SOUTH ATLANTIC FISHERY MANAGEMENT COUNCIL

SCIENTIFIC AND STATISTICAL COMMITTEE

Webinar

January 11-12, 2021

<u>TRANSCRIPT</u>

<u>SSC Members</u>

Dr. Genny Nesslage, Chair
Dustin Addis
Dr. Jie Cao
Dr. Chris Dumas
Dr. Churchill Grimes
Anne Lange
Dr. Yan Li
Dr. Amy Schueller
Dr. Fred Serchuk
Dr. Tracy Yandle

Dr. Jeffrey Buckel, Vice Chair
Dr. Walter Bubley
Dr. Scott Crosson
Dr. Jared Flowers
Dr. Eric Johnson
Dr. Wilson Laney
Dr. Frederick Scharf
Dr. George Sedberry
Dr. Alexei Sharov

<u>Council Members</u>

Mel Bell
Andy Strelchek

Steve Poland

<u>Council Staff</u>

Myra Brouwer
John Carmichael
Dr. Mike Errigo
Allie Iberle
Dr. Julie Neer
Dr. Michael Schmidtke

Julia Byrd
Dr. Chip Collier
Kathleen Howington
Kim Iverson
Cameron Rhodes
Christina Wiegand

<u>Invited Attendees/Participants</u>

Rick DeVictor
Shep Grimes
Dr. Jack McGovern

Margaret Finch
Erik Fitzpatrick
Nikhil Mehta

Other attendees/participants attached.

The Scientific and Statistical Committee of the South Atlantic Fishery Management Council convened via webinar on January 11, 2021 and was called to order by Chairman Genny Nesslage.

INTRODUCTION

DR. NESSLAGE: Welcome, everyone, to the first meeting of the South Atlantic Fishery Management Council's Scientific and Statistical Committee for 2021.   My name is Genny Nesslage, and I'm the Chair of the SSC and a faculty member at the University of Maryland Center for Environmental Science Chesapeake Biological Lab.

I would draw your attention to our agenda, which is in the briefing book. We have three items to cover at this meeting, which will span today from 9:00 to 5:00 and tomorrow from 9:00 to noon, or earlier, if we finish early.   The three main items are reviewing the snowy grouper update assessment, reviewing an update on the data workshop decisions that have been made so far in SEDAR 73 for the red snapper assessment, and to very briefly make some appointments to the upcoming mutton snapper assessment.

I would like to, if we can then, perhaps go through and introduce the other members of the SSC. I have already introduced myself. SSC members, if you could, as your name is highlighted, unmute yourself and introduce yourself and give your affiliation, please, starting with Jeff.

DR. BUCKEL: Jeff Buckel, and I'm a faculty at North Carolina State University.

MR. ADDIS: Dustin Addis, FWC stock assessment.

DR. BUBLEY: Wally Bubley, South Carolina Department of Natural Resources.

DR. CAO: Jie Cao, faculty member at NC State.

DR. CROSSON: Scott Crosson, and I'm an economist at the NOAA Southeast Fisheries Science Center.

DR. FLOWERS: Jared Flowers, Georgia DNR Research and Surveys Unit lead.

DR. GRIMES: Churchill Grimes, retired from the National Marine Fisheries Service, and I'm with the SSC.

DR. JOHNSON: Erick Johnson, faculty at the University of North Florida.

MS. LANGE: Anne Lange, also retired from NOAA Fisheries, SSC member.

DR. LANEY: Wilson Laney, retired from the U.S. Fish and Wildlife Service, but also adjunct faculty at NC State University in the Department of Applied Ecology.

DR. LI: Yan Li, and I'm from the North Carolina Division of Marine Fisheries.

DR. SCHARF: Fred Scharf, and I'm a faculty member at UNC Wilmington.

DR. SCHUELLER: Amy Schueller, and I'm an assessment scientist with NOAA Fisheries.

DR. SEDBERRY: George Sedberry, South Atlantic Council SSC.

DR. SERCHUK: Fred Serchuk, SSC, retired from NOAA Fisheries.

DR. SHAROV: Alexei Sharov, Stock Assessment Program, Maryland Department of Natural Resources.

DR. NESSLAGE: Tracy, if you're speaking, we can't hear you. While we're waiting for Tracy to hopefully work with staff to get her audio working, thank you all for your introductions, and we'll come back to Tracy in a moment. I would like everyone to look over the agenda. If you have any suggestions for changes or additions, I would like to hear them now, and if you could please raise your hand.

While you're thinking about it, I do have one thing that I would like to add to Other Business if we have the time. As the SSC members and staff are aware, we are going to try to a new approach to drafting strawman language for our report, and I will talk about that in a moment, but I would like to take a moment, if we have time during Other Business, to review that new approach and get some feedback from SSC members and from staff and the public, to see how you think it went, so we can revise and fix any problems or start fresh with a new idea, if we need to, and so I would just like to add that to Other Business. Is there anything else that anyone would like to add or change to the agenda? If so, raise your hand. If not, I am going to consider the agenda approved. Seeing no hands, the agenda is approved. Thank you, all. Okay. Do we have Tracy onboard?

DR. ERRIGO: Not quite yet.

DR. COLLIER: We're going to continue to work with her to try to get it up and going.

DR. NESSLAGE: Okay. Let us know when she's back on. That would be great. Thank you. the one thing I would like to mention, before we get started, is that we're going to try to improve our meeting efficiency. We have a lot of big, important issues to cover this year, and, in particular, at our upcoming April meeting, and so we've been brainstorming some new approaches to how we can more efficiently draft our consensus statements and come to agreement before we leave the meeting on what our main points will be in our report.

Normally, we kind of go leisurely through our discussion and draft some statements as we go along, and we're going to try something different this meeting and see how it works. SSC members, hopefully you have received your assignments. You have each been assigned to a particular section of our agenda, a particular set of questions under the action item for each agenda item, and staff will post those later, when it comes time, but, basically, what we're going to do, so that everyone knows the plan, is that we'll have our normal -- We'll go through each agenda, and we'll have a presentation, and we'll take clarifying questions from SSC members, and we'll take public comment, and then we'll start our discussion and go over the main points that we want to discuss for our action items.

As we're going through, if each person who has been assigned to that section can keep very careful notes and pay very careful attention to that, those issues as they come up, and start drafting, on your own, some strawman language for how we might address each of the action item questions.

Then each of these breakout groups will leave this main webinar and go to breakout webinars, in groups of three, and staff will post those webinar links in the chat, so that members of the public are welcome to join whichever one they might like to listen in on, but the idea here is that we'll take probably about half-an-hour or so to go ahead and try to draft that strawman language individually in these breakout groups. Then one person will keep track of what that strawman language looks like and send it to Mike at the end of the breakout group session.

Then we'll all -- Everybody will reconvene on this main webinar again, and Mike will display all that strawman language, and then what I would like to do at that point -- Because I would really like the breakout groups to have that opportunity, the SSC members, to really hash out the language in that breakout time. I would ask the public to refrain from speaking, if they could, during those breakout groups, but please keep your questions and comments. When we get back to this main webinar, and everybody has a chance to see that strawman language, I will ask for public comment at that time. That way, everyone can hear your feedback, the whole SSC and all the public and staff in attendance.

Then the SSC will go through and edit, as necessary, and review and finalize and approve that strawman language and turn it into our final consensus statements. The breakout groups will also be recorded, should they need to be consulted at a later date, and so that's the general plan, and I can't promise that it will go smoothly the first time, but, if you all are game, I'm hoping this will help speed us through our agenda items a bit more quickly, and we'll see.

SSC members, just note that one person is going to need to serve at the rapporteur and send that strawman language to Mike, and so please keep that in mind, and you'll probably want to have -- There will be a staff member assigned to each of these breakout group webinars, so you can share the screen and whatnot, and they can help you with that, but do try to pick one person who can serve as that rapporteur.

I would like to thank staff for organizing this. This has been a challenge just to do this for the first time, and I really appreciate all the time they've put into making this happen, and I'm hoping that we'll figure out a new system that will hopefully improve our efficiency. Are there any questions about how this is going to go about, because we're about to start soon into our first agenda item, and I want to make sure that everyone is onboard. Any questions? Okay. I'm sure, as we go along, there will be questions, but, at the moment, hopefully everyone understands what they are expected to do.

If there is no other business for the introduction to the meeting, I am going to open the floor for public comment on any general comments or anything that you would like to say before we launch into our main business for the day. If you have any public comment, please raise your hand. Do we have anyone on the phone only? Fred, I will get back to you in one moment. If there's no public comment then, it looks like, and there will be opportunity as the meeting progresses, and I promise. Okay. Fred, question or comment?

DR. SERCHUK: I have just one question. In the breakout groups, you have assigned Chip to two groups, and how is he going to do that?

DR. NESSLAGE: Red snapper -- We'll do a snowy grouper breakout, and then, when we get to the red snapper one, we'll --

DR. SERCHUK: Okay. Thank you.

DR. NESSLAGE: Chip isn't going to split into two, I don't think, today at least.

DR. SERCHUK: Thank you.

DR. NESSLAGE: All right. Good question. Any other questions? All right. Seeing none, and no public comment, let us begin with our Agenda Item Number 3, Snowy Grouper Update Assessment Review. Please pay attention to Attachments 1 and 2, the assessment report and the presentation, which Rob Cheshire will be giving us, along with Katie Siegfried.

As you may recall, we discussed the snowy grouper update in August of 2020, and we, the SSC, provided some initial feedback on how to incorporate new information about the maximum age of this species, and it looks like Rob is going to review some of that in his presentation, and so I won't go into too much here, but our task is to review this update assessment and then, of course, go through our normal process of providing feedback and fishing level recommendations, and so, Rob, are you teed-up to begin?

