External Peer Review

Peer review was solicited from experts in rockfish biology and conservation biology. Six individuals agreed to provide reviews, although we only actually received four reviews. Reviewers were: Dr. Milton Love (University of California, Santa Barbara), Dr. Donald Gunderson (University of Washington), Dr. Leah Gerber (Arizona State University), Dr. J. Wilson White (University of California, Davis). Reviews are provided below (in no particular order and not associated with a particular reviewer).

Reviewer 1

Six factors are listed as considerations by NMFS in listing decisions. How do these differ from the 5 factors specified in the ESA? In general the narrative that goes with each of the 6 factors considered by NMFS is very general and not specific to rockfish. Maybe this is because data are lacking, but it could be condensed and focused on including key information (or mention that there are no data in each category). E.g., describe trend data that are available

Pg. 52 and beyond: should "absolute numbers" be "absolute abundance"? Also, how can one say anything about extinction risk without having an idea of abundance (even with wide CI's)?

Pg. 56: It sounds like "expert opinion" was used to rank demographic risk. This could be strengthened if bit more attention is given to the methods in ranking criteria. Social scientists worry about things like uncertainty in how various metrics are ranked. This is not my field but I have been scrutinized in similar endeavors. Ben Halpern and Carrie Kappell have some published approaches, and I am sure a look at the social science literature could strengthen this approach.

I am skeptical about applying a trend analysis to "total rockfish" (i.e., applying analysis to combined species). Given the paucity of data, I can see why this approach was employed, but it seems like it could be misleading if the relative trends are different among species (which seems likely).

Fig. 33 legend indicates that different colors were used to show which data were treated as separate pops, but I am only seeing one color. The legend also is a little unclear (some typos), so it's hard to figure out what the figs represent. I think the figure is showing trends for combined data, which has merit because it confirms that these populations seem to be declining. But I am still hesitant about performing any kind of extinction risk analysis for combined species data for the reason mentioned above.

I realize that a lot of creative work went into these analyses, and they are really impressive given that there are so few data. The analyses certainly provide a strong basis for taking management action for these

species (e.g., listing under the ESA). When you start developing a recovery plan and criteria, you could consider a more general approach that does not rely on quantitative analyses that are potentially flawed due to sparse data. A general approach would set recovery criteria and objectives independent of data in hand, and then specify that, species should be listed until data are available to show recovery (i.e, a precautionary approach). It might be worthwhile to consider the recently developed white abalone recovery plan: http://www.nmfs.noaa.gov/pr/pdfs/recovery/whiteabalone.pdf.

Reviewer--2

Dear Dr. Levin,

As you requested, I have reviewed the draft ESA status review of Puget Sound rockfish. Overall, I found the status review to be soundly reasoned and to employ the best available statistical techniques and scientific interpretation to the available data on these species. I must agree with the Biological Review Team (BRT) that the data available for this type of evaluation are sorely lacking, especially in terms of genetic evidence for evaluating the DPS determinations and in terms of the abundance data available for examining population trends and extinction risk. However, it appears that the BRT has been careful in extrapolating from the available data to make conservative inferences regarding the five species in question.

At your request, I paid special attention to the evaluation of extinction risk in my review. There are two potential difficulties with this analysis. First is that abundance data for the individual species are not sufficient for independent analysis, particularly for species that historically made up a small proportion of the overall rockfish fishery and for species that have had catch restrictions imposed and thus have no recent fishery-dependent data. The remedy for this difficulty proposed by the BRT is to analyze the long-term trend in overall rockfish abundance, recognizing that this trend is driven primarily by more abundant species but could serve as an estimate of the trend in the rarer petitioned species. Clearly this is quite a striking assumption, and any number of factors could produce violations of the assumption. However, this approach should produce a conservative estimate of the trend in the petitioned species (i.e., the actual trend is probably more negative than that identified here). Changes in gear and switches in the targeted species should tend to prolong high catch levels in a multispecies time series, so an observed decline in overall catch probably reflects steeper declines in the actual abundance of individual fishes. Furthermore, in the petitioned species for which data were available, the proportion of the overall catch appears to have declined in recent years. Provided that this does not reflect a change in the species-specific targeting of the recreational fisheries (which seems unlikely), this suggests that the multispecies data would again be an optimistic estimate of the rate of decline. One caveat should be that the overall rockfish trend should not necessarily be representative of species that are completely closed to fishing, such as canary rockfish since 2002. The BRT notes that there was not a trend for declining canary rockfish abundance since 2002 in the REEF surveys; has there been any evidence for a temporal increase in those surveys?

