrainment and estimated losses are almost equal, but the multipliers 4 and 21 referred to estimated entrainment rather than losses. If you want to make a point about losses it should be cited recognizing the difference. AND Replace "actual" with "estimated salvage" because it too is a calculated rather than counted number.	5
49	3	1729-1731	"... these estimates were calculated based on experimental survival measurements made using Delta Smelt and other species...	5
49	3	1731-1733	Comment: Be careful about pointing out results from one apparently extreme year as rationale for lower confidence in a measure. LFS risk to entrainment and estimated entrainment vary widely based on X2 position and export timing and magnitude (in really dry years exports can be low and LFS entrainment risk low). Water year 2002 was the perfect entrainment storm: pulses of increased outflow almost equaled the magnitude of exports, so most of the flow increases were converted to exports transporting larvae and then juvenile south to the pumps. This was a real event displaying unusually high juvenile counts and thus salvage numbers. Considering at least a couple months of unaccounted larval entrainment preceded the juvenile entrainment and salvage, the losses are very likely a good ball-park figure.	5
50	3	1757-1759	Caption should be revised for Fig 3.5 as only the last two graphics are displayed.	5
50	3	1762-1764	This PTM work was based on a "modeled population" and estimated fraction entrained depend sensitively upon how the modeled larvae were distributed geographically (i.e., what fraction of total particles were injected into the central Delta/lower SJ River stations).	5
50	3	1767	.... population level consequences are likely lower now under most circumstances, but as in 2020, circumstances can still occur that lead to increased risk of entrainment and strongly negative outcomes (i.e., 2020 salvage levels represent a relatively large effect given the low abundance at the time).	5
51	3	1781-1789	Good to point out the negative bias in sampling and abundance estimates associated with wet years. Consider identifying which years plotted were underestimated due to larvae distributed in San Pablo Bay.	5
51	3	1791	Should reference Fig 3.6	5

Page	Chapter	Line Number	Comment	Reviewer
51	3	1781	Perhaps a sentence describing how decline percentages were calculated?	5
52	3	1798	Should reference Fig 3.7. Figure caption should include the range of years depicted in each graphic	5
52	3	1808 and 1811	Figure numbering off by 1. Line 1808 should refer to Fig 3.8; Line 1811 to Fig 3.9	5
52	3	1811-1812	Sampling was incomplete for 2016 and 2017 as mentioned previously. Perhaps note the percentage of tows completed during the February through October index period relative to the usual number of tows (=35 per survey month).	5
54	3	1821-1825	Missing text referencing Fig 3.10 (LFS outflow vs abundance thru recent yrs) and describing relevance of its contents.	5
55	3	1856-1866	20-mm Abundance with no X2 covariate to account for flow/distribution bias (i.e., part/most of population in unsampled San Pablo Bay during moderate to wet years)?	5
55	3	1870	The assumption of no density dependence may be a problem for age-1 abundance (see Nobriga and Rosenfield 2016)	5
56	3	1913	Check legend for Fig 2. Prob of quasi-extinction (Also renumber to match section). I would think the only age class with positive growth (OT age-1) would have the lowest probability of extinction??	5
55	3	1850	Table 1 not provided for this section or referenced correctly to its placement elsewhere	5
56	3	1883	Table 2 not provided for this section or referenced correctly to its placement elsewhere	5
56	3	1889,1893	Table 1 not provided for this section or referenced correctly to its placement elsewhere	5
59	3	1968-1969	"This DPS' relationship with Delta outflow 'has been recognized'...". It has likely existed since the estuary became ice-free.	5
59	3	1970	LFS recruitment responds poorly to low outflow (though abundance declines occur more gradually than increases). However, favorable conditions and fast growth can allow LFS to spawn at age-1,... or poor conditions may cause them to delay and spawn at age-3, suggesting some resilience	5
59	3	1974	Spawning limited to Suisun Bay and the Delta. Isn't this limited redundancy? The complex life history adds resiliency.	5
59	3	1994	Calling 20 deg C the upper limit is not correct. At 20 deg, LFS larvae begin to suffer physiological stress effects, but they tolerate higher temperatures to about 22 deg for some period and their limit, critical thermal max of 24.8 deg C (see Fig 2 in Jefferies et al. 2016). This same range is likely for small juveniles. This same reference also provides measures of monthly temperatures and confidence limits for freshwater portions of the upper estuary that make the point of temps warming above 20 de3g by summer. This happens even sooner for small bay tributaries, forcing migration even earlier.	5
59	3	1996-1998	Currently some small portion of the age-0 LFS population moves rapidly to marine conditions (5-10% of Bay Study YOY catch in E&L net). These are mostly 20-30 mm fish. By 40 mm and in June when temperatures get warm upstream, good numbers are located in Central SF Bay (Baxter 1999 Fig 7a, Table 7). Other fish may have moved to the coast so distribution in marine conditions is under represented in Bay Study data.	5
60	3	2004	Some minor portion of the POD is likely due to a downstream shift in juvenile LFS distribution (Tobias and Baxter in revision.	5
60	3	2006-2018	The POD has been described as a regime shift to a different state. It's not clear whether it was an event or a "suite of influences" (not sure they were random) that combined to change the state. This new state/regime is less conducive to LFS recruitment and survival.	5
60	3	2012-2013	A couple points: at -5000 OMR the lower SJR remains w/in the zone of entrainment, although the risk is modest and downstream transport can move larvae out of range for entrainment. Second, the lower SJR was likely once good LFS spawning and rearing habitat (as it was for Striped Bass!) because currents maintain suspension and increased residence time allowing blooms to build. This loss of spawning and rearing habitat has not been truly mitigated for by habitat restoration.	5
60	3	Table	Red FW Flow - reduced habitat and productivity resulting in lower recruitment Overbite clam -- reduced productivity reducing recruitment, carrying capacity, survival and growth. Tidal Habitat Loss - Identify as historical and replacement habitat slow to function. Increased Temperature -- first a stressor leading to reduced growth and health (lethal mortality likely limited even for young of the year to small volume habitats, e.g. , Sonoma Creek, Napa River). Temperature of the main Delta and Estuary does not become stressful until June, when most LFS can effectively migrate westward. Contaminants: although it might fit w/in definition of fitness, predator avoidance can also be affected by contaminants (see metals/copper and salmonid lit).	5
61	3	2021-2022	Plus climate change resulting in general warming and an increase frequency of dry years (or reduction in wet years.)	5
65	4	2161	W/m2? What does "W" stand for?	5
66	4	2221-2229	Narrowed spawning window, increased incubation, hatching of more larvae earlier, possibly prior to seasonal increases in sunlight and productivity; increased metabolic rate of larvae may not be supported by early season productivity; increased temperatures in spring may cause early elimination of small tributaries as rearing habitat and subsequently the Delta.	5
67	4	2243	i think you mean marine influenced rather than tidally influenced. Yet marine currents and temperatures will have their own responses to warming.	5
67	4	2243-2245	Don't you need to at least generalize about north and south Bay tributaries in the context of addressing redundancy (If I'm understanding your process)?	5
68	4	2280-2285	Well articulated. I agree with all limits.	5
68	4	2287-2291	Consider pointing out that the 22 deg C limit includes young juveniles as well. This limit is especially important in limiting habitat suitability in the first summer in small habitats... i.e., forcing migration possibly prior to developing salinity tolerance.	5
69	4	2304	It is also worth pointing out whether much of the increased temperature is occurring in Spring/Summer or in Fall. The early increases will reduce rearing time before migration; the later ones will delay return migration and at lower but increased temps, delay spawning.	5
69	4	2308	Consider broadening life stages impacted (i.e., not just adults). Could use generic Longfin Smelt as the 22 deg C limits age-0 in summer and fall, age-1 in summer and fall (latter would be the only effect on "adults". Consider using age class designations rather than the more vague terms adult and sub adult. These terms were seasonally defined for Delta Smelt, but not Longfin Smelt.	5
69	4	2315	Here and elsewhere in paragraph consider use of age-0 or age-1 or juvenile depending upon life stage considered rather than subadult.	5
69	4	2322	Narrow spawning window forces more of the larval population to rely on the same resources at the same time and limits the species ability to successfully vary spawning time to take advantage of varied conditions..	5
71	4	2334-2336	Tobias and Baxter is currently in revisions, though a preprint is available, it will not match the final.	5
71	4	2340-2343	Adult returns and likely spawning seems more confined to mid- to late-December thru early February timeframe. With fewer years showing much later spawning. I think Trinh Nguyen's data show this.	5
