Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author

MAJOR COMMENTS

The manuscript “Long-term shifts in Atlantic Cod and Yellowtail flounder distributions on Georges bank” analysed an impressive set of biological data collected across several decades in different seasons to analyse the distribution of two commercially important fish species (Gadus morhua and Limanda ferruginea) using a highly advance combination of geostatistics and SDMS tools. The paper shows interesting results on the shift of these fishes to the north, a result previously reported in several other works but still interesting scientifically and from management purposes. I find specially interesting the results related with how the stock is moving from EEUU to Canada, because of its important management implications and I think the manuscript is potentially suitable for publication in the ICES journal of marine science. However, the manuscript has important flaws which make it unsuitable for publication in its current form.

The main problem is the lack of appropriate environmental covariates used to model the distribution of the species and the final conclusions extracted from this decision. The authors fail to find near bottom temperature values for the study area in the survey’s months. These values are available for other regions such as Europe and globally (see <https://marine.copernicus.eu/>) and I find difficult to understand that these data are not available for the coast of two of the most scientifically advance countries in the world (see for instance Richaud et al, 2016). Spite of this limitation the authors still manage to model the distribution of the species, and because of that the paper is still interesting, but what they can not do is to extract the conclusion that random fields can replace relevant environmental covariates when they practically do not use any.

**Snarky initial response: Include covariates from BNAM, SST/BT, SSS/BS. We have the data so it is entirely doable just going to take a month to get it all up and running, was really hoping nobody would get hung up on that, but in the end it is a valid point.**

I find especially wrong the fact that they consider expensive to obtain data for these environmental variables (probably available in public repositories) but they ignore how expensive is to have three fishery surveys sampling the Georges bank across decades and in 3 different seasons, which is the only reason why, when they use only geostatistics, they still can accurately predict fish distribution. In a more normal situation, with a lower density of samples for a much bigger area the approach used for the authors will not be able to do a proper prediction whereas a SDM based on relevant covariates will. In that sense, the authors must modify the last part of its discussion, acknowledging the limitations of this work in terms of relevant covariates used to model the distribution of the fish species but clarifying that they were able to overcome this limitation because the high sampling effort present in the area and an appropriate use of geostatistical tools when needed.

**Snarky initial response: I’d argue that if you don’t have good sampling coverage how are you going where anything is in the first place to sort out what the relationship with the environment is and how would you know if that prediction is actually accurate unless you had a good sampling program? Mind you it is a good point that these surveys are super expensive.**

I also find problematic the fact that they model (if I understand correctly) only presence/absence and not abundance of these species, which probably will be much more relevant for a stock assessment perspective. When they talk about what they are modelling the authors say: “For all analyses, the response variable was the probability of the survey detecting the stock of interest (Occurrence Probability, OPit , where i is the individual observation at time t)”. I don’t really know how a stock can be detected or what a stock is in this context. I guess they mean the probability of find at least one individual of cod or yellowtail flounder. This has to be clarify and in general all the manuscript need to be revised trying to simplify concepts, avoiding, when possible, artificially complex language to reach a more general audience. More than this, authors really need to properly justify why they model only presence/absence and not abundance since this decision highly reduce the relevancy of the work.

**Snarky Response: Will clarify text, that was a sloppy ‘replace all’ from species to stock which confused him, I think individual is the better word there. I hadn’t really thought of it before this, but my main argument for not including biomass is that it doesn’t work at the small scales we are interested in (i.e. the cod/yellowtail closure cells), but that argument doesn’t really hold for this paper since we are now looking at Georges Bank overall. I’m feeling two options right now, there’s no reason we couldn’t model biomass in this model, or we could add a biomass index as a covariate in the model. The nice bit about adding Biomass as a response is that this would make it totally different from our Res Doc so we could hammer that out the door without having to worry about the impact on this paper, but making this change would probably end up delaying this significantly.**