DR. CHESHIRE: Yes, I'm ready.

DR. NESSLAGE: Rob, do you have natural breaks at which we can ask questions, or do you want us to wait until the end? What is your preference?

DR. CHESHIRE: I sort of have natural breaks, but I'm not opposed, if somebody wants to jump in during the presentation.

DR. NESSLAGE: I think, given it's a webinar -- If it was a roundtable, I would totally do that. I think, given it's a webinar, it would be easier, since we do the hand-raising business, if we could just do that at the natural breaks, and we'll just have folks raise their hands, and does that sound like a plan?

DR. CHESHIRE: Yes, that sounds great.

DR. NESSLAGE: Okay. Cool. Thank you. Go right ahead.

SNOWY GROUPER UPDATE ASSESSMENT REVIEW

DR. CHESHIRE: I'm Rob Cheshire, and I'm with the stock assessment group in Beaufort, with the Southeast Fisheries Science Center, and I will be presenting this snowy grouper today. Just a little bit of background, this was an update assessment of the 2012 SEDAR 36, and we tried to keep this -- Sort of stick to the script, where we could, and we made the definition for the update

assessment -- It sort of frames us a little bit, and we didn't have any panel discussions, except when meeting with the SSC to discuss max age and natural mortality, and so a lot of the decisions were made by our team in Beaufort.

The terminal year for the assessment is 2018, and we used the same boundaries as used in SEDAR 36 and SEDAR 4, from the North Carolina/Virginia border to the council boundary in the Keys. Here's a little bit of background on just the management regulations. On the recreational side, starting in 1986, the snowy grouper was included in the five grouper per person bag limit. Then, in 2006, that was switched to one snowy per person, and then, in 2011, it was changed to one snowy per vessel, and I think that regulation is the primary reason why the recreational index that we have ends in 2010 and was not updated for this assessment.

We have ACLs starting, for recreational, in 2011, and then, on the commercial side, there's a trip limit that starts in 1994, a fairly large trip limit of 2,500 pounds. That was reduced, in 2006, to 275, and then further reductions in 2008, where it was reduced to 100 pounds per trip. Then one increase there in 2015, and then ACLs, or quotas and ACL combination here, but these are in thousand pounds, and I had a note down here that the commercial ACLs have been met every year since 2013, which is in that 83,000-pound period, and then we also have ACLs being met recreationally in 2013 and 2014, and there has never been a size limit on snowy grouper.

Now to the life history, and I will start running through the data here. This was a topic for our SSC meeting that we had in August, where, for SEDAR 4 and SEDAR 36, the maximum age observed was thirty-five, and so, for this update, there was twenty-five fish aged greater than thirty-five, and one was aged out to eighty years. We discussed that at the meeting, and there was also the bomb radiocarbon age estimates that were validated out to fifty-six, and we came up with a range of values for M, based on those different maximum ages and different estimation methods.

We came up with a way to proceed with the Charnov scaled to the Hewitt and Hoenig value, with an M of 0.08, which that corresponds to a fish of fifty-six years. In the rounding, that max age was actually just truncated down a little bit, and I think it was 0.075, and so, once you rounded it, it actually matches up better with a maximum age of fifty-two, but, as we decided the range from the MCB ensemble ranges, it would be from 0.05 to 0.12. We did not update the growth curve, thinking that it probably wouldn't change that much on the small number of samples that were available to add to it, and so we adopted these recommendations.

This is just a plot of the two different mortality curves, the base run in SEDAR 36 in red, and this update is the lower line there. Just to sort of evaluate what the model thinks about our decision, we did this likelihood profile on M, and the red lines here represent the range that we put into the ensemble runs, and the red circle point there is the 0.08, and you can see that that likelihood profile sort of is fairly flat, once you get around that 0.08, up until 0.105 or so, but we just were hoping to sort of validate our decision somewhat, and I think this does help.

As for the removals, you can see the commercial handline landings in the left panel, in thousand pounds, and it's virtually unchanged, as are the commercial longline in the center panel, and the general recreational time series has some differences, but the most notable here is the 2012 value, and that's -- The other thing to point out there is that, in SEDAR 36, the 1981 MRIP value was excluded, because it was based on a very small number of intercepts, and one in particular, I guess,

was a very large value that caused that -- I forget what the exact number was, but it was even more pronounced in this time around, and so it was excluded.

For the indices of abundance, and here it's listed as general recreational, but that's the headboat index, and it was unchanged from SEDAR 36. That ends in 2010, and both the chevron trap and the MARMAP longline -- I apologize, and VLL was what that longline index was called for SEDAR 36, and I think now we refer to as the short bottom longline, but I will try just to refer to it as the MARMAP longline, so we're all on the same page, but these were both updated using the most recent methods.

The one issue here is that, for SEDAR 36, the 2012 value of the MARMAP longline index was not included, because of limited spatial coverage, and I think funding had decreased in that year, and it was excluded. This is the -- On the left panel is the spatial coverage by year, and I apologize that you can't read the top years here, and it's 1996 to 1999 on the top four panels, and this was provided by MARMAP, and I'm not exactly sure what the colors here mean, but it just shows the coverage, and you can see here, in 2012, you can see the limited spatial coverage, but the number of sets is on par at least with most of the early time series, and we wanted to retain the later years, because spatial coverage increases. We discussed several options within our team, as to how to handle this, but we finally decided on just using the value as provided, with the CV that was provided.

Now on to the composition data, and so this is the comparison of the SEDAR 36 data with the update, and there were a couple of modifications, or the main modifications are the length data, which the sample size cutoffs were applied to the regions used for weighting, and that's just to try and remove any spikes that come up from having a small sample size from an area that has considerable landings.

You can see the commercial handline length comps don't change very much, and, for these years, as with most of the other models, we try to use age comps, where we have a large enough sample size, and, if not, we'll use the length comps, and so the years for these comps are the same as they were for SEDAR 36, and they only go through 2001, with only minor changes to the compositions.

Longline has had more years with regions with very low sample sizes, and so hopefully what we do when we apply that sample size cutoff regionally is we smooth these out a little bit and remove some of the spikes, and the cutoff here was twenty-five fish, and so it's not a very large cutoff.

The general recreational length comps, and you can see we had a couple of years where, once you apply the cutoffs regionally, you don't have a large enough sample size to include them, and so 1996 and 1998 length comps were excluded. For the age comps, for the commercial handline data, these are weighted by the length comps, and you can see there is just small changes to the compositions over time between assessments.

This is commercial longline, and, again, we had a couple of years that dropped off because of sample size, overall sample size, was too low. We don't have a lot for the general recreational age comps, just due to limited sample sizes. The 1981, that represents the only -- There is three time blocks for the recreational selectivities, and that's the only composition in the middle of those two blocks, or those three blocks.

These are the MARMAP chevron trap age comps, and there's not much change to the years where we had comps before, and, with the exception of one year, I think the MARMAP longline age compositions are fairly similar, for the years where we have overlap.

Just a few things to mention that didn't change. The other life history characteristics, the length at-age, weight at-length, the maturity, and the sex ratios are all the same. As I already mentioned, that headboat index was unchanged.

So, overall, we've updated the data with six years of new information, and it also starts in 1974. We tried to use the most recent methodologies and data queries to generate the input data for the model, and we did apply the sample size cutoff regionally, and then the -- Anywhere we had model-based inputs, like the commercial discards, which were fairly minimal, the indices were refit for all years. One change to the data, one of the bigger changes, is the change to the MRIP time series, and then, of course, the big one here is the change in the vector of natural mortality at-age. This is probably a good place to stop and talk about any questions.

DR. NESSLAGE: Are there any questions for Rob regarding the data decisions that he has presented so far? Fred, go ahead.

DR. SCHARF: Rob, I wonder -- Could you go back to the slide with the chevron trap index and the longline index? In looking at the SEDAR 36 updates to those two indices, it seems like the effect of applying some of the current methodologies is to dampen some of the variability, but, in the case of the chevron trap, it seems like it has shifted the location of some of the peaks from between the years, particularly around 2000, and could you talk about that a little bit, and whether or not that was a concern for you guys?

DR. CHESHIRE: I think Wally is on the call, and I don't know if Wally has more information, but we didn't evaluate where the peaks were in this, as far as data input.

DR. BUBLEY: We switched over to using the zero-inflated negative binomial for this one, versus the delta GLM that we used in the past, but that was -- I don't know. I'm not exactly certain why that peak has shifted one year with that.

DR. NESSLAGE: Marcel also has his hand up, perhaps to address this issue.

DR. NESSLAGE: Marcel.

DR. REICHERT: Thank you. I think one of the issues with the chevron trap is that our catches are extremely low, and so there is inherently a high variability in our catches in the chevron trap, and Wally can probably address that further, but I think that also may affect the variability in the index, whether you use one method or another, and I hope that helps. Thanks. Happy new year, everyone.

DR. NESSLAGE: Happy new year, Marcel. We miss you. Does that answer your question, Fred?

DR. SCHARF: Kind of, I guess. I guess I was just concerned that, in the update -- In the SEDAR 36 data, the 2000 year is the lowest point in the time series, and, in the update, it's now the largest value in the time series, and maybe it doesn't matter that much that this whole thing has shifted by

a year, but it just -- Or maybe it was just a plotting thing, but it just seemed a little strange that, all of a sudden, you get that kind of a shift in any particular year.

DR. NESSLAGE: If I could piggyback on that, what was the -- Can you talk, Wally, maybe a little bit more about why you shifted from negative binomial to zero-inflated?