72	4	2354-2356	The conclusion, I believe is correct (though I think spawning begins about 13 deg C, there is not enough evidence to argue the difference). I am surprised that it was obtained using 20 day incubation period (long for late in the season) and <8 mm (large for recently hatched). 14 days is a better late season incubation period and use of yolk -sac or oil globule presence, rather than length will limit duration to about 2 wks post hatch (i.e., 4 weeks total back-projection).	5
73	4	2364-2366	The averages have ranged from 117 to 125 days. An average of an average would be a singe number.	5
73	4	2370-2372	Emphasize the point: "... resulting in a drastically shortened spawning season." All the scenarios result in a shortened season. W/o emphasis this last clause is redundant and not necessary.	5
74	4	2382-2386	Logic is laid out a little haphazard. Curtailed spawning window might not allow sufficient time to acquire energy and develop gonads for second spawning thereby reducing female fitness and population productivity.	5
75	4	2397	Suggest replacing "maturation" with "rearing" or "nursery".	5
75	4	2394-2396	Not sure what the significance of 12 deg C is (optimal metabolic temp?). That aspect should be bolstered.	5
75	4	2400-2411	Now <12 deg C is suitable rearing habitat. I do not agree with this limit. Suitable extends higher. The upper limit of suitable should be more like 15-16 deg C. Consider changing to some other term. Even optimal as used above is most relevant to 12 deg C exactly, because colder temps are not optimal. Perhaps an index of beneficial temperature habitat?	5
76	4	2432	use the term juveniles or age-0 rather than the undefined "subadults". Arguably, subadults could be the age-1 fish migrating back to the upper estuary to mature (become adults) and spawn.	5
77	4	2436	Accelerate is not the correct descriptor. Consider using "exacerbate" in a re-written sentence.	5
77	4	2461	Again the variable "W" is undefined.	5
78	4	2496-2500	might add a clause to the first sentence, "... ' the remaining 35-40% in the form of snow-melt run off April-June." This will clarify the flow contributions.	5
80	4	2509-2510	Revise sentence using 35 rather than 40%. On previous page the snow melt contribution was 35-40%... so 40% was the high, not low end of the range.	5
80	4	2514	Do you mean for the month of April (i.e., that month only) OR "as of the month of April (i.e., up to that month)?	5
81	4	2546-2548	"... the eggs and resulting larvae could be pushed into SFB..." It's unlikely that net flows downstream will be stronger than tidal flows and move egg on the bottom or an attached substrate downstream; thus, not a problem. Could be a problem for recently hatched larvae for a limited time period. Within several weeks (1 month) of hatching larvae can depth orient and limit downstream transport if needed and maintain position relative to salinity.	5
81	4	2555	Add that climate models predict warming temperatures but no change in the frequency of wet water years, correct? I see this is not the case based on subsequent sections. Was there a narrowly defined wet vs dry year analysis reported previously?	5
81	4	2562-2563	Replace "scale" with "stage". Also, a sharp decline in spring inflow is unlikely to coincide with many newly hatched larvae that would be exposed to rapidly increasing salinities; currently newly hatched larva numbers drop through March and remaining proportion is low in April most years. Larvae after the first month or so can depth orient and easily move upstream with receding low salinities.	5
84	4	2635-2637	being able to spawn at age 1,2 and to some extent age3 will not buffer against consecutive dry years impacting two or more generations.	5
84	4	2639	I think you mean: "...the pattern of projected future water year types...", not just a list of future water year types.	5
84	4	2644	Again, stating the LFS abundance declined because of step declines seems circular and should be explained as well as possible. Maybe cite the previous explanation.	5
85	4	2645-2646	"... coupled with greater water demand..." to avoid ambiguity	5
85	4	2674-2675	Not clear what the point is here. Continued Delta land subsidence will exacerbate the flood risk relative to adjacent mineral soil lands?	5
85	4	2671	Wonder if you want to convert sea level and rise to local water surface elevation and rise in this or the next paragraph, because the water surface elevation naturally increases upstream. Moreover sea level rise may not be directly translated into local water surface elevation increases.	5
87	4	2709-2713	Consider adding current tidal range in feet at Collinsville or other known location so percent changes can be interpreted.	5
87	4	2730	For Delta FW Marsh, why does 2 ft of SLR convert marsh land to Low Marsh for 2050 high but not for 2 ft at 2085 low?	5
87	4	2741-2743	Terminology? I think the term "leveed islands" is in more general use than "leveed ecosystems". If there is a reason for the leveed ecosystem terminology, include it in a clarifying sentence at the start of the paragraph and continue using the term.	5

Page	Chapter	Line Number	Comment	Reviewer
88	4	2762	Terminology. There "may be" or there "are expected to be...". I think you want to be more definitive where you can.	5
88	4	2776	Convert cm to feet for consistency?	5
89	4	2794	Again, I think a stronger statement could be made here. Certainly salinity intrusion depends upon outflow, and thus vary seasonally.	5
89	4	2796-2802	I don't think the extra distance (10 km) is consequential in travel time (tidal-surfing a single large flood tide) or energy/fitness. However, it could increase risk of predation and will certainly increase risk of entrainment if south Delta pumps remain operational (See CDFG 2009 Effects Analysis).	5
91	4	2835-2838	Good summary. Spawning migration would be extended and possibly result in increased predation, but again, travel time and effort are minimal, requiring a single strong tide and tidal surfing each direction. This would likely have only minimal impacts on fitness. Risk of entrainment increases for adults and larvae as adults move farther into the Delta and spawn.	5
91	4	2857-2858	M.Silversides are littoral and not considered pelagic except perhaps at night briefly. Threadfin Shad are a better example of a partial competitor. LFS quickly become at least facultatively benthic feeders on amphipods etc.	5
92	4	2875	The proliferation and durability of floating plastic trash in the oceans is providing additional substrate and means for marine organisms transversing the ocean.	5
92	4	2897	...slowing water, allowing settlement of inorganic particles and decreasing phytoplankton.	5
93	4	2921-2935	Quagga and Zebra mussels are both freshwater organisms and would affect turbidity, phytoplankton and zooplankton production upstream of P. amurensis (i.e., they would complement rather than compete with P. amurensis), drastically reducing nursery habitat quality for LFS.	5
95	4	3007	NGPO?? Do you mean NPGO? North Pacific Gyre Oscillation?	5
94	4	2973-2976	How were covariates identified and defined prior to selection? Stochastic variable search method? They were selected at random?? Clarification? Rationale? Not saying they're wrong, but in other papers variables with little plausible effect were included because they helped the model or improved fit.	5
97	4	3022	Density dependence is typically defined by a fixed limited resource at some point in the life cycle (e.g., spawning substrate in a river); whereas here it covaries with highly variable outflows and food-organism abundances. Results are plausible. The analysis identifies variables deemed to have explanatory power for patterns in vital rates; these same variables, i.e., some measure of winter spring outflow affecting age-0 recruitment, show up in other analyses as important.	5
95	4	2997	SSVS?? Does not match stochastic variable search method identified in methods.	5
95	4	3009-3012	the patterns for parameters a and b appear to be inverse of each other and a tracks outflow and b tracks winter drought. Both show breaks in 1993 when the previous drought broke and became generally wet through about 2000, before3 starting the next mostly drought period.	5
98	4	3030	Years plotted? 1981-2015?	5
161		5286-5295	High outflows create benign conditions improving survival. Apparent growth is reduced by continued recruitment of fish at the small end of the range, so growth appears slow or through competition is slow. Look at growth in 1982, when abundance was close to peak, but most age 0 recruited in San Pablo Bay: a few really fast growers, but mostly somewhat slow growth that's apparent in 1983. In poor recruitment conditions, the LFS that do survive tend to grow well.	5
99	4	3049-3050	The effect of fewer days <12 deg C is extirpation? Seems extreme for a sublethal boundary. I think larvae will spend more development time in a stressful environment which could lead to extirpation if other factors added stress. days<14degC and constricted spawning window could contribute to a mis-match of larvae and food or sufficient food Seal level rise ->salinity intrusion->spawning and rearing farther east, increasing risk of entrainment in south Delta pumps. Also for sea level rise, I do not think shifting X2 10+ km upstream will affect fitness. That distance is inconsequential from an energetic standpoint.	5