Finally, authors need to pay attention to the last papers on standards for SDMs (e.g. Araujo et al., 2020) since they ignore some key aspects of the field:

• Catchability of different gears and geolocation error. Much more data about the different surveys need to be provide in order to allow merging them in the models. At least data about the specific type of gear and the duration and speed of the hauls need to be provided. In that sense, the resolution of the environmental layers finally used must be provided (now authors provide different resolutions for each layer but it is not clear what resolution was finally used) and adapted if needed to the geolocation error of the hauls (half of its length if they are using the middle point of the haul, something they also must specify). Even if the gears of all surveys are the same, to include variable as a factor for country/boat is highly advisable before data from different surveys can be merge (see Moriarty et al, 2020).

**Snarky initial response: We don’t combine the surveys for many of these reasons, will go and clarify in the methods that each survey is an independent model. Not sure how saying the resolution of the layer isn’t clear that this is the resolution used, again will need to slightly clarify in the text.**

• Only include ecologically relevant covariates and explain its relevancy. Recently several works have highlighted the need to avoid the use of random covariates to explain species distribution since this highly increase the chances of artificial correlations (e.g. Fourcade et al., 2018). In that sense, authors include in the study several variables that could be redundant ( 3 types of benthic stress) or whit a no clear relationship with the ecology of the species (e.g. Seasonal range of benthic salinity, Benthic silicate). Please, include only variables which a clear ecological link with the species ecology or which are proxy to these links (e.g. depth).

**Snarky initial response: Totally agree in principle, I will clarify that we removed a number of covariates before this stage and we can happily remove more of these covariates. Given that none of these ‘marginal’ covariates had any impact and the only ones that were used in the analysis makes biological sense I am confused why they’d bother with this complaint, we went with the throw everything at it and only biologically sensible covariates came out.**

• Evaluation of the models. SDMs literature have a good stablish set of methods to evaluate the models which I have miss in this work. I am not saying that methods apply are wrong, but I do think that a more standard approach to evaluate the models will do the study more comparable with similar works present in the literature. In that sense, to use a spatial block cross-validation and some typical metrics such as AUC, TSS, Kappa, sensitivity or specificity is highly advisable and could complete the analysis already made. Also, it will be interesting to see if more than the final years, the models is equally successfully when predicting other group of years, for instance from the seventies when environmental conditions where probably quite different to present conditions.

**Snarky initial response: I had been thinking about adding in AUC metric for a while to make SDM crowd happy, haven’t ever done it before but will look into it. I think it should be pretty obvious that using the 2016 data to predict in the 1970’s would be a terrible idea, point was that the model works ok for a few years given the shifting random fields, will modify to point out that long-term prediction would not be a good idea given changes in random field over time.**

• Justify threshold selection based on known methods. At the moment authors apply an arbitrary value (0.75) which in my opinion they don´t justify sufficiently. Although they provide an interactive tool which allow to try others, several conclusions and metrics of this work rely on this decision and probably will change with a different value. It will be good if the authors justify better this selection or at least explain how much some metrics will change with a different election. I will advise to explore some of the methods to threshold selection available in the SDMs literature (e.g. prevalence, value which maximize Kappa, etc).

**Snarky initial response: Will expand on this, I think we mention that the general patterns are unchanged but the total areas will expand/contract based on the number used. For the follow up paper for this I started using a ‘mean’ probability and I’m thinking to avoid this argument that including this mean probability into the paper would be useful to show that the general trends hold up with either metric.**

• Maps. Spite of being a paper about SDMs there is not maps in the manuscript and all the predictions have been send to supplementary material. I find this unusual and unappropriated. I will highly recommend to the authors to make a higher effort and provide at least some maps showing the main result of this work (a north shift of the distribution of both species). A possibility will be to add the core areas from all year to generate an index of persistence for each species (see for instance González-Irusta and Wright 2017) or similar.