DR. BUBLEY: I mean, it's just been kind of the standard practice, moving forward, just with the number of zeroes, and I guess this isn't -- Actually, with the chevron trap, there's a lot of zeroes associated with the catches. As Marcel mentioned, the chevon trap is just -- It's not an ideal gear, just based on the fish depths that we go to, and so we have relatively small sample sizes going from there, and so, I mean, it's just to account for that, for the large number of zeroes that we have in the chevron trap, and, as I mentioned, that's kind of the way we've been moving, going forward, with these gears as well.

DR. NESSLAGE: But you probably -- So the negative binomial can handle a large number of zeroes, but it's usually best if you have a few number of large catches too, and you probably don't have that, right, because it's a trap.

DR. BUBLEY: Correct, yes. I mean, the trap, it really is. We'll catch one every once in a while, and I would have to look at the total number, but one or two would be a banner catch, I believe, with this gear.

DR. NESSLAGE: Okay. That makes sense. Thank you.

DR. ERRIGO: Fred Serchuk has his hand raised, and then Kyle.

DR. NESSLAGE: Fred Serchuk, please go ahead.

DR. SERCHUK: Thank you. I have a question about the very large catch in the general recreational fishery that occurred in 2012. It's twice as high as the previously high value of thirty-two in 2005, and it's bounded in 2011 by one of the lowest, and, in 2013, by also a very steep decline from 2012, and was there any concern about that very high value in the time series? If you go back to the landings and discards, you can see we've never seen anything like it until that year.

DR. CHESHIRE: We did have concern over that, and we ran a sensitivity where we took the average of the two years on either side of that point, to sort of evaluate what was going on there, and I think our approach in SEDARs has been to accept the MRIP data that were provided and then to run sensitivities to evaluate that kind of thing, because we can also make the same argument about the low values.

They are based on very small sample sizes, in some cases, and I think the year with the peak is on par, sample-size-wise, with years with very low values, and so, if we start evaluate -- We would need to evaluate the entire time series, probably, to really dig into how to deal with these issues. I would say, if you look ahead to the sensitivities, you can probably see that it doesn't really make a big difference in the current status.

I mean, if that peak had been in the last three years, we definitely would have had to have a -- We probably would have brought that to the August SSC meeting, to get some advice on how to deal

with that, but that's how we handle it here, is just sort of trying to mimic what was done in recent SEDARs.

DR. SERCHUK: Okay, and it's strange credibility to think that such a large value is actually representative, even though that may have been based on the samples, but it seems like something is amiss, but, if you indicate that it really has very low impact, in terms of the results, that is good. Thank you very much.

DR. ERRIGO: Kyle put his hand down, and it might have been in regard to the previous issue, but Wally has his hand up.

DR. NESSLAGE: Okay. Wally, please go ahead.

DR. BUBLEY: I just got some clarification from Tracey Smart, who is dealing a lot with our data management, and, over the past ten years or so, we've been cleaning up a lot of historic data as well, and so that could potentially be a change in that 2000 year, where there is a peak, where there is a lot of samples that were either included or not included from previous ones, due to some mischaracterized reconnaissance stations or other issues, and so that's a possibility too with that, is that there's just been some QA/QC recently, trying to adjust some things, in historic years.

DR. NESSLAGE: Thank you for that clarification. Other questions?

DR. ERRIGO: Kyle has got his back up.

DR. NESSLAGE: Go ahead, Kyle.

DR. SHERTZER: I was going to respond to your question, Genny, about why the shift towards the zero-inflated negative binomial, and part of the reason was the zero inflation, but the other part of the reason was the negative binomial part, which is because these are count data that we're dealing with, and so it's a more natural distribution to handle the count data. Even when we turn them into something that looks like continuous data, by dividing by the effort, the effort is pretty much constant across all of the trap instances, and so you get this sort of spiky distribution that's not really a continuous distribution to fit the lognormal distribution, and so that was the other part of the reason.

DR. NESSLAGE: That makes sense. Sorry. I missed it. I thought you were going from regular negative binomial to zero-inflated, but you're going from lognormal to zero-inflated negative binomial, and is that correct?

DR. SHERTZER: It was delta lognormal to zero-inflated negative binomial.

DR. NESSLAGE: So the old school. Okay. Got it. Thank you. That really helps. Great. Are there other questions?

DR. ERRIGO: I do not see any more hands raised.

DR. NESSLAGE: All right then. Rob, would you like to -- One thing, if I could just say, but I noticed in the report that you had a couple years, I believe, where there were no landings reported,

and you replaced that with the average as well, a similar approach to what you did with the really large peak there, and I just wanted to say that I thought that was a great catch. We've talked in the past about how there is probably -- Our eyes are drawn to the peaks, but there is sometimes probably erroneous zero landings as well, and I was glad to see that you did that, and did I characterize that correctly for the general rec?

DR. CHESHIRE: Yes, and so we filled in the zeroes, and then I think -- Yes.

DR. NESSLAGE: Great. That's great. Thanks. Okay. Continue, please.

DR. CHESHIRE: Okay, and so on to the update to the model. It's the same basic model as in SEDAR 36, and it's a catch-age formulation, fit using maximum likelihood. We used the Beverton-Holt spawner-recruit function the last time around, with lognormal error, and we continued that here. We have age-based natural mortality and age-based selectivities, and so we have logistic selectivities for the commercial fleets and handline and longline and for the MARMAP longline survey, and we have dome-shaped selectivities for the general recreational fleet, in three time blocks, and then we have the dome-shaped for the chevron trap.

For the dome-shaped selectivities, the age at full selectivity was fixed, as it was in SEDAR 36, and that was based in SEDAR 36, or maybe SEDAR 40, and then on evaluation of the composition data. We used the Baranov catch equation, and the spawning stock is based on total mature biomass, and so males and females.

Here is more items that didn't change. The model starts in 1974, and the recruitment deviations start in 1974. The modeled age is one to twenty-five plus, and the composition data didn't have a lot of information beyond fourteen, and so we have a plus-group of fourteen in the composition data. We have a constant CV of size at-age, and we used a Monte Carlo Bootstrap ensemble method to characterize uncertainty, and, as in SEDAR 36, the steepness was not estimable, and it wants to go to the upper bound.

We updated the model to the most recent version of BAM, and one of the major components of that is it gives us the opportunity to use the Dirichlet multinomial, and instead of the robust multinomial that was used for SEDAR 36, and this is sort of our standard in recent SEDARs, and the logic is that it accounts for correlation and that it's self-weighting, and so, when we do the iterative reweighting, we don't have to bother with reweighting the composition data. Also, it allows for zeroes in the data, which, for species like snowy grouper, with small sample sizes, in a lot of cases, that is pretty important.

Just to show what changing that multinomial looks like, this is a comparison using the SEDAR 36 data exclusively for both models, and then just through 2012, and you can see that, overall, they're not that different, depending on which multinomial we choose, and, in the case, for snowy anyway, it looks like you get a little bit more favorable outcome with the Dirichlet multinomial, because it's just a better fit to the composition data.

Once we applied some of the changes, we realized that we had some issues with the large plus groups when we had the initial F at 0.03, and that value was used for SEDAR 36, thinking that the exploitation before 1974 would be extremely minimal. The other issue we had was that, when we looked at the model runs, the initial numbers at-age, the log of the initial numbers at-age deviations

were all negative, which isn't something that we expect, and so we did this likelihood profile on initial F, and we used the value from that likelihood profile, the minimum, as the input to the model, and so it changed from 0.03 to 0.15. Then, for the MCB ensemble runs, I just showed here on the right, and we just went to AIC units on either side of that value, and so the MCB ensembles have a range from 0.12 to 0.19. I guess we could stop here for questions, and I didn't cover very much, but those are the updates to the model.

DR. NESSLAGE: Great. Thanks. Any questions about the model update configuration?

DR. ERRIGO: I am not seeing any hands. Alexei raised his hand.

DR. NESSLAGE: Alexei, go ahead.

DR. SHAROV: Good morning, again. Could you please remind us of the reasoning behind the selection of the shapes of selectivities, in particular why the recreational was dome-shaped? Was the basic reasoning behind the availability of large fish? In other words, the majority of the recreational fleet operates closer to the shore, and therefore large snowy are not available, or anything else? I just wanted to refresh my memory on that. Thank you.

DR. CHESHIRE: Alexei, thanks. Yes, that's exactly why the recreational is dome-shaped. I think the idea is just that the recreational fishing is occurring mostly in shallower water, where you see smaller fish, but the other issue is -- There is three time blocks for the recreational fleet, and I was a little confused about that, since there's no size limit, but it turns out, in the early years of the headboat, which is what most of the recreational fishing was during that time period, there was a targeting of deepwater with electric reels, on I'm not sure how many headboats, but it was a big enough issue, during SEDAR 4 and SEDAR 36, to break that recreational fleet into time blocks. The idea is you would have larger fish early in the first block, and then it would go to smaller fish, and then we have another block that starts in 1992, I believe, where it's sort of more like the modern era of recreational fishing.

DR. SHAROV: Okay. Thank you.

DR. NESSLAGE: Great. Are there any other hands raised?

DR. ERRIGO: I am not seeing any other hands raised.

DR. NESSLAGE: Thank you, Mike. Okay, Rob. Why don't you keep going?

DR. CHESHIRE: Okay, and so on to the base run. These are fits to the landings. Of course, they are meant to fit almost exactly. The composition data, I have it in here as these annual plots, and then the typical bubble plots that we see, and I will just kind of scroll these. If something stands out, and you want me to slow down or stop, just let me know.

