Endangered Species Act Status Review Report: Shortfin Mako Shark (*Isurus oxyrinchus*): ID430

Peer Review Comments

We solicited review of the draft Endangered Species Act Status Review Report for Shortfin Mako Shark (*Isurus oxyrinchus*). Three people agreed to serve as peer reviewers. Reviewer comments are compiled below and are not associated with the order of the reviewers as listed. Editorial suggestions to the text are incorporated into the final document as appropriate.

Reviewers (listed alphabetically):

Dr. Elizabeth A. Babcock
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Specific Responses to Charge Statement Questions (not associated with order of names as they appear above)**:**

Reviewer 1:

1. In general, does the Status Review Report include and cite the best scientific and commercial information available on the species and its biology, habitat and distribution, population structure, abundance trends, threats, and risk of extinction?

The information used in the assessment is the best available. The scientific literature and fisheries data are the most recent that would have been available to the assessment team.

2. Where available and relevant, are opposing scientific studies or theories acknowledged and discussed?

Yes, there was some consideration of alternative interpretations of the information available to the team. There is one that I would like to highlight that I think needs to considered further. In particular, there is some consideration of whether the approach used in the IUCN Red List assessment of the shortfin mako was appropriate as per the criticisms in in Kai (2021a. Are the current IUCN category and CITES listing appropriate for the conservation and management of shortfin mako, *Isurus oxyrinchus*, in the North Pacific Ocean? Marine Policy 134:104790) [page 20]. The discussion around this point however seems to be one sided, with the review appearing to dismiss the Red List assessment on the basis of this work. However, no consideration of whether the issues raised by Kai (2021a) were themselves valid are provided. I would recommend that some of this consideration be added to the assessment to provide better balance. The Red List assessment reaches a conclusion of Endangered based on extinction risk

criteria that have been widely used, including by many national governments when considering species for threatened species listing.

3. Are uncertainties assessed and clearly stated?

Uncertainty is clearly considered in the assessment. There is a large amount of uncertainty in the data and information used and this is a significant part of what the assessment deals with. While the uncertainty is clearly acknowledged, I am concerned that how uncertainty is dealt with in reaching a conclusion on the status of shortfin mako is not fully considered. It is normal practice in this type of assessment to increase precaution when uncertainty is high. This approach means that conclusion that action is not necessary, when in fact it is, are more likely avoided. It is my impression from reading the assessment that the uncertainty in the data, especially in relation to metrics such as CPUE trends and catch levels through time, have not resulted in a more precautionary conclusion. The uncertainty in these parameters means that while there are stock assessments available for some areas, and CPUE trends in others, that there is significant uncertainty about what the status of the species is. Despite this, the lack of certainty is almost used to dismiss the need for reaching a more precautionary conclusion.

As an example of this uncertainty, there is clearly a large amount of uncertainty about the level of catch in almost all regions, especially as you go further back in time. Nowhere in the assessment was there a consideration of what this uncertainty in catches would mean for the extinction risk assessment. In cases where these catch series were used in stock assessments I am sure that assumptions were made about historic catch levels. However, these assumptions are not laid out in the assessment, nor are the consequences for alternative assumptions about historic catches.

4. Are the methods used for the Extinction Risk Analysis valid and appropriate?

The methods used in the extinction risk analysis are similar to those used in some other ESA assessments for marine species. However, they lack any clear quantitative reference points. As such the conclusions reached are purely subjective and based on the assessments team's interpretation of extinction risk faced by the species. This interpretation is informed by a large amount of information, but the lack of measurable reference points means that a different group of people could reach a different conclusion based on the available information. This subjective approach is reflected in the use of qualitative approaches to reaching conclusions. This type of approach is valid, but means that there is scope to criticise the conclusions reached because they are largely opinion-based.

5. Is the length of the foreseeable future (described in section 5.2) appropriate for the species based on the best available information, and is adequate justification provided?

The foreseeable future used is 25 years, equivalent to one generation. This is a relatively short compared to many other extinction risk estimation methods. Red List assessments consider 3 generation lengths by comparison. In some ways the 25 year period used is possibly too short. For example, the assessment reports on the results of the most recent ICCAT assessment that found that the North Atlantic population would continue to decline until at least 2035 even if there was zero catch. This extinction risk assessment only considers a period 10 years beyond this, and this seems too short to consider the consequences for the population.

6. Are the results and conclusions of the Extinction Risk Analysis supported by the information presented?

It is my conclusion that the results of the extinction risk analysis have a number of shortcomings that may compromise the results. Some of these are laid out above. In particular, the issue related to uncertainty vs precaution is of concern. I also lay another concern below that go beyond the topics in the points above. This does not necessarily mean that the wrong conclusion was reached (given that the approach was somewhat subjective this will be a matter of opinion), but that the conclusion reached is less defensible that it could be.

Reviewer 2**:**

1. *In general, does the Status Review Report include and cite the best scientific and commercial information available on the species and its biology, habitat and distribution, population structure, abundance trends, threats, and risk of extinction?*

   The Report includes the best scientific information available, including an exhaustive literature review and data from all the relevant fisheries management organizations. As far as I can tell, no major sources of information are missing. The report is a thorough and well-written evaluation of the information available on shortfin mako shark biology, population dynamics, and threats.

2. *Where available and relevant, are opposing scientific studies or theories acknowledged and discussed?*

   There are several opposing threads in mako shark science. These were acknowledged and discussed.

   One is whether mako sharks lay down one vertebral band per year, or two, or whether the number of bands varies with age. Since correct ageing is necessary to determine the age at maturity and the natural mortality rate (both relevant to the species' low productivity) this is a key uncertainty. Although the various analyses were discussed, it would be useful to explain to the reader which hypotheses are best supported by the data and what that implies. For example, are the values of longevity and natural mortality used in the ESA analyses and assessments valid or are they based on interpretations of growth that have since been superseded?

   Another issue is how to interpret abundance indices depending on the size selectivity of the fishery. The majority of shortfin mako sharks caught in longline fisheries are immature, while criteria for extinction risk estimation, such as the IUCN criteria, are often based on the mature population. The stock assessments discussed in the report include production models (e.g. JABBA, BSP-JAGS) and raw CPUE analyses (e.g. , the JARA analysis for the IUCN review) based on CPUE data that apply to the part of the population vulnerable to the fishery (i.e. mainly immature animals), as well as statistical catch at age models which report trends in spawning stock (SS3 in the north Atlantic and north Pacific). For a species that does not mature until around age 20 (for females), a juvenile abundance index and spawning stock abundance index can be out of phase by decades. This is why the projections in the North Atlantic are so much more pessimistic from the age structured models; the age structured projections were for the spawning stock, which will continue decreasing for years due to past fishing mortality on juveniles. The potential for a long time-lag between juvenile and mature abundance is an important point to consider when looking at general CPUE trends in the regions without stock

assessments. There is really very little information about the trends in the mature stocks, and recent increases in (presumably mainly immature) CPUE in some regions may not be visible in the spawning populations for decades. Throughout, the text should be clear about whether trends refer to total abundance or mature abundance. Also, it is important to emphasize that either age-structured stock assessments or at least abundance trend analyses based on data for mature animals are needed for the regions that do not yet have such assessments (everywhere except the north Atlantic and north Pacific).

3. *Are uncertainties assessed and clearly stated?*

   The report is quite thorough and clear about the relevant uncertainties, including the historical under-reporting of catch data, lack of species-specific identification in the catches, etc. The report also mentioned that there have been many recent changes in fishery management, such as the north Atlantic ICCAT non-retention policy, various anti-fishing regulations, and the CITES listing, the effects of which are not yet known.

4. Are the methods used for the Extinction Risk Analysis valid and appropriate?

   The ERA method is valid and appropriate. The evaluation was based on an iterative process in which each expert scored the risk and threat factors, and then rescored them based on discussion, and the final probability of extinction was generated by allocating likelihood points to the risk categories. These are standard methods for expert judgment elicitation, and the conclusions are consistent with the data.

   The determination that there are no DPSs needs a better justification. The Report concludes that there are no DPSs primarily on the basis of nuclear DNA studies that failed to find much structure. However, there is structure in mitochondrial DNA which is attributed to female spawning site fidelity while males move sufficiently to maintain a panmictic population. However, genetic structure is not the only possible basis for a DPS finding. The Report states that one criteria for a DPS might be "Evidence that loss of the population segment would result in a significant gap in the range of the taxon." Given female site fidelity, a very low rate of migration between ocean basins and across the equator, and the slow life history of this species, isn't it possible that extirpation of the north Atlantic population (for example) could lead to a significant gap in the range of the taxon for many decades? The conclusion that there are no DPSs may be correct, but it needs a better justification.

5. *Is the length of the foreseeable future (described in section 5.2) appropriate for the species based on the best available information, and is adequate justification provided?*

   The chose time horizon needs a better justification. On the one hand, it is impossible to project population dynamics far into the future with any certainty. On the other hand, due to its slow life history, changes in threats and conservation measures for shortfin mako sharks might take decades to work through the age classes and become visible in the mature population. In the interest of being precautionary, I think it would be worth thinking about a longer time horizon than 25 years. Although a 3 generation time horizon (for example) can't be forecast with accuracy, is useful to know how long it would take for threats or conservation actions to influence the spawning stock biomass.

6. *Are the results and conclusions of the Extinction Risk Analysis supported by the information presented?*

The results and conclusions are supported by the information presented. The assessment is well done, and the results follow from the data presented. It seems correct to conclude that the global shortfin mako is at low to perhaps moderate risk of extinction in the near term. This conclusion is consistent with the facts that (1) the available trend data show moderate declines in some regions and stable or increasing trends in others, and (2) the species has very low productivity making it both vulnerable to overexploitation and difficult to assess.

Reviewer 3:

1. *In general, does the Status Review Report include and cite the best scientific and commercial information available on the species and its biology, habitat and distribution, population structure, abundance trends, threats, and risk of extinction?*

   The status report is very comprehensive and the authors have done an excellent job identifying and discussing the current state of knowledge on shortfin mako. Where relevant, the authors have used the substantial work completed by various working groups in RFMOs to complement and/or augment information in the peer reviewed literature. It is not possible to discuss all details, and some of my comments reflect that I emphasize different components of an analysis relative to the authors. I only have two substantive comments on the biological and trend information incorporated and its organization. My comments relative to the evaluation of extinction risk are covered under question 6 below.

   Section 3.2 on regional abundance estimates and population trends details results from stock assessments in the North and South Pacific Ocean while the analysis of overutilization in section 4.2B provides information for Western/Central and the Eastern Pacific. Although I recognize that the available information is generally organized this way, it hindered evaluation of overutilization in light of stock status. It became difficult to interpret fleet behaviour, catch totals and trends in catches relative to assessment results. If at all possible, I would recommend standardizing the regions among all sections of the report and adding a map to show all of the regional boundaries mentioned in the document.

   Section 2.5 Re: Population Structure. It is generally accepted in ecology that variation in natural selection among heterogenous environments produces among-population differences in life history characteristics. These differences can reflect higher fitness (and are thus adaptive) or can arise from fitness-neutral factors such as genetic drift or rapid environmental change (Kawecki & Ebert 2004). Considerable study on diadromous fishes indicate that low levels of connectivity among populations does not eliminate life history variability or lead to among-population homogenization in adaptive traits (e.g. Moore et al. 2010; Peterson et al. 2014). An omission from Section 2.5 is any discussion of life history variation among regions and what these may indicate in terms of population structure for shortfin mako. The life history information given in Section 2.4 (Table 1) shows considerable variability in key vital rates such as growth, length and age at maturity, gestation period, and parturition timing among the oceans considered in this report.

   Kawecki, T., & Ebert, D. (2004). Conceptual issues in local adaptation. Ecology Letters, 7(12), 1225–1241

   Moore, J. W., M. McClure, L. A. Rogers, and D. E. Schindler. 2010. Synchronization and portfolio performance of threatened salmon. Conservation Letters 3:340–348.

Peterson, D. A., R. Hilborn, and L. Hauser. 2014. Local adaptation limits lifetime reproductive success of dispersers in a wild salmon metapopulation. Nature Communications 5:3696.