**Snarky initial response: That last point is an interesting idea, something like taking the mean value at a point across all the ‘eras’ to identify areas where a stock is typically found vs a spot where it comes and goes (standard deviation also would be useful idea here). I like that idea ☺**

Araújo, M. B., Anderson, R. P., Barbosa, A. M., Beale, C. M., Dormann, C. F., Early, R., ... & Rahbek, C. (2019). Standards for distribution models in biodiversity assessments. Science advances, 5(1), eaat4858.

Fourcade, Y., Besnard, A. G., & Secondi, J. (2018). Paintings predict the distribution of species, or the challenge of selecting environmental predictors and evaluation statistics. Global Ecology and Biogeography, 27(2), 245-256.

González-Irusta, J. M., & Wright, P. J. (2017). Spawning grounds of whiting (Merlangius merlangus). Fisheries Research, 195, 141-151.

Moriarty, M., Sethi, S. A., Pedreschi, D., Smeltz, T. S., McGonigle, C., Harris, B. P., ... & Greenstreet, S. P. (2020). Combining fisheries surveys to inform marine species distribution modelling. ICES Journal of Marine Science, 77(2), 539-552.

Richaud, B., Kwon, Y. O., Joyce, T. M., Fratantoni, P. S., & Lentz, S. J. (2016). Surface and bottom temperature and salinity climatology along the continental shelf off the Canadian and US East Coasts. Continental Shelf Research, 124, 165-181.

MINOR COMMENTS

Abstract

Page 1, line 22: Simplified models using the random field for prediction performed similarly to models that included environmental covariates Remove this sentence from the abstract.

**Snarky initial response: Let’s see what the models with the BNAM data says first.**

Introduction

General comment: The introduction is too short, and I miss some important aspects. It is especially relevant the lack of information on the species ecology and its effect on species distribution, including mentions to previous works which have analyzed the distribution of these species in the context of climate change or using similar techniques (e.g. Drinkwater, 2005; González-Irusta and Wright, 2016; Morato et al., 2020). Some introduction to the statistical techniques applied to model the species distribution (R-INLA, Spatial Point Process? Bayesian GLMs?) is also necessary.

**Snarky initial response: Will expand this to capture these papers and put back in the INLA bit.**

Specific comment

Page 2, lines 454-48. I find this definition imprecise. I will suggest replacing that definition for this other (more recent and precise) from Elith and Leatwick (2009) “We define an SDM as a model that relates species distribution data (occurrence or abundance at known locations) with information on the environmental and/or spatial characteristics of those locations”.

**Snarky initial response: To do**

Page 2, line 55/Page 3 lines 4 and 5. It is true that SDMs usually did not consider temporal variations but is also true that some do it (even for the same species studied in this work) without necessary apply very complex mathematical models (e.g. Loot et al., 2011; Lelievre et al., 2014; González-Irusta and Wright, 2016, Asjes et al, 2016) and obtaining very good results. I suggest rewriting the paragraph to provide a more accurate state of the art of this field.

**Snarky initial response: To do**

Page 3, line 21. This could be just me, but I don’t like sections in the introduction. I suggest to remove.

**Snarky initial response: To do**

Page 3, lines 25-30. This sentence seems to suggest that with the introduction of the EEZ unsustainable fisheries finished but I don’t think this was the case according to the evolution of the stock. Please, provide evidence of a sustainable fishery after the start of the EEZ (I mean specific references) or re-write the sentence.

**Snarky initial response: To do**

Page 3, line 49. I am not an expert on Bayesian statistics or R-INLA but my understanding is that this technique is not a model itself but a tool to fit specially mathematical complex models in a much more efficient computational way. In this sense, I think the model applied to predict the distribution of the two studied species should be mentioned here as well. A previous paragraph on R-INLA and the model used to fit the species distribution will be also very welcome.