100	4	3054-3056	Might be better to approach this from a diminished habitat perspective (south Delta already lost; Delta proper often not suitable and same for Suisun at times). You already present data (Fig 4.10) that Martinez is too warm for LFS for a period each summer. The central and south Delta are warmer, as are small bay tributaries.	5
101	4	3072-3074	Possible clarification. The frequency of dry years will increase at the expense of below average years (or other moderate flow years). I thought I read that the frequency of wet years will remain the same. This is a fine but important point... as is the improved likelihood that dry years will string together.	5
101	4	3079	Depleted food supply may be sufficient, but the carrying capacity of the system has diminished. Food supply is largely responsible, but other factors may be contributing as well.	5
101	4	3085-3086	Consider removing sentence about reduced spawning and rearing habitat with increasing X2. I do not believe that was successfully argued in the text. Certainly, rearing habitat in the Delta puts larvae at risk of entrainment and potentially high water temperatures.	5
101	4	3099-3100	Also the zebra mussel invasion of the Hudson River. Zebra mussel would invade the Delta eliminating upstream subsidy of P forbesi to Suisun by eliminating production in the Delta.	5
95	4	3013-3014	Cite Fig 4.21. Data for 2065 in Tech Note?	5
2	1	120-121	resiliency, redundancy and representation do not seem important in structuring this report and are not explicitly addressed in the final conclusions which seems like an important omission.	5
99	4	3040-3041	Seems like resiliency, redundancy and representation should be topics for final conclusions here, not simply words to stimulate memories of previous sections. These topics seemed to come up irregularly in preceding sections rather than forming an organizational theme for discussion.	5
141	Tech note 2	4715	This may be redundant on my part but no LFS of anywhere near 250 mm have been verified by multiple sources or genetics from anywhere! Many such fish have proven to be typos when entering data sheet data. If based on real smelt, the Eulachon (rare stray to Sac R) too only have max length of 225 mm or so.	5
154	Tech Note 3	4988	Correct citation year. Yanagitsuru et al. citation should be 2021 here and elsewhere in the Introduction	5
155	Tech Note 3	5019-5021	Conductivity can be too high or too low for species; LFS larvae appear sensitive to both OR parentage may affect such sensitivity.	5
155	Tech Note 3	5028-5044	I would be skeptical of identification of fish < ca 4.4 mm and > 155. Larva preserved in 70% ETOH can be smaller due to severe shrinkage.	5
156	Tech Note 3	5068	Trivia: Some real negative outflow values occurred June 2004 associated with Jones Tract levee breach June 3-5? Not sure how long it took to fill.	5
2	1	87	Resiliency and Representation both include some genetic aspects -- evaluate genetic health/breath of LFS with respect to each? Do remember this being addressed. Connectivity was.	5
4	2	165	Complete genetics review here? Genetic health, breath? Saglam, I. K., J. A. Hobbs, R. Baxter, L. S. Lewis, A. Benjamin and A. J. Finger (2021). "Genome-wide analysis reveals regional patterns of drift, structure, and gene flow in longfin smelt (Spirinchus thaleichthys) in the northeastern Pacific." Canadian Journal Fisheries & Aquatic Sciences 10.1139.	5
4	2	178	I believe the original citation for Monterey collection is Miller and Lea 1972. Guide to coastal fishes. Bob Lea identified the LFS from Monterey.	5
8	2	314-317	Highlight the aspects of Resiliency and Redundancy from the last in the last sentence?	5
11	2	420-424	Confusing summary. Highest growth at lowest conductivity (range 3-10mm/month), but then notes a higher range at higher conductivity (<15mm/month). Which is it?	5
11	2	422-424	Don't know what is meant by high temperatures, but I believe that 21 deg C is a likely biological threshold and that many LFS physiological systems breakdown rapidly beyond this temperature, regardless of modeling. There are no repeat sightings of LFS at temps > 22deg.	5
84	4	2639-2646	Increasing frequency of dry and critically dry WYs will put further strain on LFS resilience?	5
155	Tech Note 3	5048	Cohort can also have a specific meaning for a group (mode) of lengths representing a large hatching event distinguishable by lengths through subsequent weeks or months. For species like LFS that possess relatively long spawning periods more than one cohort can be expected each year. In years of high numbers of hatchling cohorts may overlap to be indistinguishable and form a single mode through time.	5
156	Tech Note 3	5064	encompasses all age classes for each year class.	5
157	Tech Note 3	5104-5105	SLS and 2-mm data were used, so what about larval (yolk-sac and post-yolk-sac) to juvenile growth and transition?	5
156	Tech Note 3	5088	Not sure this method will capture the various growth changes through time (initial growth to post-yolk sac, then change at juvenile stage which is protracted and varies seasonally, the transition to mature.	5
158	Tech Note 3	5145	Adult? How defined: after assumed maturity?	5
158	Tech Note 3	5157-5164	Not following logic presented. Size at hatch varies, but distribution of sizes likely similar across years, so variation attributable to hatch dates. If this L0,Y describes variation, then it should describe the width of the hatch period, correct? Certainly if the value is small then hatching started later but also ended sooner, on average, correct? This is not to say that hatch dates cannot converge on an earlier or later date within the spawning season. Earlier seems to be happening based on adult presence.	5
158	Tech Note 3	5178-5180	Chipps Island only measures fish >=25? That was once the case.	5
159	Tech Note 3	5184	Cohort versus year-specific growth model? I do not understand the distinction? Previously cohort equaled year class (fish produced in a year)..., so this means growth through life (i.e., over 24-26 months)?	5
155	Tech Note 3	5054-5059	I agree that Martinez represents a good choice to capture dry to wet salinity conditions and intermediate temperatures.	5
156	Tech Note 3	5088-5099	This model is probably fine for juvenile to adult growth phases, but does not account for larval to juvenile changes and for apparent seasonal changes through the juvenile phase. However, simplification may be sufficient and allow for reasonable assignment of influential covariates. Does this model handle multiple ages (i.e., years not months) of maturity?	5
158	Tech Note 3	5157-5164	Where is equation T4?Presumably L subscript 0 is length at hatching?Okay, I agree with this description. I believe i mis-read it elsewhere or on a previous review of the Doc when I jumped to this location. For some reason, I cannot get the comments to assort correctly.	5
167	Tech Note 3	Figure 1	Y axis for top row of graphics is growth in mm/month? Far right y-axis appears mis-labeled. Bottom graphs depict monthly growth as well? Again right graphic appears mislabeled	5
159	Tech Note 3	5213-5216	Presuming the AIC value is a contrast between fixed and year-specific covariates, then I think the symbol should be Less than to make statement correct (i.e., AIC <2800	5

Page	Chapter	Line Number	Comment	Reviewer
160	Tech Note 3	5226-5228	I think the fixed site measurements may prove useful for observing general patterns of increase or decline, but since they will not often measure water parameters near LFS habitat in summer and fall, magnitudes are off. Specifically, growth of 30-100 mm per day is not anywhere near realistic for a fish that matures at 100 mm give or take 15 mm.	5
160	Tech Note 3	5231-5243	Not sure how L-zero is derived to comment on these results. All make sense. However, for flow I think high flows and rainy weather lead to cooler springs and protracted successful spawning and early rearing, which would tend to lead to later hatch dates (and increase the variance in L-zero), which appears contradictory.	5
160	Tech Note 3	5247	Check intended point of sentence. Not sure what survey was meant instead of SLS (Bay Study?), but SLS measures larvae and small juvenile which almost all are < 40 mm.	5
169	Tech Note 3	5396	Fig. 3. These graphic do not appear to include 2 data points prior to 1980. Check data range. If practical add annual and 5 yr ticks.	5
160	Tech Note 3	5240-5243	There is interaction between outflow and conductivity, but little of the variation in flows is related to water management. Occasionally some substantial flows in fall related to reaching flood pool behind the dams after a wet year.	5
160	Tech Note 3	5251-5253	I would be suspicious of lengths from about 2014 forward due to low sample size.	5
161	Tech Note 3	5271	Missing decimals in the parenthetical (i.e., should be 0.11 and 0.24 mm/day)	5
161	Tech Note 3	5282-5283	At about 6 months LFS transition from an extended larval form (still semi transparent) to a fully pigmented juvenile and begin to put on more weight per unit length	5
161	Tech Note 3	5286-5295	During unfavorable conditions growth and survival appear coupled, but during benign conditions, like high outflow for LFS, individuals survive outside the norm. This is particularly turn if rainfall and cool conditions persist through the spring allowing survival of more later spawned larvae. Under such conditions more slower growing individuals survive than would otherwise.	5
161	Tech Note 3	5291-5292	Recent E. affinis abundance in wet years remains a small fraction of its historical abundance. Both its seasonal density and its seasonal availability are much diminished (it was once abundant through the summer).	5