The second difficulty in the extincton risk analysis is the presence of multiple data sources that should have different error structures and different relationships to the overall mean abundance. The state-space model approach taken by the BRT is an excellent solution to the problem, and they appear to have chosen appropriate specifications for the model. The model fits to the data appear quite good (although considering the number of potential parameters in the model, a good fit is to be expected). My largest concern with this modeling approach is that it assumes a temporally stationary population growth rate (parameter a). This presumes that the overall rockfish "population" is either declining to extinction ($a < 0$) or rising to a non-zero steady-state equilibrium ($a > 0$). The model is therefore constrained to indentify an overall negative or positive trend in the data, and cannot represent behavior such as a decline to a minimum followed by subsequent increase. This assumption is probably reasonable for the current state of the Puget sound rockfish community. However, if some sort of intervention were to change the demographics in the study region, such as a large scale change in fishing regulations or environmental mitigation in response to ESA classification, this same analysis would be inappropriate for the analysis of future datasets. Rather than identifying a deterministic change in recent abundances, this type of model would fit positive observation and process error terms to those recent years. To deal with that situation, future analyses would require a temporally variable parameter a, or a three-parameter model that includes an intrinsic growth parameter and an exploitation parameter. I add this comment not necessarily as a criticism of the current approach (after all, there is pretty convincing evidence for a long-term decline in Fig. 36-37, even without the model fits), but rather as a clarification that should be made for future investigators who may desire to revisit this analysis in the future.

In addition to these major comments, I had a number of specific comments that I itemize below.

p. 16: "myctophids" is misspelled

p. 27, second paragraph: probably better to say that eelgrass and seaweed habitat support most "species" in the sound, rather than most "populations."

p. 38: This is a very nice explanation of the benefits of mitochondrial DNA evidence. I recommend clarifying at some point in this discussion that microsatellites are nuclear markers.

p. 41: With respect to the comparison with other rockfish species in Puget sound, this seems like a very reasonable approach for making inferences regarding the genetic structure of the petitioned species. I would add that given the apparent level of development and swimming abilities of rockfish larvae and post-larvae, it is probably reasonable to err on the side of shorter rather than longer larval dispersal distances.

p. 42: It is not clear what should be made of the comparison with the non-rockfish species in the sound. Are there reasons, based on spawning behavior, spawning season, or larval life history, that the petitioned rockfish species should be more or less like any of the non-rockfish species mentioned here?

p. 45: "were" not "where"

p. 69: Summary of methods: rephrase as "...using a maximum likelihood approach to fit..."

p. 70: Ward et al. 2008 and Shumway & Stoffer 2006 are missing from the references.

p. 70: To be clear, R is constant with time, and D represents fixed factors, not random variates, correct?

p. 73: What sort of model selection was used to compare the one vs. multiple population process models?

p. 76: It is reasonable to point out the importance of ontogenetic changes in fecundity, but there is no reason that those variations cannot be included in a calculation of LEP, and LEP can then be used in the standard way (comparison to a biological reference point or to the slope of the stock-recruit curve). It is correct that variation in the relationship between biomass and fecundity would invalidate the use of SSB. On the same point, is the issue of the seasonal timing of reproduction relevant to Puget sound populations? I am only familiar with that type of argument being used in open coast populations for which the timing of spawning relative to upwelling transitions, etc. is very important.

p. 76. On the topic of LEP, I was somewhat surprised that exinction risk analysis did not incorporate some of the traditional approaches to evaluating population persistence, such as a comparison of LEP to the compensation ratio. Since the relative value of those two measures is a fundamental measure of the long-term persistence of a population, that comparison would be very useful. Are estimates of these values not available for the petitioned species?

Overall, I found this status review to be a sensible and well-reasoned analysis given the limited data available.