These are the commercial handline length composition data that go through 2001, where we don't have age compositions. Then, in the middle of the left panel, we start with the commercial longline length compositions, and this runs through 1994. It's missing these length compositions, where you see that sort of estimation of the plus group, and it's a little bit higher, and so that's still the commercial longline in the top three panels on the left side, and then it goes to the general

recreational. These are relatively small sample sizes for a lot of years, and that runs through 1986. This is the general recreational from 1987 to 2018, and so we don't have a lot of years with enough age samples to get these for these recreational plots.

Now on to the age comps, and we'll start with commercial handline. These start in 1997 and run through 2013 in the bottom right. Then, picking up in the middle column here, we have the age compositions for commercial longline through 2005 and then the rest of the commercial longline age compositions.

This is general recreational, which are very small sample sizes, starting on top of the right panel, through 2014 on the bottom-right panel. Here we have the chevron trap age compositions and the MARMAP longline age compositions, starting in the top of the right panel. Then this finishes those age compositions for the MARMAP longline.

These are the same data, but just visualized here with the bubble plots. This is the commercial handline, and then commercial longline, and one issue -- I'm not sure where to bring it up, but one topic for conversation in our group was these commercial longline data and the -- Well, commercial handline too, but the impact of the trip limits on the size distribution of the fish, and so that started in -- I think the first full year was 2007, but you kind of see a shift to older fish, but there's really no mode to it, and so we did have some concerns about that. These are the sparse general recreational age comps. This is chevron trap age comps. This is MARMAP longline age comps.

These are the length compositions for commercial handline, and then commercial longline, and length compositions for the general rec. For the selectivities, these are the fishery-dependent selectivities, and you see commercial handline reaches full selectivity a little bit younger than the commercial longline. For the general recreational, the blue line here is the first time period where we were talking about we had sort of effort in deep water with electric reels, and then it shifts to this green line, where we have younger fish, where the recreational fleet was thought to be more inshore, and then this 1992 recreational, which is sort of the more modern fishing area for the recreational fleet.

This is all the same as in SEDAR 36, and, again, these age at full selectivities were fixed based on evaluation of the composition data. This is the selectivities for the chevron trap and the MARMAP longline. This is another way to look at the comps, and it's just to pool them across -- In this case, we pooled them across selectivity blocks, and so these are the age compositions for the fishery-dependent data.

The year that's listed here is just the first year that we have comps, but we only have block for the commercial handline, starting in 1997, and this is the fit, and these are weighted by the effective sample size, and then the commercial longline is here on the bottom-left panel. For the general recreational, we have one age comp from 1981, to sort of represent the middle recreational time block, and that's not the greatest fit to that one.

Then the age composition is starting in 2003, and I think there may have only been like four years contributing to this pooled composition, and these are the same type of plots for the chevron trap and the MARMAP longline, with just one block for those as well. These are the fishery-dependent length compositions, and this is just the commercial gear, handline on the left and longline on the

right, starting in the early 1980s, and this is the three time blocks for the general recreational. This last block was characterized mostly by age compositions, and so there's not many years, I think, beyond 1997 and beyond for the length composition data that was included in the model.

Now on to the fits to the indices, and this is the -- It says general recreational, but this is the headboat index that ends in 2010 and didn't change from SEDAR 36. This is the chevron trap index fit, and it's fairly flat, and the MARMAP longline index. There are large CVs on pretty much all of these indices.

Here, you can see the spawning stock is below SSB MSY and MSST for the entire time series in the base run. We have recruitment time series, in numbers of fish, and one thing to point out is that we do these two years at the end, and here it's showing 2019, but this is showing 2018 and 2019, and those are from the stock-recruit curve, and so those aren't being estimated, but you can see we have these -- Since about 2011, we have this period of low recruitment in recent years.

This is the stock-recruit curves, and this is that group of recent year low recruitment, and then, of course, these are fixed, or these are not estimated, I should say, 2018 and 2019. This is relative fishing, and it's very close to FMSY in recent years, but, when you take the three-year average, it comes out to slightly above F over FMSY, and this is the relative contribution by fishery, and you can see this large F. For MRIP in that year, we have a large landings estimate, but it's sort of switched from mostly commercial to sort of maybe a more even split between commercial and recreational in the most recent decade. Then we're sort of well below the SSB over SSB MSY. I am sure there will be questions for this section, but we can take those now.

DR. NESSLAGE: Great. Thank you, Rob. I agree there are probably many questions. Who would like to start? Who do we have on our hands-raised list?

DR. ERRIGO: Chris Dumas has his hand raised.

DR. NESSLAGE: Chris, if you don't mind, since your internet was cut there, do you mind introducing yourself and then asking your question, please?

DR. DUMAS: Sure. Hi, folks. I'm Chris Dumas, and I'm an economics professor at the University of North Carolina Wilmington, and I work on coastal and natural resource economics, and so my question is about the slide in this section that was just before the recruitment deviations, and so if you back up. Looking at the scaled residuals here, it looks like there could be autocorrelation in those residuals, and also on the recruitment deviations, and I'm wondering -- Does the analysis take into account or correct for autocorrelation? I am just not as familiar with how this particular type of analysis is done. Thank you.

DR. CHESHIRE: I'm trying to figure out how to respond to that.

DR. DUMAS: It would be sort of a time series question on whether the model can adjust for correlations over time in the residuals.

DR. CHESHIRE: Well, Kyle and Erik can correct me if I'm wrong, but I don't think that's built into our model, although I'm not sure if that's built into the error structure when the model is developed.

DR. DUMAS: Right. That was my question, and I'm not sure if it's part of the model in general, but, if it's not, then that's something that you might want to look into, because it looks like you could have some autocorrelation in those residuals, especially if recruitment -- If there is autocorrelation in recruitment over time, and that could lead to autocorrelation in the residuals, and taking that into account might produce better estimates over time. Thanks.

DR. ERRIGO: Fred Serchuk has his hand up.

DR. NESSLAGE: Great. Thanks. Go ahead, Fred.

DR. SERCHUK: Thank you, Rob. These are questions that I transmitted to Genny beforehand, but I will raise them here for the general SSC. Looking at these diagrams in front of us, you notice that they have very wide CVs, and that suggests to me that it's very difficult to discern differences across the time series, because most of the CVs are large enough that they overlap, and that's true for every one of the cases that you have shown us, in terms of the fits. Would that be a correct interpretation?

DR. CHESHIRE: I believe it would, yes. The CVs are fairly large for all of the indices.

DR. SERCHUK: So my point is that it's very difficult to see a trend here, quite frankly, in any of them, even though there's a -- The fitted line might indicate a trend with a large variability around the individual points, and it might suggest that, well, one could interpret it as no difference in the relative abundance across any of these independent measures of abundance. Would that be a reasonable interpretation, or could that be one of the interpretations?

DR. CHESHIRE: I believe it could be, and I guess I would look at it from the other perspective, is that I think the model -- Because the CVs are large, I think it's reassuring that the model is not fitting those points very well for peaks and valleys, and it is sort of running more of a smooth line through them.

DR. SERCHUK: Okay, and the other question that I raised in my correspondence is the stock-recruit relationship, and if we could go to that slide. That's the one right there. I realize that the formulation was that you were going to fit a Beverton-Holt curve, but, if you did not fit a Beverton-Holt curve, and you just looked at the relationship between recruitment and spawning stock without imposing a pre-determined relationship, apart from that very high 1975 point, one could either draw a straight line through it, through all the points, or one might even think about there is a slightly negative relationship between stock and recruitment, because the lowest points occur at the very end and in the middle, and the highest points, again, apart from that 1975, occur at very low stock sizes.

I am just wondering what impact using a Beverton-Holt curve is on it, when it's easy -- It's easily plausible that you, without predetermining the type of curve you would fit, you could see that maybe the highest recruitment curves at very low stock sizes, or at medium stock sizes, and, when you get to very large stock sizes, you get very low recruitment again. I am just asking for your comment on it. Thank you.

DR. CHESHIRE: Thanks, Fred. Being that this is an update, we didn't fully explore the Beverton-Holt application here, because it seemed like it was outside of the update, but I can see your point, and, after I saw your comments, I did try to run -- We have an option where we can run a constant average recruitment, instead of trying to fit the Beverton-Holt, and I could not get it to converge with this model.

DR. SERCHUK: Okay, and the only reason I raised it was because the stock-recruitment relationship has a major bearing on the BRPs, the biological reference points, and so, if you accept the curve that recruitment is going to increase at stock size, and the fact is the data don't overwhelmingly show that, our perception of stock status could be very different if we used a different fitting relationship. That's all. Thank you.

DR. NESSLAGE: Good point, Fred. Do we have others with their hands raised?

DR. ERRIGO: I am not seeing any other hands raised at this time.

DR. NESSLAGE: Okay. If that's the case, do you want to -- I think you have sensitivities and retrospectives next, and is that correct?

DR. CHESHIRE: Yes, that's right.

DR. NESSLAGE: Thanks, Rob.

DR. CHESHIRE: For the sensitivities, we have -- We don't have a lot of them, and we just tried to limit it to ones that match what was done for SEDAR 36 and to evaluate the changes we made to M. We have one sensitivity where we took the two years on either side of that MRIP peak in 2012 and applied an average. We have another pair of sensitivities where we used the range of natural mortality that went into the MCB ensemble. We have a low and a high F in it, based on the two AIC units from the likelihood profile, and then we have a low steepness and high steepness sensitivities to look at, and this doesn't fully cover the range that was used in the MCB ensembles, but it at least gives us an idea of bracketing the steepness for the base run, which was 0.84, and so the effect of smoothing that 2012 MRIP peak doesn't have much impact at all on the stock status.