**Snarky initial response To do**

Methods

Page 4, line 29. Much more information about the surveys is needed. At least, specific sampling gears used in each survey and type of boat. If the sampling gear is different, authors need to justify that these differences are not affecting the results for catchability differences between them. To include gear and/or research vessel as a variable is an option authors should explore. Furthermore, trawl speed and duration of each haul in each survey (winter, fall, spring) also should be provide as well as how was decided the location of each haul (middle point?) used to extract the covariate values. Finally, this will be a good place to explain if it was abundance data of each species available and if there was, why was not used in the model.

**Snarky initial response: To do**

Page 4, line 44. You have to explain why you choose one variable instead of other (ecological relevancy? Higher correlation with the response variable?) when you reduce them from the initial set. I don´t think the idea of group variables using PCA is a good idea specially when after all you did not use this very much in the work, I will suggest removing this. Also, see general comment on ecological relevancy of the initial set.

**Snarky initial response: To do**

Page 5, line 15. What is a SPDEs? Always use the whole term first time you introduce and acronym. If I

understand your approach properly you are modelling the probability of presence of both species using Bernoulli GLMs (not sure what is this, please explain) and some kind of random spatial field that I am not sure how you compute (do you use lon/lat?). I am not a mathematician and I have to admit that these techniques are more advance that the techniques I usually apply but it is also true that if I cannot understand this, many of the ICES journal readers won´t understand it neither. Please, make a much bigger effort to explain this better.

**Snarky initial response: To do**

Page 6, line 37. WAIC, CPO, DIC, please specify acronyms meaning and maybe add some references explaining them.

**Snarky initial response: To do**

Results

Page 10, line 37. Do you mean the distribution of the variables?

**Snarky initial response: To do**

Page 11, line 44. For both species. You are not predicting stock distribution you are predicting species distribution at least I am missing something.

**Snarky initial response: To do**

Figure 1. Please show the distribution of the samples in each survey separately (spring, fall, winter) with different type of dots if surveys of two different countries happen in the same moment. Please, add legend show we can see the depth in the studied area.

**Snarky initial response: To do**

Figure 2. I will suggest sending this Figure to supplementary Figure 3. Please, add bathymetry of the GB under the dots so readers can see where these dots are. Also, maybe use dots with a color gradient instead of labels to show periods.

**Snarky initial response: To do**

Discussion

Page 12 lines 27-43. This first paragraph is more a continuation of the results than a discussion of them, please modify including a discussion of the results and not just a description of them.

**Snarky initial response: To do**

Page 12 lines 46-52. I don´t think you can still call them SDMs if they don´t include covariates. Here I will say something like:

Spite the lack of relevant covariates available in this work, our geostatistical model (without environmental covariables) still was able to identify regions of consistently high and low probability of occurrence, quantify changes in the size of a core area over time and between seasons (surveys), quantify how rapidly distributional shifts occur, and provide short term forecasts of the spatial distributions in future years, probably because the very high sampling density present in the study area.

**Snarky initial response: To do**

Page 13, lines 3-10. Again, please discuss your results, for instance mentiones previous works which predicted this shift because of climate change and trying to connect your results with Cod ecology.

**Snarky initial response: To do**

Page 13, line 34. Stocks doesn´t have life history, species does.

**Snarky initial response: To do**

Page 13, line 36. ICJ line?

**Snarky initial response: To do**

Page 14, lines 42-46. I think this a very strong claim based in a not publish paper which doesn´t make too much sense to me. Most of the spatial structure of marine species is a consequence of the spatial structure of the environmental variables which drive its distribution. When using the right covariates, SDMs are very good predicting species distribution and able to extrapolate into areas with not data at all or in periods into the future (even a far future), something pure geostatic model can not do. Please, remove the sentence.

**Snarky initial response: To do**

Page 14, lines 46-49. The problem is that to collect the data which fit these models are much more expensive that collect environmental variables which can be solves just by adding a CTD to the net. These models are a very powerful tool (no doubt about it) and can help in many cases but they are not a replacement of good SDMs based on relevant covariates. Please, re-write.