Reviewer 3

I found this to be a comprehensive review of the status of the rockfish stocks in question. This was a difficult undertaking, since the review team had very little data to work with.

However, the lack of genetic studies pertinent to the species of concern led the team to generalize from what is known about other rockfish, notably the more well-studied copper, quillback and brown rockfish. All three of these species are known to mate and spawn in Puget Sound, and to be highly restricted in their migrations as adults. Recent genetic analyses suggest that many of their larvae may drift no more than a few tens of kilometers after parturition. This is probably not the case for bocaccio or canary rockfish, and a different model is more appropriate.

Bocaccio and canary rockfish probably have a life history that is more similar to species like yellowtail or splitnose rockfish. Both have large populations in offshore waters, and Puget Sound is likely a sink for larvae that drift in. While splitnose larvae are commonly found in drifting kelp mats, adults are rare throughout most of Puget Sound proper. Most yellowtail rockfish in Puget Sound are immature, migrating to the ocean once they reach sexual maturity (see Barker and Mathews 1983, Gunderson and Vetter 2006). I am not aware of any reported mating or spawning of splitnose or yellowtail rockfish in either Puget Sound proper or the San Juan Islands.

Without further genetic study, and documentation of both mating and spawning in Puget Sound, there is no reason to suppose that the bocaccio and canary rockfish there consitute a population segment that is distinct from the ocean population. Populations within Puget Sound are more likely to be sink populations that exist only through sporadic recruitment of larvae from ocean sources.

Despite the lack of genetic analyses for yelloweye rockfish in Puget Sound proper, the Canadian work cited (Yamanaka et al.) is consistent with the existence of distinct population segments in the ocean and protected waters. However, we still have no way of knowing if the yellowtail population in Puget Sound constitutes a population segment that is distinct from fish in the

Strait of Georgia. If the yellowtail in Puget Sound constitute a DPS, the teams assessment probably understates the degree to which they are at risk.

The team's analysis indicates that yelloweye rockfish were once plentiful in greater Puget Sound (Kincaid 1919). This is consistent with Yamanaka et al. 2006, who cited an 1886 report of "plentiful" "large red rock cod" in the Strait of Georgia, the most abundant and highly prized being "the red cod or snapper". A century of commercial and recreational fishing on this highly prized species has taken an enormous toll on the yellowtail rockfish population, yet the review team is unable to determine the true magnitude of the decline given data that goes no further back than the 1980s. This is the "sliding baseline" phenomenon in one of its clearest manifestations.

Rather than rely on recreational catch data of uncertain reliability, or bottom trawl surveys that don't target the rocky reefs inhabited by yelloweye rockfish, it would be better to estimate current population distribution and numbers directly, using ROV/camera surveys of known statistical validity. The technology for such surveys is capable of giving reliable results (Pacunski et al. in press, Gunderson et al. 2008; Fisheries 33:172-179), and has already been employed successfully by WDFW (Palsson et al. 2008). Historic abundances could be estimated indirectly using maps of rocky habitat in the appropriate depth range, and inferring the densities (number per square meter) that once occurred there. These inferences could be made using some combination of known current densities in unfished areas and anecdotal information provided by fishermen who were active in the 1950's.

Reviewer 4

I have gone over the draft ESA status review of Puget Sound Rockfishes.
I was impressed with the thought that has gone into the process leading to the document and I think I would have come to the same conclusions.
Really, given the shortage of data on some of these species, this was the best work that one could have expected.