Looking at some of the other output from that model, it really seems to have more of an impact on the years prior to 2012. Like I said, I think we would revisit this decision, or we would have had a lot more discussion about it, had it been in one of the last three years.

As far as the natural mortality sensitivities, this plot shows the base in orange, in the middle of the two lines there, and then, of course, the high natural mortality value, which is the same -- I should point out that's the same value that was used for the base run for SEDAR 36, is in blue, and the yellow here is the 0.05, which was the lower end of the range for the MCB ensemble. Later on, when you look at the uncertainty, this sort of range captures the uncertainty for the whole model, and I think this is what is driving a lot of that uncertainty.

The initial F doesn't seem to have an impact on recent years, but it's just that initial time period up until about 1980, or 1982, where you see some differences, but, for most of the time series, it really has no impact. As you would expect, steepness does have some impact on where we think we are, and the lower steepness leads to lower stock status. Higher steepness, as you would expect,

means we have a higher stock status, currently, but, again, those are incorporated in the uncertainty that we get for the final model.

For the retrospectives, we don't really see any alarming patterns for F over F MSY, and this is just peeling it back five years. The recruits here, these are the last year of estimated recruits in that panel, for SSB over SSB MSY. This is the table of output from those sensitivities, and one of the terms of reference was to look at this percent SPR. In the base run, it's 26 percent, and then you see the sort of bracketing that was I think part of the terms of reference, and so that's why we included that in this table and the report. That's all I have for sensitivities and retrospectives, if you want to have any discussion about those.

DR. NESSLAGE: Great. Thanks. Are there questions for Rob about the sensitivities and retrospectives runs?

DR. ERRIGO: Fred Serchuk has his hand up.

DR. NESSLAGE: Fred, go ahead.

DR. SERCHUK: Just a comment, again, and I'm sorry if I sound like an echo repeating myself, but those valuations relative to the reference points are conditioned on the stock-recruitment curve, and I recognize that this is an update, and so we basically have to more or less stay with the Beverton-Holt curve, but, to the extent that that doesn't really capture, in my mind, the relationship that we just saw a few slides ago, we're going to get exactly what you indicated, because the reference points are conditioned on the stock-recruitment curve, and would that be correct? I mean, the F to FMSY, the B to either BMSY or SSB MSY are all conditioned on reference points derived from the stock-recruitment curve.

DR. CHESHIRE: Yes, that's correct.

DR. SERCHUK: Okay. Thank you. I just wanted to make that point.

DR. CHESHIRE: I think in -- I guess the next operational assessment for snowy, that might be something worth adding to the agenda.

DR. ERRIGO: Kyle has his hand up.

DR. NESSLAGE: Kyle, please go ahead.

DR. SHERTZER: The difficulty we ran into with that issue is that, when you see steepness going to the upper bound, it means one of two things. either recruitment is a flat line, like an average, as you suggested, Fred, or else the data are just uninformative on steepness, and we've seen that play out through simulation studies, and so the thinking, at the time of SEDAR 36, was that we couldn't rely on the data to inform our estimate of steepness, and so we relied on meta-analysis of what steepness might be when you can't estimate it, and so that was the decision during SEDAR 36, or at least the line of thinking, and I think Rob mentioned, for this assessment, it was an update, and so we followed through on that line of thinking.

DR. ERRIGO: Fred Serchuk has his hand up.

DR. NESSLAGE: Fred, go ahead.

DR. SERCHUK: Thank you for that explanation. That was very helpful. My review of the report, and this is why I raised the thing about the CVs around the indices, is all of the indices, all of the independent measures of abundance, seem to indicate that there has been -- That abundance has been pretty much the same since 1984, and I'm just surprised that the relationship of the stock, in terms of being relatively constant throughout that period, is so far below BMSY and SSB MSY, and that suggests to me that the dynamics of the stock, in the past thirty-four years, doesn't seem to make sense with our estimates of the reference points.

Again, I recognize that, being an update, there are things that could not be changed or are not permissible, but I am getting a feeling that this stock has somehow reached an equilibrium, over the past thirty-four years, which seems out of sync with the reference points, and, again, I recognize that there's probably little we can do at this point with it, but I just felt a little bit uncomfortable with that. Thank you.

DR. NESSLAGE: Rob, do you have anything to respond? If not, I have a question to follow-up on that.

DR. CHESHIRE: No, I don't have anything.

DR. NESSLAGE: I am going to play devil's advocate and go the opposite direction of Fred, just to be difficult, and I am looking at Slide 11, at least on my PDF, and so the plots of the indices. If you -- Given that those -- Am I correct that the CVs on those indices are model-based, right?

DR. CHESHIRE: Yes.

DR. NESSLAGE: Let's say that they maybe were overestimating the interannual variability in these indices, and, if that's the case, then the trends could be true, or representative of the stock in general, through two out of the three have similar declines in just the trend alone, and the other one has a more peaked pattern. I guess, did you -- Let's say, or imagine, for a moment, that the CVs were low, and did you run any alternatives where the indices were upweighted, just to see how the model responds? What if at least two out of these three indices are correct, and, if you didn't do that, is that because you don't trust these data sources as much as the catch at-age, or what's your feel for that?

DR. CHESHIRE: We didn't explore that, mainly because the sample sizes are very small on these indices compared to what we normally get for fishery-independent indices. I don't know if I would say that I don't trust them, but they're not as robust as we typically see.

DR. NESSLAGE: So the decision then, if I'm understanding, is that -- Because these are not fit well, and, I mean, I think we're pretty much largely -- If I am interpreting the graphs correctly later, when you show the fits, they are pretty much not fitting the overall trends well, and so we're pretty much largely ignoring the indices then, and is that a correct interpretation?

DR. CHESHIRE: It is. It's what we consider the best available, and it's what was in SEDAR 36, and so the model is not doing a good job of fitting them, and we're okay with that.

DR. NESSLAGE: I apologize, because I didn't go back and look, but what did the fits look like during that last SEDAR?

DR. CHESHIRE: It's fairly similar. If you go back and look -- I can pull it up, if you want, but they're fairly flat, as they are in this assessment.

DR. NESSLAGE: Okay. Thanks. I appreciate that. Are there any other hands up?

DR. ERRIGO: There are no other hands raised. I know Chris Dumas had some stuff in the question box, and I don't know if you wanted to say any of that on the record or not.

DR. NESSLAGE: I can't see the question box, and so, Chris, if it's intended for the whole SSC, you're going to have to raise your hand and say it out loud.

DR. DUMAS: That was just follow-up information from the last point that I made about autocorrelation, just some additional information who might want to follow-up on that, and that's all.

DR. ERRIGO: Okay. Thank you.

DR. NESSLAGE: To that point, if I may ask a question, Rob, when you tried the average, the constant average recruitment alternative run that didn't converge, and I know there's some formulations of that that will include an autocorrelation term, for other models, and I don't remember if BAM allows for that, and was that in there or no?

DR. CHESHIRE: To be honest, BAM is -- I was doing that last night, late, just to try and see if I could get it to run, and I didn't dig too much, and it's a switch that I turn on in BAM, and so, if Kyle has any information on whether there is autocorrelation in that, maybe he could provide that.

DR. NESSLAGE: Kyle, can we put you on the spot?

DR. SHERTZER: Sure. It is an option in BAM to allow autocorrelation computing the recruitment residuals, and so it is an option and it doesn't tend to affect the estimates of recruitment very much, but it could affect the forecast, if you have autocorrelation that leads to low or high recruitment going into the future.

DR. NESSLAGE: Great. Thank you. Are there other questions, before Rob moves on to uncertainties?

DR. ERRIGO: Fred Scharf has his hand raised.

DR. NESSLAGE: Fred, go ahead.

DR. SCHARF: I guess this is sort of just a broad comment on just the observations of sensitivity and uncertainly, and maybe both Rob and Kyle could sort of address it, and I don't know if there's necessarily a specific question, but it seems like, when I look at the assessment report for the species, I am seeing patterns that I feel like I've seen over and over and over again with many of

the species, particularly in the snapper grouper complex that are under the jurisdiction of the council, in that we always have tremendous uncertainty in natural mortality and tremendous uncertainty in the stock-recruit relationship, and particularly the steepness parameter, and those two parameters often drive the stock status, whether or not we're overfished or overfishing is occurring, those changes within the boundaries that we analyze, and the sensitivity can flip the position of the stock.

We're often finding ourselves where we don't feel like we have a really good understanding of the population dynamics, those key pieces of information related to natural mortality and steepness and stock recruitment, and we find ourselves constantly kind of having to lean back on these meta-analyses, and Kyle just referred to some of those related to steepness and the stock-recruit relationship.

I just wondered, and maybe Rob and Kyle can speak a little to this, about where they feel like we are, broadly as a discipline, in terms of how confident they feel in the application of many of those meta-analyses to inform those key parameters in our assessments, because I feel like, in most cases, especially with our -- Many of these species are long-lived, and they have low rates of natural mortality, and so the uncertainty becomes even more important when you're trying to estimate something that's really small to begin with. I just wonder how they feel about where we're going to be moving forward, because we often look at these kinds of things with the species we assess, and we see the same patterns over and over again.

DR. CHESHIRE: Thanks for that. I might defer to the big-picture people on this one, because we do see these sort of patterns, and we have moved our modeling towards using those meta-analyses more frequently, but I think it's to encompass the uncertainty better, but it does make it difficult to pinpoint a base run.

DR. NESSLAGE: Katie or Kyle, would you like to chime in on that at all?