161	Tech Note 3	5292-5295	Good idea! I believe that Bay Study data from 1982 and 1983 will show this. I suspect that data from the late 1900s will also show some density dependence by at a much reduced level.	5
172	Tech Note 4	5470-5473	reproduction by only age-2 fish will capture the main signal for adult effects on age-0 recruitment.	5
173	Tech Note 4	5510-5512	I agree that use of more contemporary vital rates is better. Rather than a 10 yr period for selection, consider using break points for changes in vital rates from this run for future runs.	5
174	Tech Note 4	5529-5530	Clarification? Survival rates tend to be lower than a single rate? Think something is missing.	5
178	Tech Note 4	5641	Consider actual definitions for each variable so reader can get a better idea of how variable was developed and whether such was appropriate for intended use.	5
10-11	2.3.4	400 et seq	The model does not adequately address the fact that as LFS grow they migrate downstream beyond areas extensively sampled and return to upstream, sampled areas later. This would result in treating the downstream migration of growing fish as a loss rather than a life history stage not factored into the analysis.	6
20	2.4	610 et seq	The following comment also applies to p. 24, section 2.5.4, ll. 799-812; p. 39, section 3.1.3, ll. 1319-21; and other places in the document. The SSA acknowledges that numerous studies have documented the relationship between outflow and the abundance of several key LFS food items (mysids and copepods), yet seems to place greater weight on the speculation that the food web effects of the overbite clam invasion have had a greater effect on LFS abundance. In contrast, it could be argued that LFS and its main food items have declined in tandem as an effect of altered hydrology, and that the outflow-related change in food availability is as great or greater a factor in the population decline of LFS than other food web changes.	6
25-6	2.6.1	854-876	I believe that Kimmerer 2002 only looked at 1987, whereas Thompson et al 2010, examining all years in the record, and Nobriga and Rosenfield 2016, examining numerous years, found no clear change point.	6
55-58	3.2.2	1873 et seq	The actual risk of extinction for LFS may be higher than stated here. See the analysis conducted by W.A. Bennett of the Bay Institute in 2019 (which I will provide to the USFWS by email).	6
47-51	3.1.8	1650 et seq	It is still reasonable to suspect that actual loss of longfin smelt resulting from SWP/CVP export operations (including predation mortality in the canals and embayments leading to the export pumps and loss of larvae which is not enumerated in the salvage estimates) is likely much larger than the number reported as salvage. The expansion value that relates salvage estimates to true losses at the pumps for related species (delta smelt) suggests actual entrainment-related direct mortality may be greater than 38 times higher than the salvage estimates. While entrainment may be a less significant factor than flow conditions in driving LFS abundance, in drier years the potential for major population effects on such a depressed population should not be underemphasized.	6
59	3.3	1977-1984	Per comments above, the evidence for a step change is less conclusive than indicated, and the loss of food is also strongly linked to altered hydrology.	6
60	3.3	2010-2015	Per comments above, the risks remaining even with current export criteria of entrainment during consecutive dry years on the viability of the LFS population could be catastrophic.	6
132		4306	The author explained very well in the technical note the potential reasons of high uncertainty, lack of key considerations in the count-based PVA, as well as the simplifying assumptions in setting up the quasi-extinction thresholds. To that end, I would suggest removingthe removal the discussion of quasi-extinction based on the count-based PVA, as it is based on a model and extinctions thresholds that are known to be uncertain and known to lack key considerations.	7
36		1183	The author made a comparison between the 20th century (1906-1999) and last two decades (2000-2020) regarding frequency of water year types, and made discussions regarding lifecycle and resiliency. While it is understandable that the comparison is limited by available 21st century data, the comparison may be inadequate on a century time scale, which might leave the discussion on resiliency ill-based. For more meaningful comparison of water year type frequency, I would suggest compiling ranges of water year type in a 20-year window from the 20th century data, and then compare the data from 2000-2020 to those ranges. In addition, water year type is an irrelevant metric for the longfin smelt lifecycle. The water year type index was developed for operational regulations, explicitly combining the prior year's hydrology with the current year's hydrology, and is therefore not a relevant metric for conditions in the Delta in any given year and has no ecological connection to longfin smelt. The water year type index is based on weighted average of Apr-Jul runoff forecast, Oct-Mar runoff, and the previous year's index. The hysteresis built into the water year type index was determined to be relevant for water supply operations and storage, but - to my knowledge - there has not been any analysis that indicates that this specific formulation of current year and prior year unimpaired flow is relevant. Instead, an analysis of the historical and projected annual trends of unimpaired flow, actual outflow, or salinity (e.g., in a similar format as Figure 4.10) would be more appropriate in the discussion of longfin smelt resiliency. As a result, I would suggest either: 1) keep the analysis of water year type frequency, with this revision suggested above, as a standalone general analysis of operational and regulatory conditions and disconnect it from the discussions and conclusions of longfin smelt resiliency, or 2) replace water year type frequency with the more ecologically meaningful metrics (e.g., unimpaired flows, actual outflows, salinity at key locations, etc.) and revise the discussion on resiliency accordingly.	7
172		5461	The logit transform in the state equation appears to be a typo, so I am hoping to get clarification on the survival state-space model. Assuming an ideal situation without errors, the age-1 abundance could be modeled as age-0 abundance multiplied by the age-0-to-age-1 survival rate. Taking log on both sides of the equation, and add an error term (on the scale of log abundance) would yield a model almost the same as presented in the document, except without the logit transform. For logit transform to make sense, the log abundance needs to always be smaller than 1 (to prevent the log of negative numbers), which essentially upper-bounds abundance. Because the remainder of the model results seem plausible, I am wondering whether the logit transform is only a typo in the document and not actually the state-space model form.	7
174		5515	The adequate number of draws to approximate posterior predictive distribution depends on the variability of the parameter posterior distributions and the algorithm for sampling. In most cases 100 posterior draws would be insufficient. It is not quite clear whether there are only 100 draws of the vital rates, or that the sampling is repeated with 100 different initial values (i.e., 2005 abundance) but each time an adequate number of draws are taken. To help readers understand how posterior estimates of abundance were made, I would suggest adding clarification on the posterior draws, and more ideally check of stationarity for posterior draws, as it has the potential to affect the discussions on projected abundance and the quasi-extinction based on the model.	7
10	2	409	It's not clear why the three covariates of outflow, temperature and conductivity were used in the growth model. What is the hypothesized mechanism relating those covariates to growth? For example, some covariate representing food availability or quality would make more sense than outflow.	8
25	2	822	It should be noted that not all spawning migrations are into the legal Delta. While the current text in this section mentions it is a discussion of environmental factors associated with "the portion" migrating into the legal Delta, it should also mention the evidence of spawning in tributaries to the Delta.	8
26	2	878-880	Clarify that the mechanisms listed are hypothesized mechanisms.	8
27	2	897-909	That entrainment tends to be lower in the higher flows years may be more a function of spawning and rearing occurring more westward in wetter years and not about high flows transporting larvae away from the pumps.	8
27	2	903-909	E:I is not a good indicator of net flow direction or entrainment risk. Net flow direction toward the pumps depends more on the flow rates on the San Joaquin River compared to export rates. In contrast, E:I is generally determined by Sacramento R flows compared to export rates. Old and Middle River flows is a better metric (Grimaldo et al 2008). This whole discussion should be replaced with a discussion of OMR flows	8
26	2	878-880	The structure of this section is confusing. Lines 878-880 make it sound like the proceeding sections are about the 4 hypothesized mechanisms behind the fish-x2 relationships. However, they are basic population needs that may or may not be related to x2 and flow effects and should be discussed that way. As currently written, the 4 sections that follow (2.6.3., 2.6.4., 2.6.5., and 2.6.6.) are too focused on the potential freshwater flow relationships which hinders a more complete picture of these population needs.	8