Appendix F – Response to reviews

Responses to peer reviews

Comment	Response
External Reviewer 1	
Pg. 52 and beyond: should "absolute numbers" be "absolute abundance"?	We altered the text to read absolute abundance
How can one say anything about extinction risk without having an idea of abundance (even with wide CI's)?	Quantitative criteria for listing species do not always rely on "risk of hitting 0 numbers". Another criteria used is the rate of decline and an observed or projected severe decline. Examples include the IUCN Red-List risk criteria and the AFSC listing criteria. Severe declines, 80-99%, are considered reason for high conservation concern – if such declines persist extinction is certain. If no accurate numbers are available, one must rely on the information available, such as how often the species is observed, to form an expert opinion concerning whether the species is sparse, rare, or extremely rare.
Pg. 56: It sounds like "expert opinion" was used to rank demographic risk. This could be strengthened if bit more attention is given to the methods in ranking criteria. Social scientists worry about things like uncertainty in how various metrics are ranked. This is not my field but I have been scrutinized in similar endeavors. Ben Halpern and Carrie Kappell have some published approaches, and I am sure a look at the social science literature could strengthen this approach.	We used standard, established and peer-reviewed methods employed by NMFS for other status reviews including salmonids, Pacific hake, walleye Pollock, Pacific cod, Pacific Herring, black abalone, and copper, quillback and brown rockfishes.
I am skeptical about applying a trend analysis to "total rockfish" (i.e., applying analysis to combined species). Given the	This, and a similar comment by external review #2 are a misinterpretation of the approach we used. Importantly, we did not

paucity of data, I can see why this approach was employed, but it seems like it could be misleading if the relative trends are different among species (which seems likely).	make the assumption that the total trend is an estimate of the trend is the rarer species. The logic is as follows: N_rare(t) = (N_rare(t) / N_total(t)) * N_total(t) If N_rare(t)/N_total(t) is constant, then the trend in N_total = the trend in N_rare. If N_rare(t)/N_total(t) has been going down, then the rare spp is going down faster than the total. If N_rare(t)/N_total(t) has been going up, then the rare spp is not going down as fast as the total. The analysis consisted of 1) Evaluating the trend in the total rockfish abundance using all available data and multiple ways of looking at the data. 2) Evaluating the evidence that the prevalence of each rare species has been going up relative to the total (has N_rare/N_total been increasing). We have edited the text in several places to clarify this.
Fig. 33 legend indicates that different colors were used to show which data were treated as separate pops, but I am only seeing one color. The legend also is a little unclear (some typos), so it's hard to figure out what the figs represent. I think the figure is showing trends for combined data, which has merit because it confirms that these populations seem to be declining. But I am still hesitant about performing any kind of extinction risk analysis for combined species data for the reason mentioned above.	We updated the figure and text. The new legend reads as follows: Figure 33. The different colors in the bottom panel show which data were treated as separate (but not independent) population processes. Specifically, data from the trawl survey may be sampled from a different segment of the total rockfish assemblage (age or size) than the recreational data. Thus the trawl data are treated as an independent trajectory but with the same long-term growth rate. Each process has the same long-term population growth rate (a parameter) because over the

	long term one segment of a population cannot have a different trend than another segment of the population. But over the short-term, different population segments can certainly have different trajectories. Modeling the trawl data as its own process also allows that this segment of the population could have different process variance than the segment of the population sampled by recreational gear.
External reviewer 2	
I paid special attention to the evaluation of extinction risk in my review. There are two potential difficulties with this analysis. First is that abundance data for the individual species are not sufficient for independent analysis, particularly for species that historically made up a small proportion of the overall rockfish fishery and for species that have had catch restrictions imposed and thus have no recent fishery-dependent data. The remedy for this difficulty proposed by the BRT is to analyze the long-term trend in overall rockfish abundance, recognizing that this trend is driven primarily by more abundant species but could serve as an estimate of the trend in the rarer petitioned species. Clearly this is quite a striking assumption, and any number of factors could produce violations of the assumption. . 	As described above for reviewer #1—this is a misinterpretation of the methods. In addition to clarifying the methods, we added the following the text: Results Looking at all the data available to us, we found no evidence that the petitioned species make up an increasing fraction of the total rockfish pool over time. Instead, for some species, we found evidence that the species are a smaller percentage of the "total rockfish" over time. Thus we found no evidence to suggest that the petitioned species have increased or been stable while the total rockfish abundance has declined. Instead the evidence points to some species declining faster than the total abundance has. Discussion Because time series data on the petitioned species were not available, we could not do a direct quantitative analysis on these species. Instead we were forced to use data on total rockfish trends and trends in the species composition of the total rockfish assemblage. We recognize that the trend in total rocksfish does not equal the trend in the petitioned species. However, this does not mean we had no information on trends