2. *Where available and relevant, are opposing scientific studies or theories acknowledged and discussed?*

The authors have done a good job identifying and describing opposing results in multiple sections of the report. One useful addition would be to indicate the sample size that supports specific statements, particularly when conclusions differ. This would help the reader evaluate the weight of evidence for the opposing hypotheses. For example, Heist et al. 1996 was based on relatively few samples in comparison to Schrey & Heist 2003 when evaluating genetic diversity; similarly Hutchinson et al. 2021 was based on very few samples relative to Bowlby et al. 2021 when evaluating post-release mortality. There are also a few instances in the document were specific analyses were updated, yet are presented as if the results should be given equivalent weight to the original (older) evaluation. E.g. Murua et al. 2018 vs. Murua et al. 2012 when describing productivity, or Pratt & Casey 1983 vs. Natanson et al. 2006 when estimating growth rate.

3. *Are uncertainties assessed and clearly stated?*

The authors have identified several sources of uncertainty that influence our ability to assess extinction risk, focusing on instances where there is apparent conflict among different datasets (e.g. divergent CPUE trends), results that cause divergence in our understanding of various ecological patterns and processes (e.g. distribution patterns, population vital rates) or in data reporting and the resulting ability to quantify fisheries removals for rigorous stock assessment. I have focused specifically on the four demographic risk criteria used in the extinction risk analyses: abundance, growth rate/productivity, spatial structure/connectivity and diversity.

In terms of abundance, the predicted timeseries from age-structured assessments from ICCAT(2017) and ISC (2018) were used to represent global abundance. The authors did not explicitly detail why or how assumptions made during these stock assessments would influence absolute abundance predictions (as opposed to relative abundance such as B/Bmsy) and could introduce substantial uncertainty. A good general reference that details sources of uncertainty in stock assessments and why there is variability among different assessments in predicted stock size is Ralston et al. (2011). Ralston et al. quantified estimation uncertainty both within and among stock assessments by approximating the CV for the terminal year biomass, finding an among-assessment CV of ~36% and a within-assessment CV of ~18% in a meta-analysis. Such uncertainty in estimation (reflected by the within-assessment CV) and model specification (reflected by the among-assessment CV) suggests that abundances of 1 million or 8 million animals could be relatively uncertain values. It would be helpful to identify the main factors contributing to uncertainty in these abundance estimates, and comment how large the uncertainty may be. For example, a CV of 36% on a mean estimate of 1 million animals implies that the standard deviation is 360000. A normal approximation of a 95% confidence interval is 1.96*SD or $\pm$ 705600 animals.

Specifically for shortfin mako in the North Atlantic, Courtney et al. (2017) details the SS modeling used in the 2017 ICCAT assessment. In Table 11, there is a predicted timeseries of biomass (to 2015) as well as spawning stock fecundity (SSF), where the latter was used in the status evaluation. The FAO report (FAO 2019) states that the expert panel extracted current values from the full computer outputs available for the age-structured assessments. This does

not identify or explain how biomass predictions from Courtney et al. (2017) were transformed into numbers (e.g. did the method used to partition among ages propagate uncertainty in the age-length relationship?), nor why only one of the available assessment approaches was used to extract values, rather than all of the modeling approaches from the assessment meeting which were combined as the basis for advice. Given that the estimates of absolute abundance are a key piece of information used to determine extinction risk, there needs to be explicit consideration of the sources of uncertainty affecting these predicted values from the North Atlantic and North Pacific.

In terms of growth rate and productivity, the issues associated with age determination (which influences maturity and longevity estimates; e.g. Harry 2018) are identified, but are not evaluated/discussed in terms of how this uncertainty would affect extinction risk through our understanding of productivity. Estimates of population growth parameters such as *r* (intrinsic rate of population growth; e.g. Cortés 2016) depend on our understanding of age, given that natural mortality is typically approximated from longevity (e.g. Kenchington et al. 2014), and age at maturity (or a maturity ogive) need to be specified in the calculation. The possibility that age is underestimated (*c.f.* longevity of 29 vs. 56) would have very serious implications, in terms of limiting productivity and reducing the level of removals that the population would be expected to sustain. Bowlby and Gibson (2020) give a specific example of this issue relative to white shark. The fact that lambda estimates are already very close to 1 is particularly concerning for shortfin mako, in that these already suggest extremely limited ability to increase in abundance.

In terms of spatial structure/connectivity and diversity, the life history variability relative to the apparent genetic homogeneity represents a key source of uncertainty when delineating stock structure. Haplotype and allelic diversity in microsatellites and/or mitochondrial DNA reflect selection processes occurring over evolutionary timescales on neutral genetic markers (i.e. genes that are not linked to fitness and thus are not under adaptive selection; Frankham 2005). Evolutionary processes take place over much longer timescales than the types of ecological processes that influence population dynamics, where ecological processes are the foundation of fisheries assessment and status evaluation. Sample homogeneity at neutral markers does not necessarily equate to population homogeneity, particularly when genetic differentiation among marine populations is both expected and observed to be low, making it particularly difficult to discern a small signal (Ward 2000). Furthermore, the majority of analyses finding strong genetic evidence for population structure take samples when individuals are within distinct breeding habitats, noting that overlap of stocks would be expected on feeding grounds (e.g. Shamblin et al. 2012 for green sea turtle). Available samples from shortfin mako were taken in association with fisheries (i.e. when feeding), with no expectation that they were collected specifically from breeding aggregations or during breeding behaviour. Thus sampling would reduce our ability to describe population structure for shortfin mako *a priori*, making it more difficult to reject the null hypothesis of panmixia (random mating), particularly when some samples come from equatorial regions where overlap in distribution but not necessarily in spawning behavior would be expected to occur.

It is very difficult to distinguish among rates of connectivity using genetic data (Waples 1998), leading to scenarios where extremely low rates (i.e. those insufficient to replenish localized depletion from overfishing) would be indistinguishable from extremely high rates. The alternate interpretation of spatial structure (i.e. largely distinct populations) could lead to very different conclusions on extinction risk (further discussed under question 6 of the charge statement) and should be explicitly identified as a key source of uncertainty. It would also be helpful to

specifically discuss the reasoning by assessment teams when partitioning regions for stock assessment. It is noteworthy to me that assessment teams (e.g. ICCAT) did not consider shortfin mako to be a single panmictic population and felt it more appropriate to delineate multiple stocks.

As a final general comment, I feel it would be helpful to differentiate between values that are uncertain (where the true value can be smaller or larger) vs. values that are uncertain but known to be systematically under/over-estimated. For example, systematic underestimation of catches (owing to reporting issues: e.g. species identification, non-reporting or incomplete reporting of catches by several key nations, missing historical catches, lack of post-release mortality estimates) give inappropriately low estimates of fishing mortality and inappropriately positive status predictions in stock assessment. A comparison of the 2012 and 2017 stock assessments in the North Atlantic can give some indication of the relative severity of underestimated catch on our understanding of status. In this instance, it was largely revisions to input data (resulting in a more complete understanding of catches) that were implicated in the status reversal: not overfished and no overfishing in 2012 vs. overfished and overfishing in 2017. While it is only possible to provide conclusions relative to the data that exists, much stronger statements could be made on the relative severity of underestimated catch and the uncertainty it introduces on our perception of status, in addition to any influence on determinations relative to overutilization as well as the adequacy/inadequacy of regulatory measures.

Bowlby, H.D. and Gibson, A.J.F., 2020. Implications of life history uncertainty when evaluating status in the Northwest Atlantic population of white shark (Carcharodon carcharias). *Ecology and Evolution*, *10*(11), pp.4990-5000.

Courtney et al. 2017. Stock synthesis (SS3) model runs conducted for North Atlantic shortfin mako shark. SCRS/2017/125. Collect. Vol. Sci. Pap. ICCAT, 74(4): 1759-1821.

Frankham, R., 2005. Genetics and extinction. *Biological conservation*, *126*(2), pp.131-140.

Harry, A.V., 2018. Evidence for systemic age underestimation in shark and ray ageing studies. Fish and Fisheries, 19(2), pp.185-200.

Kenchington, T.J., 2014. Natural mortality estimators for information-limited fisheries. *Fish and Fisheries*, *15*(4), pp.533-562.

Ralston, S., Punt, A.E., Hamel, O.S., DeVore, J.D. and Conser, R.J., 2011. A meta-analytic approach to quantifying scientific uncertainty in stock assessments. *Fishery Bulletin*, *109*(2).

Shamblin, B.M., Bjorndal, K.A., Bolten, A.B., HILLIS-STARR, Z.M., Lundgren, I.A.N., Naro-Maciel, E. and Nairn, C.J., 2012. Mitogenomic sequences better resolve stock structure of southern Greater Caribbean green turtle rookeries. Molecular Ecology, 21(10), pp.2330-2340.

Waples, R.S. 1998. Separating the wheat from the chaff — patterns of genetic differentiation in high gene flow species. J. Hered. 89: 438–450.

Ward, R.D., 2000. Genetics in fisheries management. *Hydrobiologia*, *420*(1), pp.191-201.

4. *Are the methods used for the Extinction Risk Analysis valid and appropriate?*

There are numerous methods that can be used to assess extinction risk, ranging from qualitative or semi-qualitative Ecological Risk Assessment (e.g. Cortés et al. 2010) to quantitative Population Viability Analyses (PVA; Morris and Doak 2002). By necessity, qualitative methods

inherently rely on expert opinion relative to available numerical data, yet can be better at integrating among different types of information. PVA predicts abundance trajectories relative to various types or magnitudes of threats to provide numerical estimates of risk, but is less able to account for qualitative information. While there would be benefits to combining both approaches for the comparatively data-rich scenario of shortfin mako, possibly using a model similar to the one developed for basking shark (Campana et al. 2008), qualitative assessment of risk on the basis of expert opinion appears to be established practice within the status review process for the ESA and is commonly used to provide science advice for elasmobranchs (e.g. Gallagher et al. 2012, Cortes et al. 2010).

Re: the risk of extinction. Please also see comments below re: global extinction risk and the declining population paradigm. For elasmobranch species in particular, the declining population paradigm is widely understood to be a more appropriate theoretical foundation for assessments of extinction risk (Punt 2000, Dulvy et al. 2004, Field et al. 2009, VanderWright et al. 2021). However, similar to small populations, declining populations may have characteristics associated with high extinction risk yet ultimately recover as conditions change (some examples are provided in Dulvy et al. 2004). From my perspective, greater weight should be given to population trends as opposed to population size when evaluating extinction risk relative to abundance.

Campana S.E., Gibson J., Brazner J., Marks L., and Joyce W. 2008. Status of basking sharks in Atlantic Canada. CSAS Research Document 2008/004. https://www.dfo-mpo.gc.ca/csas-sccs/publications/resdocs-docrech/2008/2008_004-eng.htm

Dulvy, N.K., Ellis, J.R., Goodwin, N.B., Grant, A., Reynolds, J.D. and Jennings, S., 2004. Methods of assessing extinction risk in marine fishes. Fish and Fisheries, 5(3), pp.255-276.

Field, I.C., Meekan, M.G., Buckworth, R.C. and Bradshaw, C.J., 2009. Susceptibility of sharks, rays and chimaeras to global extinction. Advances in marine biology, 56, pp.275-363.

Gallagher, A.J., Kyne, P.M. and Hammerschlag, N., 2012. Ecological risk assessment and its application to elasmobranch conservation and management. *Journal of Fish Biology*, *80*(5), pp.1727-1748.

Morris, W.F. and Doak, D.F., 2002. Quantitative conservation biology. *Sinauer, Sunderland, Massachusetts, USA*.

Punt, A.E. (2000) Extinction of marine renewable resources: a demographic analysis. Population Ecology 42, 19.

VanderWright, W.J., Dudgeon, C.L., Erdmann, M.V., Sianapar, A. and Dulvy, N.K., 2021. Extinction risk and the small population paradigm in the micro-endemic radiation of Epaulette sharks. *Encyclopedia of Imperilled Species, MJ Costello, ed.(Elsevier)*.

5. *Is the length of the foreseeable future (described in section 5.2) appropriate for the species based on the best available information, and is adequate justification provided?*

Re: “the foreseeable future extends only so far into the future as we can reasonably determine that both the future threats and the species’ responses to those threats are likely”.

Based on the information in the status review, it is straightforward to determine that directed or incidental fishing mortality from a variety of fleets and fisheries will remain the primary threat in

future years throughout the range of shortfin mako. The future magnitude of fishing mortality remains uncertain, given that management regulations have markedly changed in specific components of the species range (i.e. the North Atlantic) and future conservation efforts seem likely in other areas or oceans. As discussed by the authors, any evaluation of recent management changes will not become possible until 2024 at the earliest for the North Atlantic and evaluation in other areas will largely depend on improvements to catch reporting.