DR. SHERTZER: Sure, and I think that's a great description of our sort of state of knowledge on the two driving parameters in stock assessments. I guess I think about natural mortality and the recruitment relationship a little bit differently. For natural mortality, we have these meta-analyses that I think are really good analyses, and that life history features to natural mortality, which we would expect to occur through life history evolution, but those average relationships are just that, and they may or may not describe any given species very well, and so that's the conundrum, but we often don't have other information that is species-specific or stock-specific to rely on, and so we're forced to use these meta-analyses for natural mortality, until we can, I guess, devise or conduct more studies on these particular stocks, for example through tagging studies or something like that.

For steepness, I think maybe our thinking is starting to get away from using the meta-analyses, and similar to what's done in the Northeast, where we're just not assuming any relationship in the spawner-recruit data and just using that average, unless we find that the data are very informative on steepness. If we do a likelihood profile on steepness and find that it's well informed by the data, then we might use a Beverton-Holt relationship with that steepness value, but, more typical, we don't -- We're unable to estimate steepness, and, in those cases, we're starting more and more to just use that average recruitment that Fred Serchuk suggested describes the data better, and then

to not use MSY benchmarks, but to use proxies instead, and so I think that's sort of where we're heading right now on the recruitment front.

DR. SCHARF: Thanks, Kyle. I appreciate that perspective.

DR. ERRIGO: Alexei has his hand up.

DR. NESSLAGE: Great. Alexei, please go ahead.

DR. SHAROV: Following probably on what Kyle was talking about, I was little bit surprised, in looking at the time series of F and the F over F MSY ratio -- Essentially, it would have been, according to the model results, severely overfished in the period of like three decades or so, and, even at the very beginning, like in the 1970s, the estimated SSB over SSB MSY is slightly over 0.4, and so I don't know if this is the true interpretation of the stock status and where it ever has been at the SSB MSY, like the 1900s, and this is not a criticism, and I am just trying to -- Could somebody, Rob or Kyle, walk us through a basic interpretation as to how we end up with a long series of the high Fs and low SSB, well below the estimated reference points, and the interworking of the calculations? I am just trying to put things together and sort of make sure that it makes sense.

DR. ERRIGO: Katie has her hand up, perhaps to answer Alexei's question.

DR. NESSLAGE: Great. Katie, go ahead.

DR. SIEGFRIED: Thank you. I can address Alexei's question, and I also wanted to mention something kind of to Fred Scharf's other point. This stock, and other deepwater stocks, will have better index information in the future, and that's one thing that we're hoping will happen with this new deepwater survey, but that can just add to that list of things that Kyle had mentioned.

To Alexei's point, one thing that the Beaufort team sort of discussed is the benchmarks are really based on recent years' data, and we don't have a moving average plot for you, and so it's not really the interpretation we want everybody to take away, is that it's been overfished based on the recent MSY, or sorry, overfishing based on the recent FMSY. It's just the way that it's plotted, and we do want to move forward and potentially provide these moving average plots, but it just takes a lot more time to run the plots and the models that way, but I just wanted to add that to Alexei's comment.

DR. NESSLAGE: Thank you. Are there other comments or questions on this topic?

DR. ERRIGO: Fred Serchuk has his hand up.

DR. NESSLAGE: Fred Serchuk.

DR. SERCHUK: One other point I wanted to raise -- I've already raised my point about the biological reference points, but I also want to raise another point that I raised in my correspondence to the Chair and forwarded on to Rob, and that is the use of the apical F as the measure of fishing mortality.

When I looked at the tables that gave the -- That listed the landings, the removals, by age, and the instantaneous Fs by age, this is a fishery that, in most cases, for most years, a large proportion of the landings, or a large proportion of the removals, occur at very young ages. I am not convinced that the use of the age, the F -- The highest F at-age, at a single age, that is the apical F, is characteristic, or captures, the appropriate fishing mortality.

I was looking at the landings table, or the removals table, and it looked like most of the landings occur from ages five and below. This is a fishery that typically lands a lot of young fish, at least in many years, and I'm just wondering whether those partially recruited Fs could somehow be looked at in terms of averaging F across age groups which compose most of the removals, and I'm just wondering whether that thought was ever considered. I realize the apical F is used in many assessments to characterize F on the stock, but here is a fishery in which a significant portion of the removals are at very young ages, often less than the apical F age. Thank you.

DR. CHESHIRE: Thanks, Fred. I did think about your comment a little bit before the meeting, and it kind of strikes me that, when there's an update assessment, some things we sort of don't reevaluate, and, as an analyst, you can't dig into everything, given the time constraints, but apical F is one that I really hadn't considered, mainly because it's just been the standard for almost every assessment that I have been involved with. That's not to make an excuse for that, but can you suggest -- So you're suggesting that possibly using an F limited to certain ages?

DR. SERCHUK: I am suggesting that I think the -- Again, I'm not trying to castigate you in any way, and this is an update assessment, and I realize that, but we've talked about how the biological reference points are dependent upon steepness and the stock-recruitment curve, and that looks at one aspect of comparing where the stock status might be, and the other one is what the F is, and I look at Tables 10 and 11 in the report, and there seems to be, in many years, significant landings, significant removals, of fish at ages younger than that used for the apical F, and those fishing mortality rates were lower than the ones that -- The fishing mortality rates were lower than the apical F, and I'm just wondering whether it might be worthwhile, perhaps, obviously in the future, to think about that question, and is there another representation of the F on the stock, which would be used with respect to our reference points, which would combine more age groups, particularly those that characterize a significant portion of the catch. That's all, and, again, I recognize the constraints that the scientists are under when they're using an updated assessment, and this is another food-for-thought question for perhaps the next time the stock is evaluated. Thank you.

DR. ERRIGO: Alexei has his hand raised.

DR. NESSLAGE: Great. Go ahead, Alexei.

DR. SHAROV: Thank you. My biggest comment was -- Well, first of all, the comment, but then there was a question, actually, or a request for the assessments to thoroughly explain what we see in the terms of the dynamics of the SSB and F for the stock, and I am just still trying to put these things together and see whether they make sense.

To follow-up on this, Fred was talking about the importance of the reference points, the sensitivity of reference points, as independent on the stock-recruitment relationship, but there is also an issue of natural mortality and the sensitivity analysis that you have done, and I'm looking at Slide 66 of the presentation, where it presents base, low, and high natural mortality values, and so those were

considered reasonable upper and lower bounds, and you could see how the status of the stock changes, and it's probably more reasonably interpretable at the higher end, but this is obviously -- The shift is expected, and it's always an interplay between the fishing mortality and natural mortality, as they are competing for the fish, and I'm not suggesting that every time we see a problem that we need to just raise the value of natural mortality and make this stock from overfished to not overfishing, not being overfished, but it's food for thought here, I think.

Even though the information, the estimated trends, are -- The shape of the trend through a decadal period seems to be the same, the ratio of SSB over SSB MSY does change quite a bit, and, to be honest, and, obviously, I am being naïve here, but, to me, what I see here for the M of 0.12 is a bit easier for me to explain, given all the information that I've got so far, and it's harder for me to explain the base run so far, to interpret it, but I would like to hear -- To be challenged, and if somebody would explain to me that I am wrong here in this guessing, and I would be happy to hear that.

DR. CHESHIRE: Alexei, I'm just giving it some thought, and I also would prefer it, the stock to be at the similar to the high M line, but it's a hard -- Natural mortality was difficult for us to come up with a number for during the August meeting, and we were doing it without seeing these results, and also without seeing the likelihood profile on M, because we didn't have the base run at that time, and so we have discussed how, maybe in future assessments, maybe we could estimate M in the model, and that might likely come out to be a slightly higher value, but then we have to struggle with the inconsistency in what we would expect, given the literature on natural mortality and point estimates in conflict with what the model is saying.

In this case, it's not that extreme, but I could see, in other assessments, where that difference might be larger, but seeing an entire time series starting in 1974, with the stock overfished, is kind of difficult to understand, given the exploitation rates, but I think, as Katie was saying, it's not meant to -- We definitely need to redo some of these plots, where we look at these sort of moving -- I'm not sure how Katie referred to them, but these moving plots, where MSY changes over time.

DR. SHAROV: If I could just continue the thought, if we had Fs exceeding FMSY for four or five or eight times in some years, but in the course of like forty-plus years, and I think we were to expect a significant truncation in the age structure of the population, and I thought that what we saw, and it was pretty much a rather stable age structure, and I didn't see a significant lowering of the age distribution.

I understand that, given the structure of the model, and given all the data that we had, and we have looked at, that's the outcome that you get, but there is quite a bit of uncertainty in some elements, and the natural mortality is an easy one to look at and point out, and it's not necessarily the one that will tell us what actually is or should be happening, but it certainly tells us how uncertain our interpretations are, given the assumptions that were made. I'm sorry, and I guess this is a pretty trivial observation, but I am trying to just observe everything and see that it just makes sense. Thank you.

DR. ERRIGO: Fred Serchuk has his hand up.

DR. NESSLAGE: Okay. Great. Fred Serchuk, go ahead.

DR. SERCHUK: Thank you. Just one other comment. I know that, in the document, there is a table that gives the natural mortality at-age based on the overall estimate, updated estimate, of M, and, when I looked at the values of M at-age, or the stock for the apical F stock, apical F -- That age group that was apical F, for example, the natural mortality at age-five is 0.16. At age-six, it was 0.14. At seven, it's 0.13. At eight, it's 0.12. At nine, it's 0.11, and at ten it's 0.11.

These are the age groups that make up almost all of the catch, and they are responsible for an M at-age that is closer to 0.10, and, in many cases, it's higher than 0.10 for the youngest age groups, and so, even though we have carried the M out across twenty-five age groups in the assessment, the age groups from fourteen and above are considered a plus group. The ages that are making up the catch have a much higher M than the average that's been used to characterize the entire stock, and I think that also has an impact. Thank you.