**Snarky initial response: To do**

Page 14-15-lines 57-3. The most likely explanation is climate change. These shifts have been already predicted by many other works for this and other species in this and other regions (e.g. Drinkwater, 2005, Morato et al., 2020). Please, add some info. on climate change effects on fish distributions and specifically cod and yellowtail flounder (if available). Also, please acknowledge that one of the main caveat of this approach is that although your model can predict fish distribution, because the lack of relevant covariates you can not explain this distribution and therefore, the causes behind the observed shift can not be deduce from your models.

**Snarky initial response: To do**

• Loots, C., Vaz, S., Planque, B., Koubbi, P., 2011. Understanding what controls the spawning distribution of North Sea whiting (Merlangius merlangus) using a multimodel ap- proach. Fish. Oceanogr. 20, 18–31.

• Lelievre, S., Vaz, S., Martin, C. S., & Loots, C. (2014). Delineating recurrent fish spawning habitats in the North Sea. Journal of Sea Research, 91, 1-14.

• González-Irusta, J. M., & Wright, P. J. (2016). Spawning grounds of Atlantic cod (Gadus morhua) in the North Sea. ICES Journal of Marine Science, 73(2), 304-315.

• González-Irusta, J. M., & Wright, P. J. (2017). Spawning grounds of whiting (Merlangius merlangus). Fisheries Research, 195, 141-151.

• Asjes, A., González-Irusta, J. M., & Wright, P. J. (2016). Age-related and seasonal changes in haddock Melanogrammus aeglefinus distribution: Implications for spatial management. Marine Ecology Progress Series, 553, 203-217.

• Drinkwater, K. F. (2005). The response of Atlantic cod (Gadus morhua) to future climate change. Ices journal of marine science, 62(7), 1327-1337.

• Morato, T., González‐Irusta, J. M., Dominguez‐Carrió, C., Wei, C. L., Davies, A., Sweetman, A. K., ... & Carreiro‐Silva, M. (2020). Climate‐induced changes in the suitable habitat of cold‐water corals and commercially important deep‐sea fishes in the North Atlantic. Global change biology, 26(4), 2181-2202.

Reviewer: 2

Comments to the Author

Thank you for the opportunity to review the paper “Long-term shifts in Atlantic Cod and Yellowtail Flounder distributions on Georges Bank”, by David M. Keith, Jessica A. Sameoto, Freya M. Keyser, and Irene Andrushchenko. The thesis of this paper is that the core areas of distribution and associated occurrence probabilities of two important groundfish stocks have changed over the time period covered by groundfish surveys conducted by the Canadian Department of Fisheries and Oceans (DFO) and the United States National Marine Fisheries Service (NMFS) in the greater Georges Bank area. The paper uses the calculated resource distributions and a wide array (n=22) of candidate environmental correlates (sea surface temperature, depth, sediment type, chlorophyll density etc.) to evaluate a suite of potential predictor variables correlated with the distribution metrics for the stocks. One of the conclusions of the paper is that the quota sharing arrangement between the USA and Canada in this region should be re-evaluated in light of changing resource distributions.

The paper makes use of three research vessel trawl survey time series conducted by the two countries. These include the USA spring and autumn series and the Canadian winter series. Each of these time series have different time spans (the longest being for the USA autumn beginning in 1963). Environmental correlates (primarily temperature and depth) are calculated from data series not associated specifically with the trawl catches of the target species and other data sets (chlorophyll and sediment type) that are available. The Integrated Nested Laplace Approximation (INLA) approach is used to model species distributions. Statistics of core area (where the occurrence probabilities were ≥75%) were calculated over several averaging periods.

Overall, the paper concludes that distributions have shifted over the time period since 1987 with larger proportions of yellowtail flounder and Atlantic cod now residing in the Canadian zone. The paper further speculates (without specific information) that even greater proportions of the cod management stock may exist in Canada because distributions may have shifted out of the sampling domain (trawl survey strata) northeastward to the Northeast Channel between Georges Bank and Browns Bank.