It is more difficult to determine whether one generation is a reasonable timeframe over which to quantify the response of shortfin mako to changes in fishing mortality. I agree with the authors that future projections are inherently uncertain and uncertainty increases with the projection interval (e.g., Patterson et al. 2001). When evaluating indices (e.g. trends in CPUE indices), there is an inherent delay in being able to describe a trend, in that several years of data are required before systematic changes become evident. However, one generation time would be more than sufficient to understand changes in parameters such as catch rates or removals (i.e. data collected on an annual basis).

It is less clear whether one generation time would be sufficient to describe population responses to changes in mortality (i.e. increased/decreased recruitment leading to changes in abundance). From the output of the SS assessment for the North Atlantic stock, future projections indicated continued abundance decline from 2019-2035 due to the length of time necessary for juveniles to start contributing to population growth through reproduction. If these projections had only considered one generation, they would have gone until ~2045, or only two 5-year timesteps past. This implies that it is only possible to demonstrate population-level response after approximately 15 years, provided our understanding of maturity and longevity in shortfin mako is correct. It would require more time if shortfin mako were longer-lived, given that greater longevity is associated with later maturity. Table 1 lists one longevity estimate for shortfin mako that is 56 years (NE Pacific) rather than 29 (SW Pacific and NW Atlantic).

The authors make the argument that the propagation of uncertainty in population projections demonstrate that two generation times is inappropriate for the foreseeable future. Due to their very nature, projections only demonstrate the logical outcome of the assumptions that are being made. As an example from the SS projections mentioned above (ICCAT 2019), if total fishing mortality was 500 tonnes in 2019 and in every year subsequent to that, could the shortfin mako stock in the North Atlantic increase with what we know about its reproductive potential? Table 10C suggests that this statement is true from 2035 onwards, and further suggests that abundance increase would be fast enough to reach MSY within 2 generations for > 50% of simulated trajectories. Obviously if any of the assumptions underlying predictions change (e.g. fishing mortality was not 500 t in 2019, but 1882 t), the corresponding projections and their associated probabilities are rendered inaccurate, regardless of how uncertainty is propagated in the projections.

In my mind, projecting over a longer interval for a long-lived species enables better discrimination among different scenarios because it gives changes time to manifest in the population. For example, even though TAC levels of 100-1100 tonnes all appeared inappropriate after 25 years, (in that shortfin mako in the North Atlantic remained overfished with overfishing occurring with > 50% probability), it became obvious that low levels of mortality may still allow rebuilding over 2 generation times. I consider the issues related to propagation of uncertainty in projections to be less influential on conclusions than considerations around the biological limits to population growth, particularly because it is known *a priori* that projections are inaccurate

(see the discussion of assumptions above). If population response to changes implemented now only start to become measurable after 15 years, using 25 years as the foreseeable future provides a maximum of 10 years that would be indicative of population response. As detailed above, several years of data are required before systematic changes become evident in annual timeseries. 10 years is quite a short timeframe over which a trend can be assessed, particularly one that is likely to be small. My preference would be to use two generation times for the foreseeable future when evaluating risk, to ensure that the time period encompasses the duration over which changes in productivity would be expected to occur (given the life history of shortfin mako) and be measurable. Using a 50-year time period would also encompass the uncertainty in longevity estimates (i.e. would be approximately 1 generation if shortfin mako live for 56 years; Carreon-Zapiain et al. 2018)

Patterson, K., Cook, R., Darby, C., Gavaris, S., Kell, L., Lewy, P., Mesnil, B., Punt, A., Restrepo, V., Skagen, D.W. and Stefánsson, G., 2001. Estimating uncertainty in fish stock assessment and forecasting. Fish and fisheries, 2(2), pp.125-157.

6. *Are the results and conclusions of the Extinction Risk Analysis supported by the information presented?*

Re: Rangewide Extinction Risk Analysis

The small population paradigm (Caughley 1994) identifies several characteristics that increase a population's extinction risk: environmental stochasticity and catastrophic events, demographic stochasticity, and possible Allee effects. As a species, shortfin mako are robust to environmental stochasticity due to their long life history (Kindsvater et al. 2016) and catastrophic events (i.e. sudden, large mortality events) are extremely unlikely to occur simultaneously in multiple oceans. From the information contained in the report, shortfin mako appear to occupy their historic distribution, with the possible exception of the Mediterranean, and habitat capacity is unlikely to be limiting abundance. Even if global abundance estimates were lower, it is unlikely that demographic stochasticity (e.g. random increases/decreases in offspring of a single sex) would reduce reproductive potential by any substantial degree. While I know of no studies that specifically evaluate Allee effects or depensation, the available catch data suggests that shortfin mako are continuously distributed over large areas, with a high degree of overlap among several ontogenetic stages (e.g. Natanson et al. 2020). While distribution is not directly related to per capita reproductive success, the species continuity and relative frequency in several oceans indirectly suggests mating success and reproductive output might be similar rather than declining. Lastly, the relatively high haplotype diversity in shortfin mako sharks would be inconsistent with a recent population bottleneck. However, it is unknown how small a population might have to be to exhibit evidence for a genetic bottleneck, given that only 42% of invasion events for marine fishes exhibited evidence of a bottleneck despite small founder populations (Chiesa et al. 2019).

Conversely, the majority of shark species are thought to conform to the declining population paradigm (Caughley 1994). Extinction risk is not a function of the number of animals but can occur because mortality rates exceed population growth rates (e.g. VanderWright et al. 2021). Slow growth rates, large body size and comparatively low reproductive output of shortfin mako sharks are associated with higher extinction risk (e.g. Walls & Dulvy 2021). A key consideration under the declining population paradigm is that when growth rates drop below replacement (e.g. lambda < 1), the population has no capacity to increase in size and continuous decline to ultimate extinction is assured, even if it takes many years (termed extinction debt; summarized

in Figueiredo et al. 2019). Shortfin mako shark exhibit characteristics associated with delayed extinctions, namely life history traits that prolong individual survival (i.e. high longevity) and potentially low levels of connectivity across oceans (potential metapopulation structure). While it is extremely unlikely that there will be zero shortfin mako shark in global oceans within a 25 year (or even a 50-year) time-period, it is less clear whether the species would ultimately decline to extinction. Population growth rates or intrinsic rates of increase (e.g. Cortés 2016) are low for shortfin mako and the past evaluations of productivity (referenced in the report) gave values nearing 1 under a no fishing scenario, and < 1 when fishing was incorporated. When coupled with the observation that the current data on catches is underestimated (and possibly highly underestimated in specific regions), it is possible that shortfin mako have much higher extinction risk, particularly in specific parts of their range. In the case when mortality exceeds population growth potential, extinction risk becomes 1 (i.e. extinction will occur in the future because the abundance will continually decline) unless there are substantive changes to mortality rates.

Connectivity throughout their range is taken as evidence of resilience to localized overutilization and stochastic events. The theoretical basis for these statements are largely derived from research on metapopulations (Hanski 1998), where source-sink dynamics and the rescue effect are considered to buffer the effects of environmental variability or localized depletion. For one concentration of shortfin mako (e.g. South Atlantic) to rescue another (e.g. North Atlantic), there have to be sufficient migrants entering the North Atlantic to make up for any deficiency in reproduction in the North (i.e. added individuals ensure that reproduction > fishing mortality). At the same time, the loss of those migrants from the South Atlantic can't be so great as to make mortality > reproduction in the South. I consider it very unlikely that transoceanic dispersal (e.g. from the Pacific to Atlantic) could meaningfully change productivity in the Atlantic over ecological timescales (e.g. decades). Also, apparently low rates of movement between hemispheres from the tagging data suggests that dispersal rates may not be high enough for Southern shortfin mako to rescue the Northern stock. I further note that shortfin mako have extremely limited capacity for abundance increase, as supported by all risk assessments, evaluations of relative population growth rates, and estimates of lambda in this document.

This exact scenario has been simulated relative to salmon populations – i.e. would the rescue effect prevent extinction when productivity was declining to 1 (Bowlby & Gibson 2020). Because permanent losses through emigration are indistinguishable from losses due to mortality (i.e. dispersing individuals are removed from the source population), dispersal essentially acts as an additional source of mortality on larger populations without sufficiently increasing productivity in smaller populations, and the metapopulation overall has higher extinction risk, specifically when productivity approached 1 (Bowlby & Gibson 2020). Note that differences in life history between sharks and salmon would not impact these conclusions, given that the model simulated productivity levels that can theoretically arise from any life history. When productivity in any population approaches 1, I consider there to be a high risk of extinction and/or local extirpation when threats also remain high (e.g. in the North Atlantic).

There is some precedence for this viewpoint for a large shark species, in that basking shark in the Canadian Pacific were common until targeted extermination programs and high fishing mortality essentially eliminated them from Canadian waters. Even though basking shark are still present in nearby US waters and are commonly seen in other regions and oceans, they remain functionally extirpated in Canada (e.g. McFarlane et al. 2008).

Ultimately, I agree with the authors that shortfin mako (as a species) will persist globally in a 25 year time period, but I consider it likely that the species will gradually decline to extinction (over an unspecified time frame) if fishing mortality continues to outweigh reproductive potential. Data in the report supports the idea that fishing mortality remains high throughout the global range of shortfin mako. The magnitude that it is underestimated remains unknown and has the potential to be substantial. The combination of at-vessel and post-release mortality will ensure that shortfin mako continue to experience fishing mortality even under moratorium, unless extremely effective spatiotemporal management strategies can be developed. The majority of information that quantifies longline effort in this report suggests it is increasing in several oceans, so the extent of overlap would be anticipated to remain high. I consider it unlikely that connectivity will protect the species from localized extirpation. Given these factors, I have a higher overall perception of risk than the assessment team.

Bowlby, H.D. and Gibson, A.J.F., 2020. Evaluating whether metapopulation structure benefits endangered diadromous fishes. *Canadian Journal of Fisheries and Aquatic Sciences*, *77*(2), pp.388-400.

Caughley, G., 1994. Directions in conservation biology. *Journal of animal ecology*, pp.215-244.

Chiesa, S., Azzurro, E. and Bernardi, G., 2019. The genetics and genomics of marine fish invasions: a global review. *Reviews in Fish Biology and Fisheries*, *29*(4), pp.837-859.

Figueiredo, L., Krauss, J., Steffan-Dewenter, I. and Sarmento Cabral, J., 2019. Understanding extinction debts: spatio–temporal scales, mechanisms and a roadmap for future research. *Ecography*, *42*(12), pp.1973-1990.

Hanski I. 1998. Metapopulation dynamics. Nature, **396**: 41–49.

Kindsvater, H.K., Mangel, M., Reynolds, J.D. and Dulvy, N.K., 2016. Ten principles from evolutionary ecology essential for effective marine conservation. *Ecology and Evolution*, *6*(7), pp.2125-2138.

McFarlane, S., J. King, K. Leask and L.B. Christensen, 2009. Assessment of information used to develop a Recovery Potential Assessment for basking shark Cetorhinus maximus (Pacific Population) in Canada. DFO Can. Sci. Advis. Sec. Res. Doc. 2008/071. vi + 98 p

VanderWright, W.J., Dudgeon, C.L., Erdmann, M.V., Sianapar, A. and Dulvy, N.K., 2021. Extinction risk and the small population paradigm in the micro-endemic radiation of Epaulette sharks. *Encyclopedia of Imperilled Species, MJ Costello, ed.(Elsevier)*.