26	2	883-895	As an example for my previous comment on lines 878-880, transport and retention are important species needs for all life stages, not just larvae and juveniles in the LSZ. By focusing this discussion on the fish-X2 relationship in the winter-spring, the discussion is limited to larvae and juveniles and makes no mention of possible tidal surfing by adults to access spawning areas and makes access to low velocity habitat sound like somehow the higher winter-spring outflow is somehow affecting retention.	8

Page	Chapter	Line Number	Comment	Reviewer
27-28	2	911-941	This section should start with the species needs described in lines 932-941, then get into possible factors influencing that production including flow, contaminants, invasives, competitors (including clam grazing), nutrients, and land-water interactions beyond the Yolo bypass flooding that is mentioned. All of these other factors potentially effecting food production should be discussed. The flow discussion is also very one sided, cherry picking the prey relationships that respond favorably to higher outflow. There are an equal number of prey relationships that do not respond to higher outflow (Kimmerer 2002, p 47) or where there is a threshold effect of flow (e.g. Dugdale et al 2012, Est. Coast. Shelf Sci. 115:187-199). Kimmerer (2002 Effects of freshwater flow on abundance of estuarine organisms: physical effects or trophic linkages? Mar Ecol Prog Ser 243: 39–55) found that, "[i]n contrast with the higher trophic levels, chl-a and several species of zooplankton declined markedly after 1987, and had either weak responses to flow or responses that changed after 1987." There are equally good relationships between nutrients and prey abundance, as described by Glibert et al 2011, Ecological Stoichiometry, Biogeochemical cycling, invasive species, and aquatic food webs: San Francisco Estuary and Comparative Systems. Review in Fisheries Science, 19(4): 358-417. (see pages 372-376). (This paper addresses the criticisms that were raised regarding the analysis in Glibert (2010)). The relationship between flow and productivity is not unidirectional, is often weak, varies over time and space, and is complicated by the many alterations in the Bay-Delta estuary and its tributaries. The discussion presented here is far too simplistic and should be expanded upon.	8
28	2	945-952	Turbidity levels are driven by more than just outflows. If turbidity is important to longfin smelt, then this discussion also needs to be expanded to include. Schoellhamer (2011) postulated that the sudden drop in SSC during this period resulted from the depletion in the erodible pool of sediment that had been in the system until the documented step decrease. (Schoellhamer, D.H. 2011. Sudden Clearing of Estuarine Waters upon Crossing the Threshold from Transport to Supply Regulation of Sediment Transport as an Erodible Sediment Pool is Depleted: San Francisco Bay, 1999. Estuaries and Coasts. Volume 34, Number 5 (2011), 885-899) Hestir (2010) investigated the relationship between turbidity and SAV cover and found that SAV cover explains an estimated 21-70% of the trend of decreasing turbidity in the Bay-Delta. (Hestir, E. 2010. Trends in Estuarine Water Quality and Submerged Aquatic Vegetation Invasion.)	8
28	2	954-962	While higher flows can dilute some contaminants, many contaminant levels are highest following rain events (Kuivila and Hladik, 2008; Weston et al 2019; Weston et al 2015). This discussion needs to be expanded to consider the full breadth of potential contaminant impacts on longfin and their prey through their entire life cycle. The singular focus on the fish-x2 relationship prevents a full understanding of the water quality needs of the species. Weston et al 2019, Chemical and Toxicological Effects on Cache Slough after Storm-Driven Contaminants Inputs, SFEWS Weston et al 2015 Stormwater-related transport of the insecticides bifenthrin, fipronol, imidadcloprid, and chlorpyrifos into a tidal wetland, San Francisco Bay, California, Sci Total Environ 527-528: 18-25 Kuivila and Hladik 2008, Understanding the Occurrence and TRansport of Current-Use Pesticides in the San Francisco Estuary Watershed SFEWS.	8
28	2	964	The description of habitat quality needs to be expanded beyond just a description of habitat under higher flow conditions. LFS evolved in a system that had large variations in hydrology even before human modifications. LFS need quality habitat under low flow conditions as well, yet a description of that is entirely missing. In addition, this section should describe habitat quality for all life stages, not just spawning and rearing in low salinity zones.	8
28	2	966-967	Here and elsewhere, the paper seems to be limiting the LSZ to only the area where freshwater from the Delta watershed mixes with bay waters; however, low salinity zones exist elsewhere in the San Francisco Bay Estuary, including where tributaries to the Bay mix with Bay waters. These areas are also important habitat areas for LFS spawning and rearing.	8
32	3	1095-1096	Water operations are regulated by SWRCB Decision-1641 which implements the WQCP.	8
32	3	1096-1097	D-1641 has more than E:I and minimum outflow requirements, it also has export limits from April 15 -May 15, Sacramento River at Rio Vista and San Joaquin River at Vernalis flow objectives and salinity objectives that must be met.	8
32	3	1104-1105	The statement that, "the months of April and May are crucial outflow months for juvenile longfin smelt" needs to include a citation to the supporting research.	8
32-33	3	1114-1116	The description of the spring outflow action in the ITP is not entirely accurate and should be revised.	8
34	3	1138-1140	California's water infrastructure dates back at least to the 1850s when Delta wetlands were drained and leveed. Diversions have impaired flows dating back to the 1800s as well. And, dams were constructed in the Delta watershed before the SWP/CVP dams (e.g. Hetch Hetchy reservoir was built in the 1920s, La Grange reservoir on the Tuolumne was built in 1890s, Pardee on Mokelumne in 1920s, Lake Spaulding on the Yuba in 1910s, Yosemite Dam on Merced in 1880s)	8
29	2	1007-1014	The unpublished, unpeer reviewed analysis by the Bay Institute and NRDC described here was heavily criticized by an independent panel convened by the Delta Stewardship Council in Reed et al 2014, Workshop on Delta Outflows and Related Stressors Panel Summary Report (p 35-37). Based on those criticisms, this text should be eliminated from the report. Even if you take into account the uncertainty in the flow estimates that the panel points out, there remains the uncertainty in the assignment of positive versus negative population growth for each year.	8
37	3	1215-1218	Outflow trends depend on the period of record evaluated. Hutton et al (2017) evaluated outflow trends over 9 decades and found a trend of increasing outflow in July and August, decreased outflow in Feb, April, May and Nov, no trend in Sep and Oct (contrary to Cloern/Jassby finding cited in these lines), and no trend in annual outflows. (Hutton, Rath, Roy. 2017. Freshwater flow to the San Francisco Bay-Delta estuary over nine decades (Part 1): Trend evaluation. Hydrological Processes 1-17).	8
37	3	1222-1224	While LFS abundance is correlated with freshwater flow, freshwater flow, in and of itself, may not the most important species need, as this sentence states. Suggest rewording to state the ecological processes and conditions created by freshwater flows are what is important to LFS. This is an important distinction when it comes to thinking about how to improve conditions for LFS and that distinction should be made here. For example, more reservoir releases may not help LFS if they need storm driven turbidity or if they need increased prey density from improved land-water interface.	8
37	3	1225-1228	First, the FMWT has never measured abundance, as this sentence states. The FMWT indices are not abundance estimates. Second, there needs to be some discussion of the bay surveys and that none of the surveys sample the full range of LFS occurrence (see Merz, Bergman, Melgo, Hamilton 2013. Longfin smelt: spatial dynamics and ontogeny in the San Francisco Estuary, California. California Fish and Game 99(3): 122-148).	8
39-40	3	1314-1330	When discussing study findings regarding X2 position, suggest including the averaging period for X2 (i.e. Mar-May) since many of the studies cited use a different averaging period.	8
40	3	1332-1348	More recent research (e.g. Fong et al 2016), has shown significant correlations between pyrethroid use and declining abundance of POD fish species and their prey items. This research should be included here. Fong, Louie, Werner, Davis, Connon. 2016. Contaminant Effects on California Bay-Delta Species and Human Health. SFEWS 14(4)	8
39	3	1355-1357	This sentence doesn't make sense. First, LFS haven't been impacted by POD, they are one of the POD species. Second, is "representation" the correct word choice? Last, what change is "this change" referring to? if it's the negative changes in biotic and abiotic factors described in previous sentence, then it should be modified to "these changes."	8
40-41	3	1366-1368	Discussion of water temperatures in the Delta need to be spatially explicit. Temperatures across the Delta vary dramatically. In addition, LFS do not occur within all parts of the Delta during all seasons. Summer temperatures in the central and southern Delta are unlikely to have any impact on LFS since they do not occupy that area during summer months.	8