Comments:

The overall findings of the paper are hardly new. For example, Gavaris and Murawski (2003) found similar trends of a Northeastward shift beginning in the early 1990s for Georges Bank yellowtail flounder and Atlantic cod, and additionally included the haddock resource in the region (which was curiously missing from these analyses). The earlier analyses did not correlate the distributions with environmental co-variates and that part of the present paper is new. In the reduced model set, SST, water depth and sediment type were found to significantly (but marginally) improve model performance. However, as noted these were “static” co-variables and did not reflect the actual environmental parameters associated with fish captures. Although there is some debate regarding the use of broader environmental data sets vs. co-variates, this approach would have provided some important insights as well. For example, the paper speculates about vertical mixing and the use of the SST proxy. For bottom tending fishes such as these, examining actual bottom temperatures and their correlations with SST for the positive captures would have been a useful set of analyses. Since the autumn survey extends all the way back to 1963 (the mid-1960s were very cold), a more thorough examinations of the drivers of resource shift, and particularly temperature may be better explored using the expanded trawl survey series.

**Snarky initial response: Adding in the BNAM data should resolve most of this, will see if I have access to the data from the 1960s**

Importantly, the paper also does not include one of perhaps the most important determinants of spatial distribution: the overall abundance of species. The well known “Basin Effect” postulates that species ranges increase as the overall abundance of the population increases and the species is found more widely distributed (MacCall 1990). It would have been an important null hypothesis to test that abundance itself is the determinant of core area size and the proportion of positive observations (generally these are well correlated. I am not sure if the abundance and temperature signals are convolved (they both seem to trend in opposite directions), but statistical models de-trended for autocorrelation may help.

**Snarky initial response: To add biomass as a covariate or as a response variable. I mean overall the point is really not that relevant to me, I thought it was implicit in this that if the range was contracting this was a bad thing and is related in declines in biomass, not entirely sure that adding it as a covariate really adds anything other than the obvious.**

At lines 333-340, it is speculated that the cod stock may have shifted outside the sampling domain of the surveys, and cites two references for this speculation Andrushchenko et al., (2018, 2019), neither of which actually address this point nor “suggestion that the observed shift in the distribution of Atlantic Cod outside of the survey domain”. This argument that the stock may have shifted to the Northeast slope is further bootstrapped into a recommendation to revisit the sharing agreement: At lines 340-343 the paper notes “In addition, because the management of this stock is shared between Canada and the U.S., the observed shift in the core distribution to Canadian waters suggests that shared management policies, such as quota sharing agreements between the two jurisdictions, may require regular review (e.g. TMGC, 2002).” This sentence seems odd because the initial sharing agreement precisely anticipates that changes in range distributions, appropriately weighted for survey variability from survey to survey should result in changing relative quota allocations. What in the current paper is inconsistent with the original text of the sharing agreement? If in fact the cod stock shifted out of domain, some kind of research project to address this hypothesis is the preferred method to address such an important point.

**Snarky initial response: Irene you mentions this in those, I mean it is one sentence but it is mentioned. Anywho, I got someone from US on TRAC all worked up eh. Given I gotta go back, I’m going to look to see what data is available outside the ‘prediction domain’ I used, again that was a relic of the initial plan for the analysis of looking at the cod/yellowtail closures. Since I have to go back and re-run stuff I’d like to loosen by definition of GB (or at least have the data to talk about shifts off the bank).**

Last, sentences 413-417 speculate about risks of species extirpation on the USA side of the Bank and more informing decision making. This paper is not the forum for calculating or commenting upon extinction risk. Other modeling platforms are more appropriate for that task. With respect to the impact on the current quota sharing agreement, how does this result impact that agreement? Understanding that temperatures may be a contributing factor in changing allocations is one thing, but the agreement already anticipates range changes irrespective of driver.