	in the petitioned species. Total rockfish abundance has declined and some of the petitioned species (list which ones) have become a smaller proportion of the total rockfish assemblage. This allows us to use the trends in total rockfish as an upper bound on the trends for the petitioned species. This was the approach taken in the analysis.
This approach should produce a conservative estimate of the trend in the petitioned species (i.e., the actual trend is probably more negative than that identified here). Changes in gear and switches in the targeted species should tend to prolong high catch levels in a multispecies time series, so an observed decline in overall catch probably reflects steeper declines in the actual abundance of individual fishes.	This is a good point that we added to the text.
Furthermore, in the petitioned species for which data were available, the proportion of the overall catch appears to have declined in recent years. Provided that this does not reflect a change in the species-specific targeting of the recreational fisheries (which seems unlikely), this suggests that the multispecies data would again be an optimistic estimate of the rate of decline. One caveat should be that the overall rockfish trend should not necessarily be representative of species that are completely closed to fishing, such as canary rockfish since 2002. The BRT notes that there was not a trend for declining canary rockfish abundance since 2002 in the REEF surveys; has there been any evidence for a temporal increase in those surveys	Several good points here which we incorporated in to the trend discussion
The second difficulty in the extincton risk	Given our data constraints, our risk metric

analysis is the presence of multiple data sources that should have different error structures and different relationships to the overall mean abundance. The state-space model approach taken by the BRT is an excellent solution to the problem, and they appear to have chosen appropriate specifications for the model. The model fits to the data appear quite good (although considering the number of potential parameters in the model, a good fit is to be expected). My largest concern with this modeling approach is that it assumes a temporally stationary population growth rate (parameter a). This presumes that the overall rockfish "population" is either declining to extinction (a < 0) or rising to a non-zero steady-state equilibrium (a > 0). The model is therefore constrained to indentify an overall negative or positive trend in the data, and cannot represent behavior such as a decline to a minimum followed by subsequent increase.	was a measure of the long-term trends. Our parameter "a" is effectively the average population growth rate over the entire data set. The analysis does allow that there have been periods of positive population growth but we use for forecasting and risk assessment the long-term average population growth. Although current rates of decline may be less (or more) than the long-term average, a negative long-term population growth means the current population is well below the levels seen 30-50 years ago.
This assumption *[from the above comment]* is probably reasonable for the current state of the Puget sound rockfish community. However, if some sort of intervention were to change the demographics in the study region, such as a large scale change in fishing regulations or environmental mitigation in response to ESA classification, this same analysis would be inappropriate for the analysis of future datasets.	This is a very hard question to address with noisy data, and we agree with the basic premise of the question. Importantly, however, the forecasting used for petition analyses asks "if trends over the last 30-years continue,…", and as this reviewer notes one goal of ESA protection would be to ensure that past trends do not continue.
Rather than identifying a deterministic change in recent abundances, this type of model would fit positive observation and process	Agreed

error terms to those recent years. To deal with that situation, future analyses would require a temporally variable parameter a, or a three-parameter model that includes an intrinsic growth parameter and an exploitation parameter. I add this comment not necessarily as a criticism of the current approach (after all, there is pretty convincing evidence for a long-term decline in Fig. 36-37, even without the model fits), but rather as a clarification that should be made for future investigators who may desire to revisit this analysis in the future.	
p. 16: "myctophids" is misspelled	corrected
p. 27, second paragraph: probably better to say that eelgrass and seaweed habitat support most "species" in the sound, rather than most "populations."	done
p. 38: This is a very nice explanation of the benefits of mitochondrial DNA evidence. I recommend clarifying at some point in this discussion that microsatellites are nuclear markers.	done
p. 41: With respect to the comparison with other rockfish species in Puget sound, this seems like a very reasonable approach for making inferences regarding the genetic structure of the petitioned species. I would add that given the apparent level of development and swimming abilities of rockfish larvae and post-larvae, it is probably reasonable to err on the side of shorter rather than longer larval dispersal distances.	Agreed
It is not clear what should be made of the comparison with the non-rockfish species in the sound. Are there reasons, based on	We added text to clarify this