42	3	1421-1423	The 2019 USFWS biological opinion also carries forward a requirement to restore 8,000 acres of tidal wetlands for delta smelt. Some portion of that restoration should also benefit LFS.	8
42	3	1430-1433	This sentence about the influence of freshwater flow on age-0 fish is out of place since the rest of the paragraph is about predators and no where does the rest of the paragraph connect predator abundance to freshwater flows.	8
42	3	1432	Menidia beryllina is a brackish, nonnative fish that has increased in abundance and has been shown to prey on delta smelt so it wouldn't be unreasonable to hypothesize a potential role in LFS predation.	8
42	3	1435-1437	There have been a whole lot of other statements and analyses mentioned that are equally or more speculative than the text that follows regarding observations of predation on LFS in this estuary. Why include this statement here?	8
43	3	1471-1477	It should be noted that entrainment and salvage of LFS in Clifton Court Forebay and the export facilities has been very low in recent years due requirements in the 2008/09 and 2019/20 biological opinions and ITPs.	8
43	3	1493-1495	I'm not aware of any study that links water diversions to increased water clarity. Increased water clarity is due to decreased sediment load (Schoellhamer 2011) and increased SAV (Hestir 2010 and Hestir et al 2016). Schoellhamer 2011. Sudden clearing of estuarine waters upon crossing the threshold from transport to supply regulation of sediment transport as an erodible sediment pool is depleted: SFB, 1999. Estuaries and Coasts). Hestir, E. 2010. Trends in Estuarine Water Quality and Submerged Aquatic Vegetation Invasion. UC-Davis Dissertation. Hestir, Schoellhamer, Greenberg, Morgan-King, Ustin. 2016. The effect of submerged aquatic vegetation expansion on a declining turbidity trend in the Sacramento-San Joaquin River Delta. Estuaries and Coasts, 39:1100-1112.	8
44	3	1509	SWRCB 2010 is not the most current 303d list. EPA approved the 2018 California Integrated Report (Clean Water Act Section 303(d) List and 305(b) Report on June 9, 2021. https://www.waterboards.ca.gov/water_issues/programs/water_quality_assessment/2018_integrated_report.html	8

Page	Chapter	Line Number	Comment	Reviewer
45	3	1572-1574	This sentence makes no sense as written. Chlorophyll declines are not the cause of productivity declines - they are a measure of it. I suspect this sentence is trying to say that the Foe et al study found that chlorophyll/productivity declined upstream of the SRWTP and thus the SRWTP may not be the only contributor to low productivity in this region.	8
46	3	1576-1578	Müller-Solger, et al 2002 also links primary productivity to higher trophic levels in the Bay-Delta (p1472-1475). Müller-Solger, A., A.D. Jassby and D.C. Müller-Navarra. 2002. Nutritional quality of food resources for zooplankton (Daphnia) in a tidal freshwater system (Sacramento-San Joaquin River Delta). Limnol Oceanogr 47(5):1468-1476	8
45-46	3	1556-1581	In addition to ammonium impacts on productivity, nutrient stoichiometry, the ratios and forms of nutrients, can play a large role in both community composition and total productivity. See e.g. Glibert et al 2011 and Glibert et al 2013). (These are not the highly criticized paper by Glibert (Glibert 2010)). Ammonium may also play a role in community composition, providing a competitive advantage to species like Microcystis (see 244.Lehman PW, C Kendall, MA Guerin, MB Young, SR Silva, GL Boyer, and SJ Teh. 2014. Characterization of the Microcystis Bloom and Its Nitrogen Supply in San Francisco Estuary Using Stable Isotopes. Estuaries and Coasts. DOI 10.1007/s12237-014-9811-8) Glibert et al 2011, pages 373, 374, 378, 379, 380, 381, 382 show and explain relationships between nutrients and longfin and their food. These findings should also be described. Glibert, P.M., D. Fullerton, J.M. Burkholder, J.C. Cornwell, and T.M. Kana. 2011. Ecological stoichiometry, biogeochemical cycling, invasive species, and aquatic food webs: San Francisco Estuary and comparative systems. Reviews in Fisheries Science, 19(4):1-60 Glibert, PM, TM Kana, and K Brown. 2013. From limitation to excess: the consequences of substrate excess and stoichiometry for phytoplankton physiology, trophodynamics and biogeochemistry, and the implications for modeling. Journal of Marine Systems, 125:14-28	8
48	3	1679-1682	The number of LFS salvaged at CVP and SWP between 1993 and 2007 is no longer relevant since the projects have been operating to new rules since that time that have significantly reduced the number of LFS counted in salvage. Suggest either including the more recent salvage count or an explanation of the change in rules and how it has reduced salvage numbers.	8
50	3	1745-1746	LFS don't only aggregate near X2 during the spawning season. Include newer research which finds LFS in other areas of the estuary during spawning season. (Merz et al 2013)	8
50	3	1746-1748	While Lake Washington populations of LFS are known to make night migrations to spawn, I'm not aware of any evidence that SFE behave similarly. Include a citation to support this statement. Lack of occurrence in salvage may indicate this strategy is not employed by SFE LFS.	8
50	3	1748-1750	While there may be some reversal in flow in the upper Cache Slough, I'm not aware of any evidence that this has increased entrainment of LFS. If this is referring to entrainment of LFS at the Barker Pumping Plant, then this should be clarified and data on the actual number of LFS detected in facility monitoring should be included.	8
50	3	1757-1759	I don't believe this is the correct caption for the figure	8
59	3	1967-1968	Since we do not know the mechanisms by which freshwater flow contributes to LFS needs, and we are learning that Bay tributaries flows are also important, suggest revising this sentence to the following: Freshwater flow to the estuary, or the conditions that it creates, is the most important species need.	8
59	3	1977-1984	Changes in the pelagic food web were also coincident with changes in nutrients. The clam invasion was definitely a significant event that created a major shift in the food web; however, there is also a hypothesis that changes in the nutrient regime may have provided the conditions that allowed the clam to establish in the first place. (See Glibert et al 2011, p.388-389, 396-397, and throughout. 133.Glibert, P.M., D. Fullerton, J.M. Burkholder, J.C. Cornwell, and T.M. Kana. 2011. Ecological stoichiometry, biogeochemical cycling, invasive species, and aquatic food webs: San Francisco Estuary and comparative systems. Reviews in Fisheries Science, 19(4):1-60	8
59	3	1986-1992	Should add observation of LFS response to salt pond restoration in southern SFB as further evidence of importance of tidal marsh, and the loss thereof, to LFS	8
59-60	3	1998-2002	Suggest adding observation that LFS appear to have shifted to deeper waters based on Bay Study comparisons of catch between MWT and Otter Trawl. Perhaps this is in response to warming temperatures.	8
61	3	2024-2027	The table above describes several high risk stressors that are unrelated to SWP/CVP operations (clam, loss of food, loss of tidal habitat, increasing temperature), or only partially related to SWP/CVP (reduced freshwater flow). Based on that, the conclusion here that construction of the SWP/CVP was the cause of the decline is not supported.	8
63	4	2081-2084	Loss of food and loss of tidal habitat were also described as the highest magnitude threats and should be included here since future scenarios could address these stressors as well. While invasive clams may have been one cause for loss of food, there are other hypotheses that should be considered and therefore this description should not be limited to only the invasive species component of food loss.	8
66	4	2227-2229	Increased frequency and severity of storms and floods could result in improvements in habitat suitability, not reductions.	8
68	4	2295	Catch data from Bay study indicates adults are also present in other areas of the bay, not just those listed here.	8
77	4	2445	Freshwater flow may be the most important correlate to abundance; however, the actual driver may not be freshwater flow itself, rather the conditions that freshwater flow and/or wet years creates. This sentence should be re-worded to capture that distinction.	8
81	4	2546-2548	What evidence supports the statement that higher flows prior to April may be detrimental by pushing eggs and larvae into SFB? None of the previous statements indicated there was a threshold of flow, below which is beneficial and above which is detrimental. This statement also conflicts with the statement on lines 2555-2557	8
81	4	2561-2562	This statement is more definitive than the data support. Suggest rewording to larval productivity may continue to decline.	8
82	4	2586-2587	Again, freshwater flow, in and of itself, may not be the most important species need. Until we know the mechanism, this statement, and others like it, should be reworded to the conditions that are created by high freshwater flows are the most important species need.	8
83	4	2613-2615	The statement that the species has always displayed low resistance against drought should be revised to include a temporal aspect. For the time period during which we have survey data, 1960s to present, the species has displayed low resistance to droughts. Clearly, if the species has survived the megadroughts in figure 4.14, it used to have the capacity to weather droughts better than it can today, indicating that flow regulation alone is not the only stressor limiting productivity. In addition to changes in water routing and management, another huge difference between then and now is the amount of tidal wetlands and the habitat conditions they created, including food productivity.	8