Walls RHL, Dulvy NK (2021) Tracking the rising extinction risk of sharks and rays in the Northeast Atlantic Ocean and Mediterranean Sea. Sci Rep 11:15397

Re: Significant Portion of its Range Analysis

Without an explicit evaluation of the rate of abundance decline in the future from the SS projections for the North Atlantic (which would optimally use actual values for mortality– i.e. ~ 1700 t under live release restrictions not the levels evaluated by ICCAT 2019), I consider extinction risk to be high to moderate for this population segment given that: (1) initial management changes to protect the stock (2018 onward) resulted in fishing mortality that was ~ 7 times higher than the level considered appropriate in the current rebuilding plan (> 1700 t vs. 250t). This catch was supposed to represent animals that were dead at vessel plus post-release

mortality, which implies that interaction rates with shortfin mako remain very high from fisheries in the North Atlantic. (2) productivity for shortfin mako as a species is consistently estimated as being very low, with the available estimates for population growth potential below 1 when fishing is accounted for, (3) demand remains high, both nationally as well as internationally in trade, and (4) total abundance decline from historical levels is expected to be substantial before current consideration measures benefit productivity. As a rough calculation for illustration purposes: the report references a range of 47-60% as the decline in the North Atlantic from 1950-2015, with continued decline predicted until 2035 even under zero fishing mortality. Being conservative and using 50% as the historical decrease, this translates to an annual rate of decline of ~1% per year. Assuming this rate remains constant over the next 19 years (2016-2035), total abundance decline would be ~90% from 1950-2035 before productivity would increase. All estimates of resiliency (i.e. how fast shortfin mako would increase following declines in abundance) provided in the report are very low. With the uncertainty associated with the absolute estimate of abundance in the North Atlantic, population size of shortfin mako in the North Atlantic has the potential to become extremely small relative to historic values before any recent conservation measures begin to benefit reproductive capacity and promote abundance increase.

For the South Atlantic, I agree with the assessment team that extinction risk is lower based on the available data. I would place the level in the moderate to low range.

Re: Distinct Population Segments.

Beyond the comments above on haplotype and allelic diversity, analyses using more modern and robust genetic techniques (e.g. SNPs) are better able to characterize genetic variation in natural populations (summarized in the introduction of Aylward et al. 2022). When these have been applied to shortfin mako, they reveal interesting and discernable patterns of diversity within the Atlantic Ocean (Takeshima et al. 2022). The existence of these patterns within a single ocean suggests that global evaluation using new techniques would be very unlikely to conclude shortfin mako are globally panmictic. I recognize that the document became available immediately after the April timeframe, yet I think consideration of its findings are necessary in light of the importance of spatial structure and diversity on the evaluation of extinction risk.

For shortfin mako specifically, I would argue for the existence of largely discrete populations among ocean basins for four main reasons: (1) there is some evidence of genetic variation at neutral markers in addition to more recent work using more advanced technology that supports the idea of considerable variability in shortfin mako (Takeshima et al. 2022), (2) available life history data indicates diversity among oceans in key vital rates and this diversity is more likely adaptive. Life history characteristics ultimately determine reproductive potential and thus would be more influential on future population dynamics in relation to exploitation rates, (3) low levels of connectivity among ocean basins are indicated from the experimental tagging data, in that the majority of tagged individuals did not migrate among hemispheres or oceans, and (4) fishing mortality is inappropriately high in specific regions (e.g. the North Atlantic) with little potential for rescue from shortfin mako in other areas, given low dispersal rates and very low productivity estimates.

Aylward, M., Sagar, V., Natesh, M. and Ramakrishnan, U., 2022. How methodological changes have influenced our understanding of population structure in threatened species: insights from tiger populations across India. *Philosophical Transactions of the Royal Society B*, *377*(1852), p.20200418.

Takeshima et al. 2022. Preliminary results of the genetic population structure of the Atlantic shortfin mako using mitogenomics and nuclear-genome-wide single-nucleotide polymorphism genotyping based on additional samples comprehensively collected from in and around the Atlantic Ocean. SCRS/2022/085. [Presented at the sharks intersessional meeting of ICCAT in May 2022].

Editorial Comments (by section of the report; reviewer numbers are not associated with order of names as they appear above)**:**

LIFE HISTORY AND ECOLOGY
Taxonomy

Reviewer 3: Regarding citation of Compagno 1984: Would it be worth citing the most recent version? Compagno, L. J. V. 2001. Sharks of the world. An annotated and illustrated catalogue of shark species known to date. Vol. 2: bullhead, mackerel, and carpet sharks (Heterodontiformes, Lamniformes and Orectolobiformes). FAO species catalogue for fishery purposes 1. FAO, Rome. 269 pp.

Distribution and Habitat Use

Reviewer 3: add "From traditional dart tagging data," to the beginning of the sentence Maximum recorded time at liberty at sea is 12.8 years, and the maximum straight-line distance between tag and recapture localities is 3,043 nautical miles (NM) (Kohler and Turner 2019).

Reviewer 3: add "Shorter-term" to the beginning of the sentence "Electronic tagging results from several studies indicate that the species commonly makes roundtrip migratory movements of more than 20,000 km (Francis et al. 2019)."

Reviewer 3: It would be useful to give this a time-frame – is it over the course of a year? (regarding roundtrip migratory movements of more than 20,000 km)

Reviewer 3: Regarding the sentence "It is unknown whether these behavioral states are tied to feeding or breeding behavior as both states were observed to last for several months,…" the reviewer comments the following: the way this is written implies that breeding or feeding behaviours are the only two options for why the species might exhibit two behavioural states.

Reviewer 3: Is there some evidence that adults tend to be concentrated more offshore? I am thinking of: Kai, M., Thorson, J.T., Piner, K.R. and Maunder, M.N., 2017. Spatiotemporal variation in size-structured populations using fishery data: an application to shortfin mako (Isurus oxyrinchus) in the Pacific Ocean. Canadian Journal of Fisheries and Aquatic Sciences, 74(11), pp.1765-1780.

Reviewer 3: Regarding the sentence "The vertical distribution of shortfin mako sharks is affected by numerous environmental variables,…" the reviewer comments the following: These results represent various types of correlations and did not evaluate causation. The reviewer suggests replacing "affected by" with "related to."

Reviewer 3: Regarding the sentence ""Bounce" or "yo-yo" diving behavior, in which individuals repeatedly descend to deeper water and then ascend to shallow depths, has been regularly observed in both adults and young-of-the-year (YOY) (Sepulveda et al. 2004; Abascal et al. 2011; Vaudo et al. 2016; Santos et al. 2021)." The reviewer comments the following: This implies that diel diving behaviour is restricted to two age classes, rather than being a fairly general characteristic of several shark species (e.g. porbeagle; Francis et al. 2015; Wang et al. 2020;

thresher, Cartamil et al. 2010; blue shark, Braun et al. 2019, etc.). Is there specific evidence that shows juvenile mako don't do this type of diving? I am also unsure why a new term is being used (yo-yo diving). Am I missing why this is different from diel diving patterns?

Reviewer 3: Regarding the sentence "However, conventional tagging data indicates that mixing does occur across these features (see Figure 4 below; Kohler and Turner 2019). The reviewer suggests adding "a low level of" before "mixing does occur," and adding ", with xxx of xxxx tagged animals crossing the equator" after "features." The reviewer comments the following: An alternate interpretation would be that the conventional tagging data as well as the satellite tagging data support the idea that certain ocean currents or features limit dispersal. While these dispersal events have occurred, nothing in the data below suggests that they are common.

Reviewer 3: Regarding the sentence "In the Pacific, tagging data supports east-west mixing in the north and minimal east-west mixing in the south (see Figure 5 below; Sippel et al. 2016; Corrigan et al. 2018)." the reviewer comments: Is this on the basis of the single 'green' animal in the plot below that crossed the mid-Atlantic ridge? The reviewer also comments the following: Possibly a function of tagging locations? Nothing was tagged off the coast of Africa in Figure 5 below. Reviewer also suggests addition of "(one animal)" after "north," replacing "minimal" with "could not fully evaluate," and "due to a lack of tagging along the African coast" after "south."

Reviewer 3: Regarding the sentence "Genetic studies indicate a globally panmictic shortfin mako shark population with some genetic structure among ocean basins, as discussed further in section 2.5 Population Structure and Genetics." the reviewer comments: Because the nuances to this interpretation are given in a different section, my preference would be to make a statement about population structure relative to the tagging data exclusively and leave consideration about genetics in section 2.5. This would be a good place to document how the tagging data has been interpreted in the context of stock assessment. For example, the observation that ICCAT considered two stocks: North and South Atlantic as the appropriate units for assessment was largely based on the tagging data."

Reviewer 3: In the sentence "The presence of mature and pregnant females in the Gulf of Mexico provides further support that this is a gestation and parturition ground for the species; however, neonates are more widely distributed along the coast of North America and largely overlap with the distribution of older immature sharks and adults (Natanson et al. 2020)." The reviewer suggests adding "fisheries data suggests that pupping is geographically widespread in the northwest Atlantic, given that neonates" after "however" and deletion of the word "more" before "widely distributed."

Feeding and diet

Reviewer 3: Throughout this section, there is no mention of marine mammal prey. I know of this record, which I recognize documents data from only one mako. e.g. Lyons, K., Preti, A., Madigan, D.J., Wells, R.J.D., Blasius, M.E., Snodgrass, O.E., Kacev, D., Harris, J.D., Dewar, H., Kohin, S., MacKenzie, K., and Lowe, C.G. 2015. Insights into the life history and ecology of a large Shortfin Mako shark Isurus oxyrinchus captured in southern California. J. Fish Biol. 87:200-211

Reviewer 3: There is some indication from the Mexican Pacific of ontogenetic shifts in diet – although I recognize that this paper did not have sufficient sampling on adult makos to truly evaluate the hypothesis. Malpica-Cruz, L., Herzka, S.Z., Sosa-Nishizaki, O., and Escobedo-Olvera, M.A. 2013. Tissue-specific stable isotope ratios of Shortfin Mako (Isurus oxyrinchus) and white

(Carcharodon carcharias) sharks as indicators of size-based differences in foraging habitat and trophic level. Fish. Oceanogr. 22(6):429-445.

Growth and Reproduction

Reviewer 2: Are the ERAs and assessment based on the most recent, validated ageing?

Reviewer 3: Regarding discussion of vertebral band deposition, the reviewer comments the following: I am unsure why there is so much detail on the uncertainty in band pair deposition to determine age in historical research (that had no validation method) when the maximum age estimates come from studies that incorporated validation. Is there a concluding sentence that could be added, to guide the reader on what was drawn from this paragraph? E.g. Does the uncertainty in band pair deposition likely lead to underestimation of age in the oldest and largest individuals? Is it likely that band pair deposition rates change as animals grow and one method may be preferred for individuals of a particular size? Etc.

Reviewer 3: It is not immediately apparent to the reader why only specific values from certain oceans were detailed in the text, rather than the suite of values from Table 1. The reviewer suggests adding "Other estimates are detailed in Table 1."

Reviewer 3: Is there any interest or need to report weight at maturity? E.g. Natanson et al. 2020?

Reviewer 3: It would be very useful to have some idea of the conversion between FL and TL to be able to compare values in the table below. I recognize the issues with conversion factors and the need to show the metrics in the manner they were reported. As a general comment, conversion factors are needed to standardize information among studies or for stock assessment and could be identified explicitly as a source of uncertainty affecting our understanding of life history and population dynamics.

Reviewer 3: Regarding estimate of theoretical longevity of 56 years (Carreon-Zapiain et al. 2018) in Table 1, the reviewer comments the following: Is there a specific reason that this value is considered less credible and not discussed in the text above? Also, NE Pacific has a theoretical longevity estimate plus a length at maturity but no growth model?

Reviewer 3: Regarding Table 1 row titled "Growth rate (von Bertalanffy growth function)" the reviewer comments the following: More than the growth rate is being reported in this section. Because of this, would it be possible to include the parameter estimates from any type of growth model that was chosen as most representative from each study (i.e. not only VBGF)? This would likely address my comment above for the NE Pacific.

Reviewer 3: I feel that it is misleading to present the growth models from these two studies [Pratt Jr. and Casey 1983; Natanson et al. 2006] without recognizing that they are directly dependent on each-other and that one is considered more representative than the other. Natanson et al. used the original data from Pratt & Casey as well as additional samples in their analyses, and they also specifically undertook the work to address issues with the original analyses. In light of this, I would consider Natanton et al. to provide a more robust growth model than Pratt & Casey.

Population Structure and Genetics

Reviewer 3: Regarding the sentence “Although certain ocean currents and features may limit dispersal between different regions to a small degree as discussed in section 2.2…” the reviewer comments the following: This is inconsistent with the data above – if dispersal was limited to only a small degree, it should be relatively common in the population, rather than rare.

Reviewer 3: Suggests adding sample sizes for all studies referenced.

Reviewer 3: Regarding the sentence “Corrigan et al. (2018) also found evidence of matrilineal structure from mtDNA data, while nuclear DNA data provide support for a globally panmictic population.” the reviewer suggests replacing “provide support for a globally panmictic population” with “showed no evidence of population structuring from xxx samples in the xxx ocean.”

