

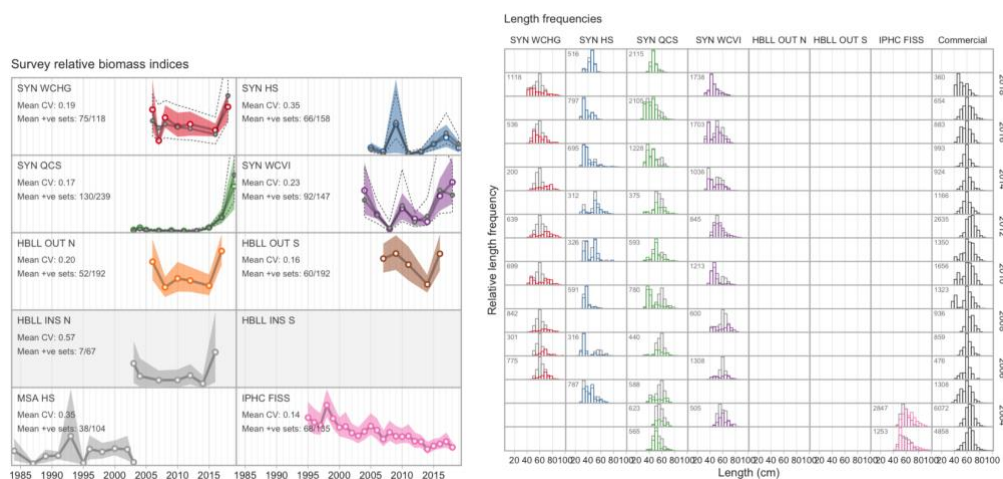
Internal review of the Science Response “Evaluating the robustness of candidate management procedures in the BC Sablefish (*Anoplopoma fimbria*) fishery for 2019–2020.”

The described updates to the operating model and proposed management procedures and conclusions of the document seem reasonable given the available information.

We have no reason to doubt that the changes to the conditioning of the operating models were important for improving fit to the data; however, the effects of many of these updates are hard to judge given the available documentation. For example, how does the new trawl selectivity function (Gamma shaped) compare to the previous selectivity function? How much of an impact did this have on the operating model conditioning? How much of an impact did the new assumption about tighter observation errors on the trawl at-sea release have on the operating model? What do the inferred trawl age-composition data look like for selection of years and what do the length-composition data look like that go into that? What do the raw aging error plots look like (Reader 1 vs. Reader 2)? This might be more than typically goes into a Science Response, but there are quite a few changes made to the operating model here and we imagine some documentation bridging the operating model changes will be helpful for future analyses.

Given the importance of the very high 2015 estimated year class throughout this document, we would like to see some diagnostic plots to help understand what led the model to estimate this. The Response notes high trawl at-sea release observations. To what extent is this evident in age composition data?

As an aside, there may be some qualitative confirmation in the synoptic trawl survey data, where the surveys are largely catching juvenile sablefish. All four surveys show some level of increase in biomass density after 2015 (although maybe not Hecate Strait in 2019) and the length distributions are available. Queen Charlotte Sound and West Coast Vancouver Island are the clearest.



As noted in the results, one important reason that the retention policies examined in the document are able to meet conservation objectives while sustaining higher catches and higher fishing mortalities is that they reduce growth overfishing contingent on fishing stopping once the TAC is met. The end of the methods notes "The most critical assumption, and even unrealistic in some cases, in the above is that fleets stop fishing when their fleet-specific TACs are fully landed." Is there currently no implementation error? If so, this seems like an important dimension to explore in a future update.

The section in the results on cross testing that is yet to be written seems particularly important. The summary in the conclusions and the tables themselves make it clear that there is a great asymmetry in risk here. The consequences of overestimating a large 2015 year class has real conservation risks.

We assume there is a historical reason for dividing the steepness-SSB joint posterior into 5 regions and then recombining them afterwards. In this current document, the 5 operating models are never considered independently and always as a weighted average. Separating them and then combining them weighted by their marginal density would seem to only add complexity, add some error from the arbitrariness to the divisions, and—perhaps most importantly—not sample from the outer tails of the posterior. The only advantage we can think of is that it could make communication easier by being able to consider the components individually. Presumably other longer documents in the future will take advantage of the separation. Otherwise, it would make sense to me to just sample from the full joint posterior.

A main conclusion seems to be that the current management procedure with no cap on at-sea releases is able to meet conservation objectives but that management procedures that involve some retention policy could also meet those conservation objectives while increasing catch and fishery revenue (currently written as income). The economic conclusions are contingent on the price structure for sablefish size and also on constant costs to the fishers (if income). Is there reason to think that a retention policy would affect average trip costs? Presumably there is considerable uncertainty about future price structure for sablefish size. This may be worth a brief discussion.

We've included a number of more specific minor comments as track changes and comments in the .docx and PDF files.