**Snarky initial response: Anyone know what a paper that shows they aren’t on the US side of the bank isn’t the form to discuss extirpation risk from GB? Serious knickers in a knot eh…**

I think that the current study should first be tabled as a working paper to the current Transboundary Resources Assessment Committee forum, where a more robust discussion of the data sources, models and management implications could occur and where a wider array of Canadian and USA experts could participate in its review: <https://www.fisheries.noaa.gov/resource/peer-reviewed-research/transboundary-resources-assessment-committee-documents>.

**Snarky initial response: Paper tabled as a CSAS document rather than TRAC and initial results were presented at TRAC (without anyone clamoring for a Working Paper)..**

References

Andrushchenko, I., Legault, C. M., Martin, R., Brooks, E. N., and Wang, Y. 2018. Assessment of Eastern Georges Bank Atlantic Cod for 2018. TRAC Ref. Doc. 2018/01.

Andrushchenko, I., Legault, C. M., and Barrett, M. A. 2019. Alternative Methodologies for Providing Interim Catch Advice for Eastern Georges Bank Cod. TRAC Ref. Doc. 2019/XX.

Gavaris, S., and Murawski, S. 2003. The role and the determination of residence proportions for fisheries resources across political boundaries: the Georges Bank example. In: A. Payne, O’Brien, C., and Rogers, S. (eds.) Management of Shared Fish Stocks. Blackwell Publishing. London.

MacCall AD (1990) Dynamic Geography of Marine Fish Populations. Seattle: University of Washington Pr. 153 p.

Reviewer: 3

Comments to the Author

This manuscript quantified spatiotemporal changes in the distributions of two groundfishes in Georges Bank, using the center of gravity and size of “core areas” as indicators of important stock-specific habitat. The authors provide a very useful comparison of models with and without environmental covariates, specifically focusing on near-term predictive skill. Important findings include long-term distributional shifts with considerable implications for international fisheries management.

Suggested Revisions – General

The manuscript is clearly written and the analytical methods are robust. There is, however, one major issue that should be rectified prior to publication.

Although seasons appear to have been modeled separately, the authors repeatedly make direct comparisons between winter and spring or fall. In my opinion, representing winter, spring, and fall as comparable “treatments” is highly problematic. Winter surveys were conducted by DFO, whereas spring and fall surveys were administered by NMFS. The authors did not explicitly describe how data collection methods differed between the two agencies, though there is a high probability that some variation exists. Any potential difference in sampling or processing would need to be addressed (e.g., in the form of catchability coefficients) in order to appropriately assess seasonal variation. Thus, I recommend that the authors focus solely on interannual (within season) changes in distributions or maintain clear distinctions between winter and spring/fall throughout.

**Snarky initial response: Should be easy enough to revise, as per an above comment will expand the survey description in the methods. I think we can compare them and be a bit clearer that they are modelled separately.**

Additionally, there are ways in which the authors can more clearly present their findings to the reader. Please see specific suggestions below, which should help elucidate the take home messages of this study.

I strongly recommend that the authors make all data and code publicly available to promote reproducibility. The “interactive dashboard” was not available via the link provided; thus, review of the material therein was not possible.

**Snarky initial response: We really need a public Shiny server…. no clue why it didn’t work for dude, but if you weren’t good at R you weren’t ever gonna get this to work.**

Suggested Revisions - Specific

To increase readability, I suggest that all passive language (specifically in the methods and results sections) be changed to active. I also suggest changing “Dep” to “depth” and “Sed” to “sediment type” throughout the main text.

**Snarky initial response: To do… though I thought everyone wanted passive language in science, maybe I’m dating myself that that world view.**

The authors should include a table with model parameters, test statistics, and performance metrics so that the reader has a better understanding about the predictive skill of each model. Relying on the interactive dashboard for this information is insufficient.

**Snarky initial response: To do, add to supplement I guess**

Line 97: Figure 1 is incorrectly referenced (Lines 103-105 would be more a appropriate location).