Reviewer 3: Regarding the sentence “Taken together, results of genetic analyses suggest that female shortfin mako sharks exhibit fidelity to ocean basins, possibly to utilize familiar pupping and rearing grounds, while males readily move across the world’s oceans and mate with females from various basins to produce a single population (Heist et al. 1996; Schrey and Heist 2003; Taguchi et al. 2011; Corrigan et al. 2018).” the reviewer suggests removing the word “readily,” replacing “produce a single population” with “homogenize genetic variability,” and comments the following: None of the information provided above suggests that the rate males move is substantial, just that it is enough that the null hypothesis of genetic homogeneity can’t be rejected. Please also see the discussion in the charge statement.

Reviewer 3: As written, the information on haplotype diversity does not directly relate to the question of population structure. Can an introductory or concluding sentence be added on what low haplotype diversity would indicate (e.g. linked to population bottlenecks and extremely low abundance)?

Demography

Reviewer 3: In the sentence “Using seven life-history invariant methods, Cortés (2016) estimated the instantaneous rate of natural mortality (M) to be 0.075 yr^{-1}.”the reviewer comments the following: Is this an average? What is the range of the estimates? The reviewer also suggests rewriting as “Using invariant life-history parameters and seven analytical methods,…”

Reviewer 3: The reference list includes Murua et al. 2018, which gave a lambda estimate of 1.049 (1.036-1.061) in Table 4. Shortfin mako was assessed as having the highest vulnerability of any species in the analyses. The reviewer suggests adding “In an updated risk analysis, these values became…” to address this comment.

GLOBAL AND REGIONAL ABUNDANCE ESTIMATES AND TRENDS
Global Population Trends

Reviewer 3: Regarding the sentence “A global trend was estimated by weighting each region’s trend by the relative size of each region.” The reviewer comments: Please add information on the extrapolation to 3 generations as well (to standardize the time period over which the trends were calculated).

Reviewer 3: Regarding the sentence “The Just Another Red list Assessment (JARA) framework (Winker et al. 2018; Sherley et al. 2019) used by Rigby et al. (2019) has been described as

inappropriate for this long-lived, sexually dimorphic species as it only uses mean annual trends in the population over the assessment period and does not consider size or age structures of the population over recent decades (Kai 2021a)." the reviewer comments: This same critique could be leveled at some of the models used for regional assessments, e.g. surplus production models.

Regional Population Trends, North Atlantic

Reviewer 2: The reviewer suggests adding "spawning" before the word "stock" in two instances

Reviewer 3: Regarding the sentence "While a non-retention policy would ostensibly reduce mortality, shortfin mako sharks frequently interact with surface longline fisheries and the potential inability for fishermen to avoid the species may not lead to sufficient decreases in mortality; therefore, the SCRS noted that other management measures, such as time-area closures, reduction of soak time, safe handling and best release practices, may also be required (ICCAT 2019)." The reviewer suggests adding "total" before the word "mortality" in two places, adding "given that capture and post-release mortality would still occur." before "therefore, the SCRS…," and other minor editorial changes.

Reviewer 3: The reviewer suggests minor editorial changes to the paragraph beginning "The 2017 stock assessment and 2019 update to the stock assessment present more accurate and rigorous…"

Reviewer 2: The reviewer suggests adding the sentence "Finally, the age structured model in 2017 more accurately captured the time-lags in population dynamics a long-lived species than the production models used in 2012"

Reviewer 3: The reviewer suggests rewriting the sentences "However, ICCAT soon adopted Recs. 17-08 and later 19-06 (available at https://www.iccat.int/Documents/Recs/compendiopdf-e/2019-06-e.pdf), which both encourage release of live sharks, effectively limiting the harvest of shortfin mako shark. The estimated fisheries mortality could, therefore, be elevated relative to the reductions caused by these management measures." to "However, ICCAT soon adopted Recs. 17-08 and later 19-06 (available at https://www.iccat.int/Documents/Recs/compendiopdf-e/2019-06-e.pdf), which both encourage release of live sharks, which would be expected to reduce fishing mortality. Thus, the 2017 estimates are likely higher than what actually occurred under the two new recommendations." The reviewer comments the following: To my knowledge, there was no explicit evaluation of the effectiveness of the live release measure on shortfin mako.

Reviewer 3: Regarding the sentence "Long-term combined trends for shortfin mako shark and porbeagle (*Lamna nasus*) in the Mediterranean Sea indicate up to a 99.99% decrease in abundance and biomass since the early 19th century, though considerable variability among datasets as a result of geography and sample size was noted (Ferretti et al. 2008)." The reviewer comments: Just curious, but would these trends be affected by species identification issues?

Reviewer 3: The reviewer suggests deleting "(suggesting breeding/pupping in the region)" in the sentence "While shortfin mako sharks spanning a broad range of sizes (suggesting breeding/pupping in the region) are occasionally reported as bycatch in swordfish and albacore longline fisheries (Megalofonou et al. 2005), or in other artisanal or commercial fisheries (Kabasakal 2015), from the eastern Mediterranean Sea, no reliable estimates of abundance are available for this region." The reviewer comments the following: In Section 2.2, a concentration of adults and/or YOY and small juveniles are taken as evidence of pupping and breeding

locations, not a broad size distribution. Given the high mobility of the species, a broad size distribution could also result from movement rather than reproduction within the Mediterranean.

Reviewer 2: The reviewer suggests adding "for the mature component of the stock" after "…the shortfin mako shark has experienced historical declines in the North Atlantic Ocean, which will continue until at least 2035."

Regional Population Trends, South Atlantic

Reviewer 3: My understanding of the 2017 assessment was that conflicting trends in the CPUE indices relative to productivity and removals resulted in the high degree of uncertainty, not poor data quality per se. i.e. results from different modeling approaches were highly inconsistent.

Regional Population Trends, North Pacific

Reviewer 3: In the sentence "The most comprehensive information on trends for shortfin mako sharks in the North Pacific comes from the 2018 ISC Shark Working Group stock assessment,…" the reviewer suggests deletion of "on trends" and comments the following: This was suggested only because the rest of the sentence deals with stock assessment outcomes, not only trend information.

Reviewer 3: In the sentence "While the IUCN Red List Assessment evaluates the risk of a taxon going extinct, stock assessments evaluate the status of a stock relative to productivity of the stock and its capability to produce MSY." the reviewer comments the following: Taxon here is taken to mean a group of populations of the same species?

Reviewer 3: Regarding the sentence "However, these data represent trends for both longfin and shortfin mako sharks combined , and the performance of the standardization model was poorer than for other studied shark species, making the estimated trend less reliable." the reviewer comments the following: It is my understanding that longfin mako are relatively rare in comparison with shortfin mako so the trend would be largely driven by shortfin?

Reviewer 3: Regarding the sentence "Considering that standardized CPUE trends from observer data are more robust and reliable than trends from fishery logbook data, we find that the best scientific information available indicates that shortfin mako sharks in the North Pacific are neither overfished nor experiencing overfishing, and the population is likely stable and potentially increasing, despite evidence of historical decline." the reviewer comments the following: The conclusions re: trends didn't seem to follow from the fishery-independent data, more from the output of the stock assessments. For example: the ISC stock assessment uses fishery dependent data and indicates very slight increase in recent years (1.8%); the fishery-independent WCPFC data gives a significant decline of 7%, and catch rates in the fishery-independent Japanese research vessel logbook data also show decreases since 2008 (fishery-dependent show increases). The updated indicator analysis from Rice only goes to 2010. The fishery-independent trend data seem to be mostly indicating decline, but the more comprehensive stock assessments typically suggest that the population is stable/increasing, but tend to rely heavily on fishery-dependent indices. Can this paragraph be reworded to more clearly identify the grounds for the conclusions re: trends?

Regional Population Trends, South Pacific

Reviewer 3: Regarding the sentence "Trend analysis of modeled biomass indicates a median increase of 35.2% over three generation lengths (Rigby et al. 2019)." the reviewer comments the following: While it does not change the conclusions, this statement is misleading as the Rigby et al trend analysis uses a constant rate to extrapolate outside of the range of the data to get a prediction over 3 generation lengths. The rest of the paragraph refers to the rate of change within the limits of the data being analyzed. For all of the regional information from Rigby, it seems more comparable to me to give the estimated annual rate of change, where for the South Pacific during 1995-2013 is +0.48% (supplementary information; Figure 3b).

Regional Population Trends, Indian Ocean

Reviewer 3: The reviewer suggests adding the sentence "Catch data have the potential to be substantially underestimated and the increase in CPUE from the Taiwanese fleet may not reflect trends in abundance." The reviewer comments the following: Please see comments in the charge statement about the need to identify instances where data are not just uncertain, but known to by systematically biased.

Regional Population Trends, Summary

Reviewer 3: Minor editorial changes are suggested.

ANALYSIS of ESA SECTION 4(a)(1) FACTORS
(B) Overutilization for Commercial, Recreational, Scientific or Educational Purposes; Commercial and artisanal fisheries

Reviewer 3: Regarding the sentence "The species is targeted in semi-industrial and artisanal fisheries in the Indian and Pacific Ocean, and as a sportfish in several recreational fisheries, though recreational fisheries contribute to the species' overutilization to a minimal degree in comparison to impacts from commercial fisheries." the reviewer suggests minor editorial changes, and comments the following: I do not know of any research at a global scale that specifically evaluated the relative contribution of various types of fisheries to shortfin mako mortality (e.g. recreational, artisanal, semi-industrial and commercial). This comparison exists for Canada, in the 2019 COSEWIC status report and references therein. https://www.canada.ca/en/environment-climate-change/services/species-risk-public-registry/cosewic-assessments-status-reports/shortfin-mako-2019.html.

Reviewer 3: Add "The types of leaders or branch lines were not reported." to the paragraph discussing results from Campana et al. (2016), for consistency with other gear descriptions.

Reviewer 3: Regarding post-release mortality rate discussion, the reviewer comments the following: I consider it important to say why the duration of monitoring is important and specifically how it would affect the estimates. The reviewer suggests adding: "Any mortality related to capture and handling that occurs after the monitoring period would cause post-release mortality rates to be underestimated (Musyl et al. 2009, Musyl & Gilman 2019)."

Reviewer 3: The reviewer suggests adding sample sizes and number of tagged sharks in discussion of post-release mortality.

Reviewer 3: The reviewer suggests deletion of the following sentence: “The study has since added additional tags on shortfin mako sharks to the dataset and the analysis will be rerun in mid-2022.” The reviewer comments the following: This statement seems tangential to the goal of the paragraph. I am sure that similar statements would apply to a lot of the research discussed throughout this status report.

Reviewer 3: The reviewer suggests adding “and Bowlby et al. (2021) found a significant increase in recovery time following capture on circle hooks” to the discussion of hook type.

Reviewer 3: The reviewer notes that Keller et al. 2021 is not in reference list.

Reviewer 3: Regarding the sentence “Upon correcting for these errors, the results presented by Keller et al. (2021) demonstrate that there is no significant difference in shortfin mako shark retention due to hook type. Furthermore, at-haulback mortality decreases by 10% due to circle hook use, a result that is statistically significant.” the reviewer comments the following: One of two data treatment errors documented by Keller et al centered around the confounding effect of bait type in Foster et al. 2012, and its resulting impact on meta-analyses. I agree that teasing apart the relative contribution of different variables is extremely difficult when trying to understand catch rates or mortality. It is not immediately obvious how best to treat the data. As an example, splitting information into two components (as was done in the Keller re-analyses) reduces the statistical power of the test a priori, meaning that it becomes more difficult to reject the null hypothesis at a 5% level simply because of smaller sample sizes. On the other hand, unbalanced data relative to correlated covariates is a distinct issue in analyses and one of the solutions is to split information into two components to control for confounding effects. My worry is that the re-analysis was one-sided (i.e. only evaluating issues in studies suggesting high catchability, not in all studies contributing to meta-analyses on catchability and at haul-back mortality). For example, Carruthers et al. 2009 reported the odds of survival are significantly higher on circle hooks, however the authors were not able to control for the effect of bait type. If bait type is considered a confounding variable that must be controlled for in all analyses, Carruthers should have been excluded, which would have an impact on the statistical significance of any differences in mortality at haulback. In the absence of such standardization among all studies contributing to meta-analyses, I do not agree that such definitive statements can be made.