spawning behavior, spawning season, or larval life history, that the petitioned rockfish species should be more or less like any of the non-rockfish species mentioned here?	
p. 45: "were" not "where"	fixed
p. 69: Summary of methods: rephrase as "...using a maximum likelihood approach to fit..."	done
p. 70: Ward et al. 2008 and Shumway & Stoffer 2006 are missing from the references.	fixed
It is reasonable to point out the importance of ontogenetic changes in fecundity, but there is no reason that those variations cannot be included in a calculation of LEP, and LEP can then be used in the standard way (comparison to a biological reference point or to the slope of the stock-recruit curve). It is correct that variation in the relationship between biomass and fecundity would invalidate the use of SSB. On the same point, is the issue of the seasonal timing of reproduction relevant to Puget sound populations? I am only familiar with that type of argument being used in open coast populations for which the timing of spawning relative to upwelling transitions, etc. is very important.	We agree with the statement that calculation of LEP can be adjusted to account for ontogenetic changes in fecundity, and this is mentioned in the text. The purpose of pointing out the importance of maternal effects here is to state that rockfish populations seems particularly sensitive to such age class truncation. We are therefore concerned not only with population decline, but also the modification of population composition
p. 76. On the topic of LEP, I was somewhat surprised that extinction risk analysis did not incorporate some of the traditional approaches to evaluating population	The reviewer is correct that the necessary data were not available to the BRT.

persistence, such as a comparison of LEP to the compensation ratio. Since the relative value of those two measures is a fundamental measure of the long-term persistence of a population, that comparison would be very useful. Are estimates of these values not available for the petitioned species?	
Reviewer 3	
I found this to be a comprehensive review of the status of the rockfish stocks in question. This was a difficult undertaking, since the review team had very little data to work with. However, the lack of genetic studies pertinent to the species of concern led the team to generalize from what is known about other rockfish, notably the more well-studied copper, quillback and brown rockfish. All three of these species are known to mate and spawn in Puget Sound, and to be highly restricted in their migrations as adults. Recent genetic analyses suggest that many of their larvae may drift no more than a few tens of kilometers after parturition. This is probably not the case for bocaccio or canary rockfish, and a different model is more appropriate.	We agree that copper, quillback and brown may have shorter larval lives than bocaccio and canary. However, all species are subject to hydrological restrictions that certainly have a large effect on dispersal distance.
Bocaccio and canary rockfish probably have a life history that is more similar to species like yellowtail or splitnose rockfish. Both have large populations in offshore waters, and Puget Sound is likely a sink for larvae that drift in. While splitnose larvae are commonly found in drifting kelp mats, adults are rare throughout most of Puget Sound proper. Most yellowtail rockfish in Puget Sound are immature, migrating to the ocean once they reach sexual maturity (see Barker and	The basic premise of this comment seems to be that Puget Sound represents a sink population. That is, there is no spawning of bocaccio or canary in the Sound. This is certainly a valid hypothesis and did gain a minority of support in the BRT's DPS deliberations. However, the reviewer offers no direct evidence of this hypothesis for bocaccio and canary rockfish. In the absence of direct evidence of population structure, the BRT reasoned that

Mathews 1983, Gunderson and Vetter 2006). I am not aware of any reported mating or spawning of splitnose or yellowtail rockfish in either Puget Sound proper or the San Juan Islands. Without further genetic study, and documentation of both mating and spawning in Puget Sound, there is no reason to suppose that the bocaccio and canary rockfish there constitute a population segment that is distinct from the ocean population. Populations within Puget Sound are more likely to be sink populations that exist only through sporadic recruitment of larvae from ocean sources.	if Puget Sound was a sink population that is seeded with larvae from coastal populations then the age structure of Puget Sound and coastal populations should be similar. As the BRT report notes, this does not appear to be the case. In revision, the BRT expanded and clarified this analysis. In particular, the analyses shows that the existence of strong year classes in Puget Sound that were absent along the coast, as well as strong coastal year classes that are missing from Puget Sound.
Reviewer 4	
I have gone over the draft ESA status review of Puget Sound Rockfishes. I was impressed with the thought that has gone into the process leading to the document and I think I would have come to the same conclusions. Really, given the shortage of data on some of these species, this was the best work that one could have expected.	No response needed