DR. NESSLAGE: So that was more of a comment. Rob, do you have anything you want to add or respond to that?

DR. CHESHIRE: No, I don't think so.

DR. NESSLAGE: Great. Are there any more questions? He still has a little bit more to present to us, and are there any more questions that are burning on the SSC's mind at the moment, or should we let Rob finish up here?

DR. ERRIGO: I don't see any more hands.

DR. NESSLAGE: Great. There will be more opportunity, but, Rob, if you don't mind, let's finish up with the projections, or uncertainty, I guess, and then projections.

DR. CHESHIRE: Right. Uncertainty. This is our typical MCB ensemble approach. For the Monte Carlo components here, we have the M drawn from random uniform distribution with the range that we discussed from 0.05 to 0.12. Then we put those in our model, and then the Charnov is scaled using those values. The F_init was drawn from a random uniform distribution from 0.12 to 0.19, and then steepness -- We wanted to go to the upper bound, and it was drawn from a truncated beta distribution from 0.32 to 0.99. That's the same as it was in SEDAR 36.

For the bootstrap, our typical approach is, for the landings, discards, and indices, we do a parametric bootstrap for the original data, with the CVs applied and the fitting procedure, and then we resample the number of fish from the length and age comps and assign them to bins with the same probabilities as those from the original weighting.

We attempted 4,000 runs, and I think the ultimate goal is to at least have 3,000 retained, which is about where we're at, and so fifty of these runs just didn't converge at all, and I'm guessing it's the combination of the steepness and M, where those runs were excluded, and these plots just kind of show how the other 896 values were removed, and most of them were hitting a bound, or R zero, and so the plot on the left shows a fairly clean break here between the R zero value predicted relative to the constant M and steepness that were put into the model.

Pretty much all of the light-blue dots there were excluded because they were hitting an R zero as an upper bound, and what that does to distributions in the Monte Carlo approach is you see a slight

change to the uniform distribution for the constant M, and some of the lower values were removed, probably in conjunction with the low values of steepness, and so what we wind up with is more of a distribution that runs from about 0.5 to 0.99 for the steepness and the Monte Carlo ensemble.

These are just the density plots, and so the distribution for these parameters, with the solid line as the base run and the dashed line as the median from the MCB bootstrap runs. Here, you can see we have a pretty wide range for some of the MSY and SSB, MSY especially. These are the uncertainties for those parameters. You can almost see that they mimic almost exactly what the range of -- The uncertainty in the range of Ms that had put into the model. The range in steepness is a pretty big part of this, also. It looks like we've got a little over half of the runs are showing that overfishing is occurring, and all except for about 18 percent show that the stock is overfished.

This is just a comparison with the previous two assessments, and so these are plotted -- This is F over F MSY, and you can see that the SEDAR 4 ends in 2002, right here, and SEDAR 36 is a blue-dashed line, which ended in 2012 here, and, with the exception of this 2012 F, these values aren't extremely different over the time series, but the value going into that 2012 for MRIP was quite different.

Then this is the SSB over SSB MSY comparison across assessments, and these initial sort of decreases -- I think these are where the initial F changed, but those didn't have much impact beyond the mid-1980s. If anybody has questions on uncertainty, we can talk about those.

DR. NESSLAGE: Maybe just finish with the projections, because they're kind of linked, right, and then -- Why don't we do that, and then you'll be done your slides, and we'll just do some more questions, if that's okay.

DR. CHESHIRE: Yes, that's great.

DR. NESSLAGE: Great. Thanks.

DR. CHESHIRE: The projections, we ran a suite of typical runs here, the F current, F equals zero, 75 percent of FMSY, and then we had a run where, if the probability of rebuilding is 0.5 in 2037, I believe, that value turned out to be 0.08. Then we did a couple of runs just for discussion, if they're needed, with sort of using the average recruitment of those low-recruitment years from 2011 to 2017 and running 75 percent FMSY and F equals zero for those two.

I will only show a couple, or maybe just one of these, but I have them all in the extra slides, if we want to look at them, but what we have for each one of these runs is the usual panel. The thick, blue, solid line here is the benchmark for the base, and the dashed green line is the median, and then we have the closed black circles is the deterministic. The dashed line that's hard to see with the open circles is from the median, and then the thin lines represent the 5th and 95th percentiles. In these particular runs, the 75 percent FMSY, you can see slightly -- It's 2039, I believe, and the terminal year is slightly above 50 percent probability of rebuilding.

This is the table from that run, and this is assuming that management starts in 2023, which we had a little discussion about before we ran these. This is just an example of the low recruitment, and this is 75 percent FMSY, using the average from 2011 to 2017.

I will just do this quick summary slide, and then we can have questions, but, right now, we're showing snowy is overfished and overfishing. The change in natural mortality had a pretty large impact on its status, along with those low recruitment levels that we're getting for the recent years.

Some recommendations for the next operational, hopefully, would be to reevaluate some of those, many of those, MRIP landings estimates. One thing that we kind of didn't have information on to make an informed decision, like we would if there was a size limit change, is if the selectivity for commercial fishing had changed, given those trip limits, and there was a good discussion during the tilefish panel discussion from describing how fisheries changed pretty dramatically for a lot of these deepwater species, and it would be really good to have that conversation for snowy grouper.

Of course, natural mortality, if we can maybe dig into that a little bit deeper in the future, where we incorporate some of the -- I think there's a paper coming out soon by Lorenzen that was discussed for the scamp SEDAR 68, and for red snapper, and then possibly the methods used in those assessments, where you trend that and trim it to just the meta-analysis that's used to just the species that makes sense. Then, of course, this is one species that would benefit greatly from a really robust deepwater survey. If you want to have questions now.

DR. NESSLAGE: I am looking at the clock, and we've been at this for two hours, and I'm thinking that folks might need a biological break, especially those of you in the hot seats there, and how about we take -- Can we take a five-minute break? Is that reasonable? Do folks think they could be back -- Let's make it six and be back by 11:10, and we'll start with questions about the last two sections here, and does that sound good?

DR. CHESHIRE: Sounds good to me.

DR. NESSLAGE: All right. Let's reconvene at 11:10. We'll get started right on the dot. Sound good? All right. Thanks, everyone.

(Whereupon, a recess was taken.)

DR. NESSLAGE: Hopefully folks have rejoined us. I would like to entertain any clarifying questions on uncertainty and projections for Rob.

DR. ERRIGO: Just so you know, I took control back to put up the slide here, but I thought that maybe I can keep control of the screen for now, and I will put up whatever slides need to be put up, and I have the presentation here, and Rob can just answer questions, since there's no more actual presentation. Does that work?

DR. CHESHIRE: Yes. Thanks, Mike.

DR. NESSLAGE: Are there questions for Rob? I have one. Maybe you mentioned it and I am just starting to tune out, but you assumed a uniform distribution for M, and is that correct?

DR. CHESHIRE: Yes, it's uniform for F_init and M, and then the steepness was the truncated beta.

DR. NESSLAGE: I think it's tilefish, and I don't remember what some of the other species are doing, but we've discussed using a non-uniform distribution, and I was wondering if you got -- I know it's an update, but maybe this is something for research recommendations, or next time around, but do you think you would -- You had a lot of runs that were thrown out, and I'm just wondering if having some distributional assumption around that might help, and did you explore that at all or talk about it at all?

DR. CHESHIRE: We did have a brief discussion about it, but we just decided to stick with the SEDAR 36, and there might have been some other justification for this, given the uncertainty in M. I don't exactly recall if anyone else remembers that discussion from the team.

DR. SIEGFRIED: We didn't have a really good distribution of max ages, and we just had a lower and upper bound, and I can't imagine fitting a different distribution without better information between that min and max.

DR. NESSLAGE: That makes sense. Are there any other questions for Rob? If there is no other clarifying questions -- We'll take one from Fred Serchuk.

DR. SERCHUK: I don't know whether it's to Rob or to Mike, but, just by way of comparison, from the last assessment, what was the year in which the stock would be rebuilt, because I think it's important relative to changes we made to M with respect to what we had previously recommended in terms of a rebuilding period and what we're now recommending for a rebuilding period. Do we have any idea when we felt the stock would be rebuilt from the last assessment?

DR. CHESHIRE: I believe it was 2039, and I did do the -- With the new M, you do get a different generation time, and so I did recalculate that, and I can't remember right off the top of my head, and I think I put it in the report, but it was just a few years later.

DR. SERCHUK: Thank you.

DR. NESSLAGE: Excellent. Are there other questions? I don't see any hands raised. Jeff Buckel, go ahead.

DR. BUCKEL: Rob, nice job on this. With respect to estimating natural mortality, Kyle mentioned -- I am just thinking about the research recommendations, and Kyle mentioned tagging, but I wonder how much the BAM -- You mentioned estimating natural mortality within the assessment, and so thinking about research recommendations in the next assessment, do you have a sense of what would improve the estimation of M, or even the likelihood profiling, to zero-in on it a little bit better? That would help us when we get to the research recommendations. In other words, better age comps, or to track cohorts, or --

DR. CHESHIRE: That's a good question. As far as the likelihood profiling, I would probably do that over a finer scale, and I think it was fairly fine already, but I believe age comps would inform that better, or maybe I'm wrong, and I am trying to think what would help with that.

DR. SIEGFRIED: Rob, I can jump in, if you want a little assist.

DR. CHESHIRE: Yes, please.

DR. SIEGFRIED: I think a little bit better information in the indices would likely help, and one of the issues that we have is there is these very highly variable indices that the snowy population probably can't actually realize, but, because of the low sample sizes, that's what we have, and so a better index. However, that will take a while, because the deepwater survey just started, but, yes, better age comps and better index information is where we expect to get that from.