Reviewer 3: Regarding the sentence “Bringing incidentally caught shortfin mako sharks on deck to remove gear has recently been shown to reduce survival and increase recovery times, and therefore may result in delayed post-release mortality (Bowlby et al. 2021).” the reviewer suggests deletion of “and therefore may result in delayed post-release mortality” with the following comment: Animals that recovered from capture and handling were not thought to then experience post-release mortality at a later date due to the capture process. They had recovered.

Reviewer 3: Regarding the sentence “Given that other nations targeting swordfish and tuna in the Northwest Atlantic and other ocean basins use similar gear configurations as used in the study by Campana et al., similar un-reported mortality levels may be impacting the shortfin mako shark were prohibited throughout its global range.” the reviewer suggests replacing “impacting” with “expected if landings of” and addition of “were prohibited” before

"throughout its global range." The reviewer comments the following: I think it is an important distinction that animals that die during capture (at-vessel mortality) tend to be landed by the majority of fleets, so the Canadian estimates wouldn't suggest that total mortality would be landings plus 49%. The unreported mortality would have only been the PRM component or 30-33%.

Reviewer 3: Other minor editorial changes suggested.

(B) Overutilization for Commercial, Recreational, Scientific or Educational Purposes; Commercial and artisanal fisheries, North Atlantic Ocean

Reviewer 3: Regarding the sentence "Across the North Atlantic, shortfin mako sharks are incidentally caught mainly in pelagic and surface longlines, and to a lesser extent, demersal trawls and gillnets." the reviewer comments the following: Wouldn't purse seine be the second most common gear type? E.g. Moroccan fleet description below.

Reviewer 3: Regarding the sentence "Reported landings for all CPCs in the North Atlantic (including dead discards) remain moderately high, and are presented in Table 3." the reviewer suggests deletion of "moderately" and comments the following: These values are in excess of 1.5 times the highest amount evaluated in the TAC projections by ICCAT 2019 (1100), which was associated with an 8% probability of rebuilding biomass and stopping overfishing by 2070. If 1100 is already too high, then I would not consider 1709 moderate.

Reviewer 3: Regarding the sentence "Due to the marketable nature of the species, the Spanish fleet has retained the vast majority of shortfin mako shark bycatch, and discards have been incidental since the beginning of this fishery (Mejuto et al. 2009)." the reviewer comments the following: I am unsure what is meant by 'incidental'. E.g. Negligible? Ongoing?

Reviewer 3: regarding the sentence "Due to at-vessel and post-release mortality, retention bans will not eliminate fishery mortality. However, because post-release survival can be as high as (and potentially greater than) 75% as discussed above, this retention ban may significantly reduce shortfin mako shark mortality in the Spanish pelagic longline fleet operating in the North Atlantic, and therefore overall mortality in this region." the reviewer suggests replacing "post-release survival can be as high as (and potentially greater than) 75% as discussed above" with "approximately 50% of catches would be expected to survive as discussed above," and comments the following: 75% is misleading in the sense that it does not account for at-vessel mortality. Under a landings prohibition, mortality would include animals dead-at-vessel and those that die post-release, which was discussed above as being 49.3%.

Reviewer 3: The reviewer suggests deletion of the sentence "Shortfin mako shark catch in U.S. pelagic longlines represented only 0.8% of total international longline catch of the species across the Atlantic Ocean in 2019 (NMFS 2021), and due to the poor reporting of other ICCAT Contracting Parties and Cooperating Non-Contracting Parties (CPCs), this percentage is likely significantly lower." The reviewer comments the following: This section and this paragraph are specific to the North Atlantic so it does not seem consistent to add a comparison with catches throughout the Atlantic (N+S). Would it be possible to move this text to the start of section 4.2B? Given that no specific management recommendation has been developed for the South, catches remain high (as expected).

Reviewer 3: Regarding catch in Canadian fleets, the reviewer comments the following: A more recent reference that gives landings by gear type up to 2014 would be: Showell, M.A., Fowler, G.M., Joyce, W., McMahon, M., Miri, C.M., and Simpson, M.R. 2017. Current Status and Threats to the North Atlantic Shortfin Mako Shark (Isurus oxyrinchus) Population in Atlantic Canada. DFO Can. Sci. Advis. Sec. Res. Doc. 2017/039. v + 45 p. If the time series to 2020 is of interest, it was published in May (which I recognize is outside of the time period considered for published research for this document): Bowlby, H.D., Coates, P.J., Joyce, W.N., and Simpson, M.R. 2022. Recovery potential assessment for the North Atlantic designatable unit of Shortfin Mako Shark (Isurus oxyrinchus). DFO Can. Sci. Advis. Sec. Res. Doc. 2022/025. v + 73 p.

Reviewer 3: Regarding the sentence "ICCAT SCRS catalogs (available at https://www.iccat.int/en/accesingdb.html) last updated in October 2020 indicate that the fleet continued to catch less than 100 t each year through 2019, with 62 t reported in 2019." The reviewer suggests adding "(except 109 t in 2017)" after "through 2019," and comments the following: It would be useful to update all data derived from the SCRS catalogue to include the most recently reported year. The 2020 information is in the current nominal catch tables. Also, other places reference SCRS 2021? Would it be possible to standardize the data and the most recent year referenced throughout?

Reviewer 3: The reviewer suggests adding the sentence "Total fishing mortality from all Canadian fleets was 55 t in 2018 and 64 t in 2019, with a requirement to release all live animals."

Reviewer 3: Regarding the sentence "In April 2020, Canada prohibited retention of shortfin mako sharks in Atlantic Canadian waters; however, as discussed above, the combination of at-vessel and post-release mortality form a substantial amount of total shortfin mako shark mortality in this fishery, estimated at 11 t each year by Campana et al. (2016)." the reviewer suggests replacing "form a substantial amount of total shortfin mako shark mortality in this fishery, estimated at 11 t each year by Campana et al. (2016)" with "still led to 20 t of shortfin mako mortality in 2020 (18 t from PLL)." The reviewer comments the following: Because it was essentially double the estimate, I thought it would be better to put in the actual value.

Reviewer 3: Regarding the paragraph beginning "Risk assessments have repeatedly found shortfin mako sharks to be at high risk of overexploitation by pelagic longline fisheries in the North Atlantic." the reviewer comments the following: This paragraph seems to fit better with information given at the start of section 4.2 (B), which is more global in scope. These risk assessments are not specific to the North Atlantic, but to the North and South combined.

Reviewer 3: Other minor editorial changes suggested.

(B) Overutilization for Commercial, Recreational, Scientific or Educational Purposes; Commercial and artisanal fisheries, South Atlantic Ocean

Reviewer 3: Regarding the sentence "The authors found that standardized catch rates of shortfin mako shark increased 8-fold in phase A (1979-1997), decreased by 55% in phase B (1998-2007), and increased 1.3-fold in phase C (2008-2011; Figure 13)." The reviewer suggests addition of "from a zero-truncated model" before "increased 8-fold in phase A" and "even

though nominal catch rates for all sharks combined were highest in phase B." The reviewer comments the following: I think this is a critical addition because the % of sets with zero catches was 90% in phase A, 77% in phase B and 21% in phase C.

Reviewer 3: Regarding the sentence "Spanish longline fleets in the South Atlantic reported shortfin mako shark catches of 1,049 t in 2017, 1,044 t in 2018, 1,090 t in 2019, and 799 t in 2020 (SCRS 2021)." the reviewer comments the following: was the only regulation change a requirement to release live animals? If 75% would be expected to be alive (25% dead at vessel), this suggests that they interacted with a huge amount of mako in 2020 (3196 t) or that at-vessel mortality is extremely high (~75%).

Reviewer 3: Regarding the sentence "In total, 96% of hooked shortfin mako sharks were retained, and of those discarded, 82% were dead (Jordaan et al. 2020)." the reviewer comments the following: Similar to above, the available data suggests at-vessel mortality is substantially higher from fleets in the South than North? Would other differences in the capture process be causing mortality – e.g. prolonged time on deck prior to release?

Reviewer 3: Regarding the sentence "Reported landings increased from 869 sharks in 2000 to 37,946 in 2015, although the earlier landings may have been under-reported (Petersen et al. 2009)." the reviewer suggests replacing "may have been" with "were likely" and comments the following: Relative to other information in the document, this increase is a bit shocking in magnitude: 44 times in 15 years.

Reviewer 3: Regarding the sentence "Combined with the continued high level of fishing effort, high catches, and low productivity, we conclude that overutilization of shortfin mako shark may be occurring in the South Atlantic Ocean, though there status is highly uncertainty due to poor data quality." the reviewer comments the following: It is not immediately apparent why data quality is considered to be so poor in the South Atlantic Ocean – there are few mentions of data issues beyond potential under-reporting in South Africa. Please see the previous comment under 'Regional Population Trends' on the inconsistency among input data and its effects on the 2017 ICCAT assessment. Reviewer also suggests minor editorial changes.

(B) Overutilization for Commercial, Recreational, Scientific or Educational Purposes; Commercial and artisanal fisheries, Western and Central Pacific Ocean

Reviewer 3: Regarding the sentence "However, when conducting an integrated ERA (incorporating the ERA, IUCN Red List index, annual body weight variation trend, and the inflection point of population growth curve), Liu et al. (2021a) found the species to be in the least risk group, possibly because the average body weight of the species in the western North Pacific hasn't experienced significant decline." the reviewer comments the following: Research in the North Atlantic also shows no changes over time in maturity or size at age (Natanson et al. 2020) despite very poor status. I feel it is possible that the size distribution mainly reflects the selectivity of the fleet (dome-shaped; targeting juveniles) and may not be overly indicative of status. Strong selectivity for particular sizes could increase stability in the sampled size distribution, particularly when the largest animals are rarely intercepted.

Reviewer 3: Regarding the sentence "Taiwan's catch of mako sharks (shortfin and longfin) in all longline fleets as reported in WCPFC data catalogs are high in the most recent six years of data:

1,216 t in 2015; 1,073 t in 2016; 1,088 t in 2017; 1,146 t in 2018; 1,680 t in 2019; and 1,665 t in 2020." the reviewer comments the following: Would it be possible to list them or indicate how many there are?

Reviewer 3: Regarding the sentence "From 1995-2006, shortfin mako sharks made up 2.9% of all observed shark catch in Hawaii-based PLL fisheries, with higher nominal CPUE rates in the shallow-set sector than the deep-set sector (Walsh et al. 2009)." the reviewer comments the following: This seems inconsistent with the statement above that: "Substantially higher numbers of shortfin mako sharks are caught in the deep-set sector than the shallow-set sector."
Is the deep-set PLL fishery much larger than the shallow-set one, or is the 3-year closure of the shallow-set fishery affecting this comparison?

Reviewer 3: Regarding the sentence "The New Zealand tuna longline fishery is composed of foreign-licensed vessels (which ceased fishing after the 1994-1995 season), foreign chartered vessels, and domestic vessels (Francis et al. 2001)." the reviewer comments the following: The Francis et al. paper states that mako and porbeagle catches weren't differentiated until 92-93. Rather than using the combined data, would it be possible to update this section? I only did a quick search, but there are NZ fisheries assessment reports with more recent information. E.g. https://docs.niwa.co.nz/library/public/FAR-2013-13.pdf I recognize that the more recent reports don't provide extrapolated totals as Francis et al. but only have observed numbers.

Reviewer 3: Regarding the sentence "In 2019, 1,659 shortfin mako sharks were caught in the Eastern Tuna and Billfish Fishery (7 were alive, 574 were dead , and 1,078 were released in unknown condition), 127 were caught in the Western Tuna and Billfish Fishery and released in unknown condition, 3 were caught in the Small Pelagic Fishery (one alive and two dead), and 92 were caught in the gillnet, hook, and trap sector (2 alive; 82 dead; 8 in unknown condition) (Patterson et al. 2020)." the reviewer comments the following: See previous comments that at-vessel mortality seems to be much higher in Southern fisheries than Northern.

Reviewer 3: The reviewer suggests adding the following sentence "A noteworthy exception are catches from Indonesia, recognized as the top shark fishing nation in the world." before "Interactions with shortfin mako shark commonly occur in pelagic longline fleets in this region."

Reviewer 3: I am a bit confused on the regional boundaries in the Pacific, which reflects my lack familiarity on where specific fleets are fishing and how to interpret catch totals relative to the stock assessment results. Please see the comments in the charge statement on standardizing regional information among sections.