84	4	2643-2644	The reasons for the decline in the latter portion of the century are not as certain as this statement makes it sound. Plus the step declines are not a reason for the decline, they are a measure of it. Additional potential reasons for the decline include food quality and quantity. Reword this statement to capture the uncertainty and that there are other potential reasons for the decline that are discussed elsewhere.	8
89	4	2797-2800	Merz et al 2013 found a much broader distribution of all life stages when they looked at all surveys data combined, than the distribution indicated by individual surveys, none of which capture the full range of LFS across their entire life history. They found that adults during the spawning period already occur in upper Delta reaches and in SF Bay, indicating that shifts in X2 may not be as detrimental to spawning adults as this section indicates. In contrast, Merz et al 2013 findings support the observation that juvenile LFS distribution fluctuates in relation to the low salinity zone. Merz, JE, PS Bergman, JF Melgo and S Hamilton. 2013. Longfinsmelt: spatial dynamics and ontogeny in the San Francisco Estuary, California. California Fish and Game, 99(3): 122-148	8
90	4	2830-2832	Salinity intrusion can also be abated by strategically located tidal wetland restoration that changes the tidal prism/energy.	8
91	4	2832-2833	See previous comment regarding distribution of adult LFS (p 89 lines 2797-2800).	8
91	4	2843-2851	Glibert et al 2011 also discuss how changes in the nutrient regime has contributed to invasions and shifting community composition within the Delta. Glibert, P.M., D. Fullerton, J.M. Burkholder, J.C. Cornwell, and T.M. Kana. 2011. Ecological stoichiometry, biogeochemical cycling, invasive species, and aquatic food webs: San Francisco Estuary and comparative systems. Reviews in Fisheries Science, 19(4):1-60	8
91	4	2853-2860	Suggest broadening this section title to future nonnative and and invasive species. Not all nonnative species are invasive. Line 2856 talks about striped bass which are nonnative, but many would not describe it as invasive. Also, suggest adding a description of the proliferation of Microcystis and it's associated toxins and their potential impacts on LFS and their prey. Microcystis is another species that may increase in frequency and magnitude as the Delta warms.	8
91	4	2854-2855	Assuming this section is talking about invasive and non-invasive nonnative species, Pseudodiaptomus forbesi is an example of a nonnative species that may have had a positive effect on LFS	8
91-92	4	2864-2875	New species are being introduced all the time, not all of them become established. A warming climate may increase or change the species composition of those that are able to become established. That is a far more likely mechanism for future invasions than an increase in tourism to an area that is already a tourist destination. In addition, not only might survival rates of organisms in ballast water change with climate change, a warmer SFB and Delta may make conditions more hospitable for the establishment of those transported organisms.	8
100	4	3051	The table and future conditions discussion describes loss of tidal marshes as a result of sea level rise. This potential negative impact should be countered with a description of the currently planned tidal marsh restoration projects.	8
100	4	3068-3070	Provide citation to support the conclusion that "current impaired April-June flows are already insufficient for LFS productivity."	8
125	TN1	4183-4187	Might an alternative hypothesis be that greater than 1 growth rate indicated by the age-1 otter trawl index is due to a shift in the vertical distribution of the species?	8

Page	Chapter	Line Number	Comment	Reviewer
overarching			This paper would benefit from a more thorough examination of the role of marsh habitat historically, under current conditions, and potential futures. Unlike changes in temperature and sea level rise, tidal habitat restoration is an area that can be managed. LFS have been observed in restored habitat in the S Bay, and elsewhere. Levi Lewis's presentation on 8/27/2021, slide 32 lists multiple references for "Bay-wide spawning & use of wetlands. The loss of >95% of tidal marsh is mentioned but deserves further elaboration.	8
		all	voice varies throughout the document, a final revision is necessary to create a more uniform writing style for clarity throughout.	9
	2	140	I havenever seen a pinkish longfin. I've only ever seen them with purple hues	9
		167	https://nrm.dfg.ca.gov/FileHandler.ashx?DocumentID=152480&inline	9
	2	218	You could also cite more recent community analyses like Castillo et al on SKT and 20-mm Surveys	9
	2	250	is there a more recent publication you can cite? We've learned a lot in 20 years, plus there is strategy as to what year LFS spawn.	9
	2	319	Been, not be	9
	2	330	I don't think it's appropriate to surmise that they lay eggs in sand or mud, because we really have no idea. Unless that is a conclusive finding from Lake Washington, but the sentence needs more clarity if that's what you are referring to.	9
	2	361	I don't really understand this paragraph. 4.4 mm seems way too small for Longfin based on SLS data. In SLS, we typically do not see larvae less than 6 or 7 mm, and they retain their yolksac until 9 or 10 mm TL. Unless SLS is missing the youngest larvae, something doesn't seem right here. I would suggest pulling in more recent unpublished data to help this paragraph. I also think unpublished data would be useful earlier in the document, but it seems as though unpublished data was avoided up until this paragraph. Lots of unpublished data exists so to be consistent it should either not be included or used more frequently.	9
	2	373	I think you mean that yolk sac presence is not an approximator of age? It sounds as if you are assuming that yolk sacs are absorbed at a consistent rate, but I do not think that's accurate. Yolk sacs are absorbed at various rates based on food quality and availability, so is likely not a good indicator of age. I'm not sure length is either, but I have the assumption that length is better than yolk sac presence, do we have otolith data to suggest growth rates vary at this life stage? I think this statement needs clarity or more information.	9
	2	419	Saying 'we predict' sounds like a hypothesis. Are these model predictions?	9
	2	438	I'm not sure how the cohorts were handled in the model, but the model doesn't account for cohorts, this is very likely to be an issue. This is a screenshot from the 20-mm Survey webpage, showing Longfin smelt length frequency biweekly from March to June (x axis is total length in mm).	9
	2	458	It would be good to include notes throughout the document about where to find definitions or background information. For example, X2 and the 3R's. They are discussed earlier in the document, and then referred to in later sections, but that requires someone to read the document in it's entirety or use the search functions. It would be better to make notes about where to find background info or definitions on specific topics).	9
	2	504	As a measurement: 20 mm As a survey title: 20-mm Survey	9
	2	539	This is worded oddly. Catches don't favor one net or another. Catch increases or decreases, or the ratio changes.	9
	2	550	Rather than using parenthetical examples, it's more clear to write, "It is unclear what may have caused this change, but tubidity and prey changes are likely factors."	9
	2	629	Can this whole section be moved up earlier in the the document	9
	2	654	The last 2 sentences of this paragraph are strange. Either leave out recruitment and say something like, 'Since 20-mm starts in mid-March, it may miss cohorts of Longfin Smelt" or chnge the last sentence to something like "Gear recruitment is defined as fishing reaching the appropriate size and shape to be effectively retained by the gear."	9
	2	715	It's important to note that Longfin smelt in the SFE are at the most southern end of their historical range, indicating this is the warmest area they are found. https://nrm.dfg.ca.gov/FileHandler.ashx?DocumentID=152480&inline	9
	2	726	Or that this is the average range of temperatures in the SFE from Nov-March? We don't typically see temperatures outside this range in the winter, what about other LFS populations?	9
	2	750	How many years of data did this paper cover? Was this compared to longterm monitoring? It's apparent that there is lack of published reports available, and it would be ideal if raw data and more technical reports were created throughout chapter 2 to support claims.	9
	2	987-991	How about the distribution of larvae? Larvae are more broadly distributed that adults and indicate more available spawning locations	9
	2	1039	parenthetical needs to be addressed	9
	3 and 4		You reference the 3 R's but not how they fit into the broader framework. Either link to the info at the beginning of the document, or provide more context on how it all fits together.	9
	3 and 4		The approach to Bay Delta tribs was lacking. Successful recrutment in Bay tribs only occurs under rare conditions that will become even more rare with sea level rise and increased water temp. Both chapers would benefit from more anaysis and discussion regarding these tribs and their potential contribution to the population, or lack thereof.	9