**Snarky initial response: To do**

Statistical Analysis: Please mention that each season was modeled separately. It wasn’t until Line 168 that this started to become clear (although it was never explicitly stated).

**Snarky initial response: To do**

Lines 125-128: How were these values identified? Please provide some justification and/or supporting references.

**Snarky initial response: To do**

Lines 133-134: The link to the dashboard was inoperable at the time of review.

**Snarky initial response: Look into.**

Line 153: Please define WAIC, CPO, and DIC for the reader.

**Snarky initial response: To do**

Line 162: Complete model results should be made available in the main text or in Supplemental Material rather than forcing the reader to source that information from an interactive dashboard tool, which could be broken or taken down at any point in time.

**Snarky initial response: To do**

Line 165: Figure S1 was the previous figure referenced (this line jumps to Fig. S5). Please reference figures in chronological order within Supplemental Material and in the main text.

**Snarky initial response: To do**

Lines 215-216: Please provide reasoning for selecting different seasons for model validation.

**Snarky initial response: To do**

Lines 247 (onward): Because different surveys were used to collect the data and there was no apparent treatment for differences in catchability, seasonal comparisons should only be made between fall and spring. Any comparisons between winter and fall or spring are likely confounded with survey type.

**Snarky initial response: Given we aren’t talking statistical comparisons I don’t see why we can’t talk about differences between season, just need to make it clear it could be season/survey differences.**

Table 1. The font is difficult to read and makes bold and regular text look similar, especially when italicized. I suggested changing the font to something sans serif.

**Snarky initial response: To do**

Figure 1. I suggest adding latitude and longitude labels to all maps. Because three separate survey locations are shown here, it would be useful to see them represented as different colors and/or symbols. This would help inform the reader about any potential differences (or lack thereof) in spatial extent. Since the topography is not particularly informative here, I suggest removing that layer for maximum contrast with individual data points.

**Snarky initial response: To do**

Figure 2. I suggest moving this map to Supplemental Material.

**Snarky initial response: To do**

Figure 3. The year labels are too small to read. I suggest increasing the font size and removing the boxes for clarity. Because these data rely on multiple surveys (DFO in winter, NMFS in spring and fall), I strongly recommend listing the source of survey data in all captions and differentiating winter from fall and spring (e.g., with an asterisk, different color, separate panel).

**Snarky initial response: To do**

Figure 4. The transparency and lack of contrast among colors makes it difficult to differentiate the seasons in this figure. The fonts are also quite small and this type of representation is unconventional. I suggest using individual points and regression lines (GLMs, GAMs, or loess smoother) for illustration purposes. Again, because these data rely on multiple surveys (DFO in winter, NMFS in spring and fall), I strongly recommend listing season-specific survey types in all captions and differentiating winter from fall and spring (e.g., with an asterisk, different color, separate panel). In this plot, there could be four panels (e.g., top = winter, bottom = fall and spring, left = cod, right = yellowtail). Representing the data in the current fashion can be misleading and encourage the reader to make direct comparisons across seasons. I also suggest selecting either Fig. 4 or 5 based on your main take-home messages and moving the other to Supplemental Material rather than including both in the main text.

**Snarky initial response: To do**

Figures 8 and 9. These two figures could easily be combined using dual axes or an additional panel row. Again, winter should be represented separately from fall and spring (e.g., with an asterisk, different color, separate panel). “Range of field” is not defined in the main text.

**Snarky initial response: To do**

Figure 10. It seems as though this information would be better represented as a table. If a figure is desired, I encourage the authors to increase the contrast (in terms of color) and size of the points and reduce the scale for the bottom panel to better illustrate differences among variables.

**Snarky initial response: To do**

Figure 11. The font is quite small, contrast is lacking, line differences are difficult to detect, and y-axis has too large a range. This is one of the most important figures for this manuscript and should be revised so that the reader can easily differentiate among models.

**Snarky initial response: To do**

Figure S4. This figure is missing a complete caption.

**Snarky initial response: To do**