Reviewer 3: Other minor editorial changes suggested.

(B) Overutilization for Commercial, Recreational, Scientific or Educational Purposes; Commercial and artisanal fisheries, Eastern Pacific Ocean

Reviewer 3: Regarding the sentence "Within the Mexican EEZ in the Pacific, shortfin mako sharks are taken in the artisanal fishery and the pelagic longline fishery, and were historically taken in the drift gillnet fishery until 2010 (Sosa-Nishizaki et al. 2017)." the reviewer comments the following: Throughout the document, it would be useful to identify which catch information

contributes to which stock assessment, and/or if any have been excluded. An artisianal fishery seems like it would not contribute to the North Pacific assessment?

Reviewer 3: Regarding the sentence “Despite being defined as small-scale, the magnitude of fishing effort and the high proportion of juvenile shortfin mako sharks landed may have a large impact on the population off of Peru.” the reviewer comments the following: Similar comment to above. In the summary, there needs to be an explicit statement on the level of mortality from small-scale fleets.

(B) Overutilization for Commercial, Recreational, Scientific or Educational Purposes; Commercial and artisanal fisheries, Indian Ocean

Reviewer 3: Regarding the sentence “Based on logbook data for this fishery over the period 2005-2018, Wu et al. (2021) found that the Taiwanese tuna longline fleet catches were largely made up of juvenile shortfin mako sharks: 97% of females and 74% of males were immature. This could be due to bait and gear selectivity favoring the catch of juveniles, or scarcity of adults in this ocean basin (Wu et al. 2021)” the reviewer comments the following: Summary information that comments on the life stages most commonly intercepted would be helpful to put catches by various fleets in context. Maybe as general information in the utilization section? When discussed throughout the document, most information suggests that juveniles are predominantly caught/targeted by various fleets (excluding those with minimum size regulations). As a more general comment: There would also be the potential to discuss why targeting juveniles can be problematic in the context of reducing survival at age and the proportion of the population that survives to maturity. The outcome of targeting juveniles is shown by the north Atlantic SS projections, in that many years are required before conservation actions influence the spawning population. The delay between identifying overutilization and addressing it is problematic and should be recognized as an issue that can limit the effectiveness of mitigation. By the time it becomes obvious there is a problem, many years are already required before it can begin to be solved.

Reviewer 3: Regarding the sentence “The number of active vessels increased since the beginning of the fishery in 1998 until 2006, rising to 17 vessels, and has decreased to as low as 3 vessels in recent years (Coelho et al. 2020).” the reviewer comments the following: Three vessels are now fishing the same amount of effort as 17 vessels previously did? What might this imply about the vessels themselves (higher efficiency?). It is striking to me that SMA catch has essentially an identical trend to effort, given the change in the number of vessels targeting them.

Reviewer 3: Regarding the sentence “In an updated ecological risk assessment of IOTC longline, gillnet, and purse seine fisheries, Murua et al. (2018) found that the most vulnerable species to the IOTC pelagic longline fleet is the shortfin mako shark based on its low productivity (λ=1.059) and high susceptibility (0.867) (Figure 18).” the reviewer comments the following: Table 4 of Murua et al. – 1.049?

Reviewer 3: The reviewer suggests adding the sentence “Post-capture mortality represents the proportion of captured animals that die as a result of interaction with the gear, calculated as the sum of landings and dead discards (Cortes et al. 2010)” after “The post-capture mortality rate in Indian Ocean purse seine fleets was reduced between the 2012 and 2018 assessments due to safe release best practices implemented by the European purse seine fleet beginning in 2014,

but is still quite high for shortfin mako sharks (approximately 55%) (Murua et al. 2018).” The reviewer comments the following: It would be helpful to readers to have a definition of this term (post-capture mortality) because it was not used in earlier text re: types of mortality.

Reviewer 3: Other minor editorial changes suggested.

(B) Overutilization for Commercial, Recreational, Scientific or Educational Purposes; Recreational fisheries

Reviewer 3: Similar to other areas – last year of available data is 2020.

Reviewer 3: Regarding the sentence “It is most likely that injury from foul hooking, which was significantly more common with J-hooks than with circle hooks, decreases post-release survival (French et al. 2015).” the reviewer suggests replacing “most likely” with “possible,” addition of “given that 2 of 3 mortalities were foul-hooked animals caught on J hooks” after “post-release survival” and comments the following: I don’t think that strong statements on covariates with mortality can be made from such a small sample size.

(B) Overutilization for Commercial, Recreational, Scientific or Educational Purposes; Trade

Reviewer 3: Regarding the sentence “While trade in shark fins appears to have decreased slightly since the early 2000s, the trade in shark meat has grown over the last decade or so (Dent and Clarke 2015)” the reviewer comments the following: Another interesting reference on increasing demand for shark meat in a key shark fishing nation: Karnad, D., Sutaria, D. and Jabado, R.W., 2020. Local drivers of declining shark fisheries in India. Ambio, 49(2), pp.616-627. As a more general comment, this paragraph focuses on trade among countries but doesn’t have information on domestic consumption?

Reviewer 3: Other minor editorial changes are suggested.

(C) Disease or Predation; Predation

Reviewer 3: From a theoretical perspective, predation is a component of natural mortality (M). The long life history and low M of shortfin mako suggests that predation rates are already low. For predation to be considered a threat, wouldn’t there have to be evidence that its magnitude has changed over time? E.g. a rapidly increasing predator population – such as the dynamics between Grey Seals and Northern Cod in the North Atlantic?

(D) Inadequacy of Existing Regulatory Mechanisms; U.S. Domestic Regulatory Mechanisms

Reviewer 3: Regarding the sentence “However, as a result of indications that the abundance of Atlantic sharks had declined, commercial quotas for pelagic sharks were reduced in 1997,” the reviewer comments the following: this text implies that recreational catches are not managed by quota but only by bag limits from the FMP. Is this correct?

Reviewer 3: What specifically is in place now or will be in 2022 in relation to Rec-21-09 in the North Atlantic? Ban on all landings (commercial and recreational), state and federal?

Reviewer 2: The reviewer suggests adding “fish” in the sentence “The FMP requires a Federal permit for all commercial HMS vessels that for HMS off of California, Oregon or Washington, or land HMS in these states” to correct a typo.

Reviewer 3: Regarding the sentence “Per the FMP, due to the stock’s vulnerability, possible importance of the U.S. West Coast EEZ as nursery habitat, and poorly known total catches and extent of the stock, the recommended harvest guideline for shortfin mako sharks is 150 t round weight,” the reviewer comments the following: Is there any data on what harvests are relative to the harvest guideline?

Reviewer 3: Regarding the sentence “The United States is a world leader in sustainable shark fisheries, with 12 of 16 sustainable shark fisheries globally (Ferretti et al. 2020),” the reviewer comments the following: I think that there needs to be details on the criteria used for sustainability. Is it also correct that this comparison would be specific to directed fisheries for sharks (e.g. spiny dogfish) rather than species affected by incidental catch? In the NMFS info given, there are no examples of a stock that is neither overfished nor with overfishing occurring (i.e. in the green quadrant of a Kobe plot). Which ones are the sustainable ones?

Reviewer 3: Regarding the sentence “As of 2017, of the 38 shark stocks or stock complexes in U.S. fisheries, 15 (39%) were listed as not subject to overfishing and 10 (26%) were listed as not overfished, 4 (11%) were listed as subject to overfishing and six (16%) were listed as overfished, and 19 (50%) had an unknown overfishing status and 22 (58%) had an unknown overfished status (NMFS 2018),” the reviewer comments the following: I find it difficult to interpret the outcome of a multispecies stock assessment relative to single-species overutilization. Even if a stock complex is not overfished, abundance of a single species can be low and status poor.

(D) Inadequacy of Existing Regulatory Mechanisms; International laws and agreements

Reviewer 3: Imports and exports of shark fins are banned under the 2019 revision to the Canadian Fisheries Act: subsection 32.1(1). This would regulate trade across boarders but does not specifically prohibit the sale of shark fins within Canada from Canadian fisheries. All sharks must be landed with fins naturally attached.

(D) Inadequacy of Existing Regulatory Mechanisms; Regional Fisheries Management Organizations

Reviewer 3: Regarding the sentence “Despite these requirements, reporting of shark catches has been very irregular and information on shark catch and bycatch is considered highly incomplete (Murua et al. 2018),” the reviewer comments the following: Please see the comments in the charge statement on the need to identify systematic bias – this statement implies that the stock assessment is extremely optimistic in terms of status and that true status can be fairly safely said to be much worse than currently thought.

EXTINCTION RISK ANALYSIS
Rangewide Extinction Risk Analysis, Foreseeable future – team discussion

Reviewer 2: Regarding the sentence “From this language, it is clear that the 50-year projection period was questioned on its scientific merit, and estimates over that time frame were only provided because the Commission requested them,” the reviewer comments “This is

misleading. It is true that metrics like probability of overfishing become useless so far into the future, and these were only produced because they are part of the Kobe protocol and the Commission asked for this time horizon. However, a long time horizon is necessary to see how long it would take for current changes in mortality to work their way through the age classes and rebuild the population. Long term projections are necessary to understand the dynamics of a long-lived species, not as a forecast.

Reviewer 2: The reviewer suggests adding "spawning" in front of the word "stock."

Rangewide Extinction Risk Analysis, Methods

Reviewer 3: Regarding the rankings used for the threats assessment, the reviewer comments the following: I am uncertain precisely how these levels relate to or were used relative to the levels given under the 'Overall Extinction Risk' section below. Why weren't the same ranking criteria used throughout?

Reviewer 3: Regarding the definition of low overall extinction risk (A species, subspecies, or DPS may be at low risk of extinction if it is not facing threats that result in declining trends in abundance, productivity, spatial structure, or diversity), the reviewer comments the following: This implies that fishing can never be considered a low risk threat, in that exploitation is known to result in declining trends and limited productivity for shortfin mako in the majority of their range.

Reviewer 3: Regarding the definition of moderate overall extinction risk (A species, subspecies, or DPS may be at moderate risk of extinction due to current and/or projected threats or declining trends in abundance, productivity, spatial structure, or diversity), the reviewer comments the following: This implies that global estimates of decline rate should be given more weight relative to absolute estimates of population size.

Reviewer 3: Regarding the definition of high overall extinction risk (A species, subspecies, or DPS with a high risk of extinction is at or near a level of abundance, productivity, spatial structure, and/or diversity that places its continued persistence in question), the reviewer comments the following: Information in the 'Demography' section suggests that shortfin mako are at a level of productivity that place its continued persistence in question (lambda < 1, indicating no capacity for abundance increase).

Rangewide Extinction Risk Analysis; Evaluation of Demographic Risks, abundance

Reviewer 3: Regarding the sentence "While recovery as defined by these criteria is likely to take decades, this does not indicate that the stock is at risk of becoming extirpated now or in the foreseeable future (25 years)," the reviewer comments the following: I agree that the information provided by these projections did not directly address the issue of extinction risk. I disagree that the probability of reaching reference points can be used to infer risk of extinction. It would be more appropriate to request the median abundance trajectory predicted by the SS model at the highest level of removals (1100 t) and determine if/when it approaches zero. Also, there needs to be recognition that actual levels of mortality were higher in recent years in relation to those evaluated in the projections. Fishing mortality in 2019 and 2020 was nearly double: 2019 – 1882 t and 2020 – 1709 t. These levels of removals do not introduce uncertainty, they demonstrate that the projections are too optimistic in terms of status. With the exception of 2025, all combined probabilities of reaching reference points remain below 10% at a TAC of 1100 t. For there to be such little indication of abundance increase (given the level of

uncertainty considered in the projections), trajectories needed to be declining, and in all likelihood, were strongly declining. Similar to the proportion reaching reference points, projections can be re-evaluated relative to the proportion reaching an extinction threshold over a given timeframe (e.g. Campana et al. 2008). Without this analysis, impressions relative to extinction risk remain conjecture. Campana et al. 2008. Status of basking sharks in Atlantic Canada. CSAS Research Document 2008/004 https://www.dfo-mpo.gc.ca/csas-sccs/publications/resdocs-docrech/2008/2008_004-eng.htm

Reviewer 3: Regarding the sentence "The 2017 stock assessment for shortfin mako sharks in the South Atlantic indicated a high degree of uncertainty due to poor data quality," the reviewer comments the following: Similar to comments above, data quality did not appear to be the issue, rather conflicts among different data sources leading to variability in assessment results from different modeling approaches.