		1047	Bay-Delta DPS may act as source for other populations farther north. While the focus of this status assessment is the Bay-Delta DPS, the authors should consider and provide brief discussion of potential ramifications for other populations.	9
		1078	Authors should discuss hydrologic effects within the Delta, i.e. the prevalence of net negative flow in Old and Middle Rivers (OMR in CDEC) and net negative flow past Jersey Point (Qwest in DayFlow) due to exports. In addition to altering the timing magnitude and duration of flow, exports have also changed the direction of flow and residence time in areas where Longfin Smelt rear and spawn. This contributes both to direct loss due to entrainment into the export facilities, retention in poor habitat and poorly understood/undescribed impacts to habitat (potentially includes entrainment of prey items, reduced turbidity, and reduced primary productivity).	9
		1138	Add language describing the increase in export capacity associated with the completion of SWP south Delta export facilities in 1968.	9
		1158	The effect of successive bad years is apparent in figure 3.2. The authors could compare odd year abundance patterns depicted in figure 3.2 to support the assertion that consecutive bad years may amplify the effects of dry conditions.	9
		1170	The authors should add discussion about how water year types are classified and how exports may mute effect of wet, above normal, and below normal water year types. The Sacramento and San Joaquin Valley water year type indices are based on inflow to reservoirs. However, the conditions experienced in the Delta may not accurately reflect water year type classification. Operations result in consistent Delta Outflow to meet SWRCB regulations resulting in drought like conditions in fall and early winter unless precipitation is sufficient to increase outflow beyond the influence of exports.	9
		1189	Flow required for population growth has been calculated by The Bay Institute (TBI) and State Water Resources Control Board (SWRCB). TBI determined a 50% probability of positive Longfin Smelt population growth associated with 51,000 cfs January through March or 35,000 cfs March through May. SWRCB conducted similar analysis and found that 50% probability of population growth was associated with 42,800 cfs January through June. The authors could compare outflow in above normal years with the values these values to confirm that flows observed in below normal years are conducive to positive population growth.	9
		1238	LFS recruitment occurs in Bay tributaries only in exceptionally wet years. Successful recruitment in these regions is rare. The authors may want to revise this sentence to reflect the rarity of successful recruitment in Bay tributaries to support the assertion that the Bay Delta DPS is especially vulnerable to catastrophic events in all but a few rare years.	9
		1362-1364	Typo? The temperature at which the spawning migration occurs is well below 22°C. Analysis conducted for the SWP ITP showed that spawning migration, as indexed by catch at Chipps Island, coincides with water temperatures at or below 12°C.	9
		1405	Lewis 2019 found that successful rearing in Bay tributaries only occurred in extremely wet years. In most years this habitat is too warm or too salty to be good larval rearing habitat. Add language to acknowledge these limitations.	9
		1657	Salvage of adult Longfin Smelt is extremely rare in the post POD era. This could be due to substantially reduced population or a shift in habitat use associated with poor conditions in the south and central Delta.	9
		1735	The 2019 PA does not include protections for Longfin Smelt or any regulations related to Barker Slough pumping plant operations. The authors should distinguish between these two documents as there is a substantial gap between the protections in the PA and those in the ITP. Regarding Longfin Smelt, the PA may provide ancillary protections by limiting OMR to be no more negative than -5000 cfs. Beyond that it is not expected to reduce entrainment or loss of Longfin Smelt.	9
		1781	The authors may want to add a caveat that 20-mm does not efficiently capture newly hatched larvae and that it is a better index for older larvae.	9
		1969	Suggest using 'low outflow years' in place of 'dry years' to avoid confusion with water year type designations.	9
		1973	As mentioned in other comments, successful recruitment in Bay tributaries is rare and is not likely to make substantial contributions to the population except in rare extremely wet years.	9
		1977	Authors should add discussion of how reduced outflow also results in reduced abundance of prey items.	9
		2017	Impacts to larval Longfin Smelt described in this table should include retention in, or entrainment into poor habitat. Net negative flow past Jersey Point (Qwest) may prevent downstream dispersal. The magnitude of this impact is unknown but may be equal to or greater than loss to entrainment of larvae into the export facilities.	9
		2027	Authors should add that good habitat conditions need to occur frequently enough to offset losses in bad years, and that the population must increase substantially when good habitat is available for it to persist through prolonged periods when conditions are not conducive to population growth.	9
		2221	This paragraph is a bit disjointed and touches on ideas addressed elsewhere. I suggest the authors revise this paragraph to summarize/set up the following section by listing the stressors described here in the order they are addressed in the following sections.	9
		2243	see previous comments regarding availability of rearing habitat in Bay tributaries.	9
		2256	The authors should provide details regarding how daily average air temperatures calculated from Wulff et. al. is converted to water temperature. If a one-to-one relationship was assumed, the authors should describe possible errors associated with this assumption	9
		2263	define acronyms GCM and RCP and explain significance of numbers 4.5 and 8.5.	9
		2302	Provide measure of variability around mean number of days in which projected temperatures exceeded 22°C.	9
		2315	Suggest using juveniles instead of subadult. The impacts described here appear to be greatest on young of year Longfin Smelt. Subadult could be misinterpreted as referring to older fish that are returning but have not yet matured.	9
		2413	This description of optimal temperatures for larval rearing should come before the results.	9
		2425	Authors should describe potential impacts to Bay tributary rearing habitat. These smaller systems are likely more susceptible to increases in water temperature and salinity intrusion associated with climate change.	9
		2464	It is unclear what representation means in the context of this sentence. Suggest revising for clarity.	9
		2696	Include units for all numbers. i.e. 0.6 to 1.1 feet.	9
		2707	Define acronym MSL	9

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		2745	The connection between larval rearing habitat and deeply subsided islands in unclear. If a large, subsided island floods due to catastrophic levy failure, the immediate effect would be increased salinity intrusion and a substantial, short-term change in hydrology as bay water moves in to fill the void. After this initial change, the flooded island potentially could be rearing habitat for multiple species, including predators or undesirable species, if salinity and temperature were suitable. Reclaiming a flooded island would either reduce or have no effect on available habitat. Historically, converting flooded islands for agriculture or other uses has been a major cause of habitat loss. The authors should reassess the potential effects of levy failure and revise this paragraph accordingly.	9

Comment Codes	Reviewer 1 Comment
W1:	Draft does not explicitly identify or adequately explain level of certainty
W2:	Draft is based on insufficient scientific modeling, as required to support "[e]xplicit logic chains" and ensure "an explicit, transparent and, therefore, repeatable method of analysis, which supports peer review of both the methodology and the conclusions."
W3:	Draft does not represent "best available biological information on the species (taxonomy, life history, and habitat) and its ecological needs at the individual, population, and species levels based on how environmental factors are understood to act on the species and its habitat."
W4:	Draft does not adequately address "probable explanations for past and ongoing changes in abundance and distribution within the species' ecological settings."
W5:	Draft does not provide "clear chain of logic for how factors affect the species or the resources it needs" or, "conversely show[] why a factor does not or cannot affect [the] species."
W6:	Draft does not adequately explain species' "current population structure, distribution, abundance, demographic rates, genetics, and habitat/resources."
W7:	Draft does not adequately explain "changes from historical to current distribution."
W8:	Draft does not adequately explain "the causes and effects that resulted in the current species' condition with respect to the life history and habitat needs identified in Stage 1" of the SSA process.
W9:	Draft does not adequately explain "the implications of any missing or diminished resources or circumstances affecting the demographic parameters at the population level, and the number, distribution, and connectivity of populations within the species' ecological settings."
W10:	Draft does not adequately account for "conservation efforts that are likely to occur," or the species' likely responses to future environmental changes.