Reviewer 3: Regarding the sentence "The authors conclude that despite high uncertainty, in recent years the South Atlantic stock may have been at, or already below, BMSY and fishing mortality is likely exceeding FMSY (ICCAT 2017)," the reviewer comments the following: Given this conclusion, it is telling that removals of shortfin mako in the South Atlantic remain high: ranging from 2308-3158 from 2016-2020 (last 5 years data were available). If productive potential for mako in the North and South Atlantic is the same (i.e. if the population is truly panmictic), the results from the North Atlantic suggest that fishing at these levels are unsustainable, particularly if the stock is already overfished and with overfishing occurring. However, I support the authors general reluctance to making strong statements on status when the original assessment team was not willing to.

Reviewer 3: Regarding the sentence "Reported landings represent a substantial underestimate of mortality resulting from fisheries interactions, and therefore there is some level of uncertainty in available stock assessments and abundance indices, particularly in the South Atlantic and Indian Oceans," the reviewer suggests replacing "there is some level of uncertainty in" with "status determination from," addition of "are too optimistic" after "available stock assessments," and addition of "may not be indicative of regional abundance" before "particularly in the South Atlantic and Indian Oceans." The reviewer comments the following: Please see the charge statement on the need to identify systematic bias throughout the document; although I recognize that the assessment team took bias into account in their risk evaluation.

Reviewer 3: Regarding the sentence "Overall, there is no indication that global abundance has declined so low that reproductive success of the species has declined or inbreeding has resulted, nor is there evidence of other depensatory processes associated with small populations," the reviewer comments the following: This statement isn't well-supported by the information provided in the status report. Please refer to previous comments on the need to characterize uncertainty in population size estimates, in addition to comments in the charge statement on how the small population paradigm relates to shortfin mako.

Rangewide Extinction Risk Analysis; Evaluation of Demographic Risks, Productivity

Reviewer 3: Please refer to comments in the charge statement related to situations when mortality exceeds reproductive capacity. This statement presupposes recovery (through population increase) is possible, while with lambda < 1, it is not.

Rangewide Extinction Risk Analysis; Evaluation of Demographic Risks, Spatial distribution

Reviewer 2: Regarding the sentence “Connectivity among ocean basins has been demonstrated by several genetic studies,” the reviewer comments: I would say: Studies have not found structure, not that they have demonstrated connectivity. These studies presumably used lack of structure as a null hypothesis.

Reviewer 3: Please see previous comments/edits related to this text as well as general comments in the charge statement related to Distinct Population Segments.

Rangewide Extinction Risk Analysis; Evaluation of Demographic Risks, Diversity

Reviewer 3: Regarding the sentence “We found no evidence that gene flow, migration, or dispersal has been reduced,” the reviewer comments the following: This implies that reductions in these rates have been studied for shortfin mako. To my knowledge, there has been no explicit evaluation of changes in rates of gene flow, migration or dispersal among oceans, beyond documenting that they occur.

Rangewide Extinction Risk Analysis; Evaluation of ESA Section 4(a)(1) Factors; Overutilization

Reviewer 3: While it is true that recreational fishermen cannot access the high seas, this statement also presupposes that shortfin mako in these regions also do not disperse into coastal environments and thus remain permanently unavailable to recreational fishing. The available tagging data does not support this interpretation, as coastal movements appear relatively common. Animals that are tagged coastally regularly moved into international waters, suggesting that the entire population is at least partially vulnerable to fisheries occurring in restricted components of their range as they move between coastal and offshore habitats. It is my understanding from the information provided that recreational landings represented the last source of directed fishing mortality on shortfin mako in the North Atlantic. This mortality is concentrated on adults (due to minimum size restrictions) and thus might be expected to affect reproductive potential to a larger degree than bycatch mortality from commercial fisheries, which predominantly catch juveniles. The overall magnitude of mortality from recreational catch is similar to or above the level coming from pelagic longline in the US in 2018 and 2019. The results from the 2017 and 2019 stock assessments for the North Atlantic encompass the contribution from all sources of fishing mortality (commercial and recreational). Thus it seems inconsistent to have concluded that the stock assessment and projections were concerning in relation to commercial fisheries (text above), but not in relation to recreational. Mortality is cumulative and any source contributes to our poor perception of status relative to mako in the North Atlantic. Please also see the previous comment on the need to evaluate the predicted timeseries from the SS projections at various levels of removals to appropriately gauge if continued mortality results in high extinction risk over the foreseeable future.

Reviewer 3: Regarding the sentence “Considering the recent declines in the fin trade and increases in the meat trade, the Team generally agreed that the preference for shortfin mako shark meat (in addition to fins) presents a concern for overexploitation of the species,” the reviewer comments the following: Would it be fair to say that the concern would be relative to both international trade as well as national markets? By this I mean, shortfin mako are still a sought after species in national fish markets (where catches by a particular country are sold locally), in addition to international.

Rangewide Extinction Risk Analysis; Evaluation of ESA Section 4(a)(1) Factors; Indaequacy of existing regulatory mechanisms

Reviewer 3: The US was able to achieve a much greater reduction of shortfin mako landings than Canada when the regulations permitted landings of animals dead at vessel. It was noteworthy to me that electronic monitoring appeared much more effective than at-sea observation in ensuring compliance with the requirement to discard live animals from pelagic longline fleets. My question relates to the overall goal of regulations and I will use the North Atlantic as an example. Is the goal of regulations to reduce mortality by the maximum possible amount? If yes, it is difficult to make an argument that allowing directed mortality from the recreational fleet is adequate, given that the potential for live release is very high (~90%; French et al. 2015). However, if the goal of the regulations is to ensure that the level of mortality occurring in US waters is below what might threaten the population (i.e. 250 t), I would argue it is adequate. Even if fishing mortality in the US was brought down to zero, collective mortality could still remain higher than acceptable levels throughout the North Atlantic. This would render regulatory actions in the US ineffective but it would be very difficult to argue that they were inadequate. Very large reductions in total mortality from US fleets in the North Atlantic have been achieved in recent years from changes implemented in 2018.

Reviewer 3: Regarding the sentence "This measure enters into force mid-2022 and its effects can be assessed will be will not be fully assessed until 2024," the reviewer suggests addition of "the first year that" before "its effects" and replacing "will not be fully assessed until" with "can be assessed will be." The reviewer comments the following: One year of data is not sufficient for robust assessment, as is made clear in the next sentence.

Reviewer 3: The reviewer suggests adding "In the North Atlantic when data limitations were addressed, assessed status reversed from 2012, indicating overfishing and that the stock was overfished with high probability in 2019." The reviewer comments the following: This is only a suggestion for discussion. Would it be worth making explicit statements on the likely outcomes of improving data quality?

Reviewer 3: Regarding the sentence "The Team was split on how this factor contributes to the extinction risk of the species, with just over half of the Team concluding that the inadequacy of existing regulatory mechanisms will contribute significantly to the species' risk of extinction in the foreseeable future, but does not in itself constitute a danger of extinction currently," the reviewer comments the following: Is trade not considered a danger because in and of itself, it does not directly cause mortality? I.e. animals are killed in fisheries and then traded, not killed by trade. This is a more general comment, but I don't understand how a factor can contribute significantly to extinction risk in the future if it doesn't also significantly contribute to extinction risk currently, unless future mortality is expected to be greater. Relative to trade, all available information suggests that trade restrictions and finning/landing prohibitions are getting stronger, not weaker. Why wouldn't the risk be the highest now (under current regulations) as opposed to in the future (likely under stricter regulations)?

Rangewide Extinction Risk Analysis; Evaluation of Overall Extinction Risk

Reviewer 3: Regarding the sentence "However, the Team did not conclude that this region is at risk of extirpation based on available projections carried out by ICCAT's SCRS," the reviewer comments the following: Please see my previous comment about the projections and how an evaluation of future abundance trends would be necessary to support this statement.

Reviewer 3: The reviewer suggests adding "and very likely underestimated" at the end of the sentence "In the Indian Ocean, preliminary stock assessments indicate that the shortfin mako

shark population is experiencing overfishing, although compliance with reporting requirements is quite low in this region, so the Team felt that the extent of the species' decline in this region is highly uncertain."

Reviewer 3: With regard to discussion of increasing population status in the Pacific Ocean, the reviewer comments the following: Please see previous comments on the level of uncertainty associated with terminal year biomass/abundance predictions and the need to consider how it would affect this assessment.

Reviewer 3: Regarding the sentence "The Team also concluded that the shortfin mako shark's high genetic and ecological diversity, connectivity between populations, and wide spatial distribution reduce the species' extinction risk by providing resilience in the face of stochastic events and threats concentrated in certain regions," the reviewer comments the following: Please see comments related to the rescue effect in the charge statement.

Significant Portion of its Range Analysis

Reviewer 3: Regarding the sentence "The South Atlantic population may be both overfished and experiencing overfishing and has highly uncertain data," the reviewer comments the following: Uncertain or that different types of data used within various modeling approaches are inconsistent? Possible reasons for inconsistency are well-reviewed in Maunder et al. 2006, and the inconsistency doesn't necessarily arise due to uncertainty but to other characteristics of indices. Maunder, M.N., Sibert, J.R., Fonteneau, A., Hampton, J., Kleiber, P. and Harley, S.J., 2006. Interpreting catch per unit effort data to assess the status of individual stocks and communities. Ices Journal of marine science, 63(8), pp.1373-1385.

Reviewer 3: Regarding the sentence "Despite its continuing declining trend, the Team did not feel that the rate of decline in the foreseeable future would be great enough to put the species in this portion at high risk of extinction in the foreseeable future," the reviewer comments the following: There has been no evaluation of the rate of decline in future even though it could be easily derived from the SS projections for the North Atlantic. Please see previous comments.

Distinct Population Segments Analysis

Reviewer 2: Regarding the second significance criterion (2) Evidence that loss of the population segment would result in a significant gap in the range of the taxon), reviewer comments "This could apply to sf mako."

Reviewer 2: Regarding the sentence "This finding does not support the existence of discrete population segments of shortfin mako sharks," the reviewer comments "What about the substantial loss of range criteria?"

Appendix 3

Reviewer 3: Regarding the entry in the table for Canada, the reviewer comments the following: I recognize that the information in this table is derived from a 2017 reference. However, this is an older regulation that was replaced with a licence requirement in 2018 that fins must be naturally attached when landed, similar to regulations in the US.

General Comments (not associated with order of names as they appear above)**:**

Reviewer 1: Regarding population structure. The assessment concludes that there is a single global population of shortfin mako sharks, while acknowledging that there is some evidence for

weak structuring at the ocean basin scale. My concern here is that this conclusion is based almost entirely on genetic evidence. While genetics is often used for such purposes, it does have limitations. These include that it only demonstrates structure on evolutionary timescales. Given that microsat and mtDNA methods were used these represent relatively long evolutionary time scales, and as such do not match to the time scale used in the extinction risk assessment (25 years). There are many other approaches to investigating population structure (e.g. tagging, telemetry, life history, parasites, microchemistry, stable isotopes). Many of these approaches inform at a much shorter time scale, and would have been more appropriate to the extinction risk time scale used. Some reference is made to tagging and tracking data. However, this is quite brief compared to the genetic results, and suggests some structuring. For example, there is no detail provided on the numbers of tagged/tracked animals, and how these results relate to specific population structuring questions. At the very least the tagging and tracking data appear to support separation of populations between ocean basins. The application of a wide range of methods is often best because they inform at a range of time scales. Other genetic methods, such as single nucleotide polymorphisms (SNPs) can also inform on shorter time scales. Based on this I think that the conclusion that the assessment should be carried out at the global scale is poorly supported. Again, given the uncertainty about population structure at the extinction risk time scale a precautionary approach that assumed structure would have been appropriate. I also think that the conclusion that there are no Distinct Population Segments could be challenged given that the tagging and tracking data do not show movements between ocean basins.

The other challenge with reviewing the assessment is that there is a lot of information included in text form, often in long and detailed paragraphs. The reader would have been greatly assisted by the use of more summary tables and/or figures. This would have made getting a broader picture of the information much easier. There was also a lot of repetition of information between sections, which made the assessment document much longer than it needed to be.