DR. BUCKEL: Thank you.

DR. SIEGFRIED: If I can also add something, and so the scamp and the red snapper assessments are looking at this new study from Lorenzen, and that will help the way of specifying which species are used, and the Then estimator will help with -- It would be nice to take a deeper dive into those data, to see what it can tell us as well. That's it.

DR. NESSLAGE: Great. Thank you, Katie. Church, go ahead.

DR. GRIMES: I guess this is a related question to Jeff's. One of the questions that is posed to us later on is about what would the council want to look at between this update and when another assessment is done, and it seems like you analysts might be best qualified to answer that question. What indices, what indicators and metrics and so on, should the council be interested in trying to monitor? Thanks.

DR. CHESHIRE: Thanks, Church. It's hard to say. As far as the indices, the only ones that are ongoing are the -- Well, we have the deepwater, hopefully, coming online, and that would be the biggest one that we don't really have anything to compare it to, and the trap index is very -- It has very small sample sizes, and the best index, I think, right now, in my opinion, is the MARMAP longline, but it still has issues with spatial coverage and smallish sample sizes, but that seems to capture a little bit better what's going on, and it would capture more fish anyway.

DR. NESSLAGE: Thanks, Rob. Alexei.

DR. SHAROV: I am not sure if we are in the period of offering recommendations, research recommendations, but that appears to be what people were talking about in the last couple of minutes, and so, if that's the time, I have something else to add, or I will wait until we get into the research recommendations.

DR. NESSLAGE: Are you about to make recommendations, you're saying, or -- We're just taking questions right now.

DR. SHAROV: They were talking about what will improve the model in the future, potentially, right?

DR. NESSLAGE: Right.

DR. SHAROV: We have talked, in the last few minutes, about improving the indices and improving M estimates and reviewing the Then paper to weed out the species that are less suitable and not as similar in their biology to snapper grouper. Then the deepwater survey, and, since I am already talking, I will just say that, yes, I think certainly improving natural mortality estimates

would very likely be very useful, but I just wanted to bring the point that we have not touched yet on that this is a protogynous species, and we're mostly concerned about the SSB estimates, and so the bulk of the SSB, female SSB, is in the first fifteen ages, or maybe even ten, and that is the age range where the natural mortality -- We modeled that the natural mortality declines pretty steeply, and that's where it's important whether we have the right estimates.

Currently, we're just using the Charnov curve or the Lorenzen curve, where we just distribute a single point estimate, and, of course, the youngest groups get the much higher estimates, and that's it, and we're not challenging that at all, and so a little change in that steepness of decline will have a huge effect on overall estimation or estimated SSB and a huge effect on whether the Fs are too high or whether the Fs are lower, and so getting any better estimates, probably, from the field on the actual natural mortalities for those first ten to fifteen ages is paramount. That's what I wanted to say. Thank you.

DR. NESSLAGE: Thanks, Alexei. All good points, and hopefully the folks assigned to that are taking good notes. Fred. Are there questions? I don't want to launch into discussion quite yet, and Alexei was veering that way, but let's see if we can wrap up questions for Rob. Fred, is this a question?

DR. SERCHUK: Yes, a question that I have, and I know we've had a number of poor year classes, although the most recent one was very good. Was the recruitment of the poor year classes -- Was that forecast? In other words, how good was the forecast last time relative to what we expected recruitment to be in the most recent years? I think this is an issue that relates back to the robustness of the projections. Thank you.

DR. CHESHIRE: Thanks, Fred. I will point out the one year of good recruitment wasn't estimated, and that was from the stock-recruit curve, unfortunately. As far as the robustness of the recruitment and how that relates to projections, I think you made the point earlier that the stock-recruit curve maybe could be modeled differently, and so, as far as what's in the stock-recruit curve now, projecting forward, I mean, it's hard to say, without maybe doing some runs with different scenarios, maybe, to see what changes in the recruitment -- Like if you change the recruitment, what that would do to the forecast. I think it's definitely worth exploring how robust that is, and maybe somebody else has an idea.

DR. SERCHUK: Okay. Thank you for that. My reason for raising the issue, and we can discuss it later on, is we'll need to provide guidance on the next assessment, addressing its timing and type, and that's part of the research recommendations, and I am reluctant to personally -- We can give these projections, when it would be rebuilt under the current situation, but we've talked about a number of issues related to the assessment, which we think should be investigated, and, rather than -- I think we have to, as a committee, think about, well, are there a number of issues, such that we would like to see an operational assessment sometime in the near future, maybe four or five years, well short of the rebuilding period, because we think that might change how we look at our forecast, how we look at our biological reference points. I think management will want to say, well, why didn't this stock rebuild according to what we thought it was going to rebuild from the last assessment. Thank you. That's not a question for Rob, but it's just a question to the committee. Thank you.

DR. NESSLAGE: And it's a good question. Thank you. Are there other questions for Rob? I don't see any hands raised. Then I am going to take a moment here to see --

DR. LANEY: Madam Chairman, I was late raising my hand there. May I ask a question?

DR. NESSLAGE: Of course. Go right ahead.

DR. LANEY: I am not sure that this one is -- I will ask it to Rob, but, if you think it's more appropriate for a research recommendation, we can push it forward to that section, but going back and considering Fred Scharf's comment about how often we tend to see this same pattern, and also resonating in my brain the discussion from our Ecopath with Ecosim model discussions of last year, would it be useful at all to perhaps pose a what-if question with respect to snowy grouper and its relationship to other species in the complex, similar to what I think we've been discussing with respect to red porgy, red snapper, and red lionfish and the potential interactions between those species?

I guess my perception is, and I will defer to Churchill and our analytical team on this, but my perception is that snowy grouper may be a bit more of a deepwater species and may not interact as much with some of these species as red porgy might, but, anyway, I will just throw the question out there, as to whether or not it might be useful to formulate some questions for the model team to try and answer, and, again, that would probably be more useful for a future assessment, as opposed to this one, but I will toss it out there. Thank you.

DR. CHESHIRE: Thanks, Wilson. I think it would definitely be worth looking at it. It seems to have the same pattern, and maybe not the same timeframe, as some of the other species that you mentioned, but, given what we've seen lately with other species, it seems like it's a possibility, although it is a deepwater species, and so maybe it's more immune to that, and so maybe something else is going on, but, yes, I definitely think it would be worth checking into.

DR. NESSLAGE: Thanks, Rob. Kyle, do you have something to add?

DR. SHERTZER: Yes, and this has come up with a number of species in recent assessments, and the SSC has talked about it, and we've talked about it within the lab quite a bit, and we actually have a project funded, and we're bringing in a post-doc, starting this spring or summer, to sort of evaluate hypotheses about what's driving this low recruitment in several of the snapper grouper stocks that we're seeing. If the SSC has any input into that, or different hypotheses that they would like to see evaluated, that would certainly be welcome, and we'll try to address that with the work that the post-doc is going to do over the next couple of years.

DR. NESSLAGE: Thank you, Kyle. Are there other questions? Erik has got something to add. Go for it, Erik.

DR. WILLIAMS: Just to sort of follow-up on Kyle's and Wilson's comments, which are good ones, and just something to sort of keep in mind, if we step back and look at the bigger picture, and I know a lot of these issues come up when we look at individual stock assessments, but there is clearly some things that are growing, in terms of sort of data modeling and information, that we're collecting from the science world that has bearing at the ecosystem level.

The question then is how, as an SSC, are we going to -- Well, not we, but you guys going to sort of tackle that information, and are we going to look at this when we come up with these individual stock assessments and try to tap this information into the assessment, or are we better off approaching this information from another sort of way, as sort of a holistic ecosystem report kind of thing, and it just happens to include say snowy grouper.

That's just sort of something that I've been sort of thinking about, is where are we going to go with, for instance, if the results from the analysis that Kyle mentioned indicates some sort of environmental driver of recruitment for several of these species, and, well, we can attempt to bottle that in an individual stock assessment, but it does have sort of implications at a larger scale for the whole ecosystem, and then the question is how does that then feed to the SSC and get reviewed and then work its way into management advice, and even ABC setting, and that's just something f you guys had time on your schedule to sort of think about from a holistic point of view.

DR. NESSLAGE: Thanks for that, Erik. It's a good point, and we could start thinking broadly. Wilson.

DR. LANEY: Thank you, Madam Chairman, and so the first thing I was going to say is, wow, it's great to hear that the Center is hiring a post-doc to work on that, and that's exciting news, and I think it's a great way to proceed. Then, with respect to Erik's question, that's a good one, and I don't know that -- I mean, I think -- Genny, correct me if I'm wrong, since you kind of provided the SSC's input on the EwE model, but I think that we all kind of agreed that the model, even though it's come very, very far, it's not quite ready yet for -- Well, it's ready for the model team to begin taking questions that will give us some insight into the relationships between these different species and how that might explain the patterns that we're seeing in the individual species stock assessments.

Whether you could formulate a question that was specific enough, Erik, that would make it useful to include within a single-species assessment, or whether, if we have a bunch of questions, it's better to just cover those in a separate report that looks at the whole complex, I don't know. I mean, I could see utility in either direction, depending on how many questions you ask and how specific an answer the model might be able to provide to you.

One thought that occurs to me, Erik, is that, if the SSC could come up with a set of sort of generic ecosystem questions for the South Atlantic snapper grouper complex, then that might be something that could be worked into the Center's Southeast Ecosystem Status Report that they're working on right now, and, obviously, we couldn't do that for the present one, but certainly maybe for future ones, and that would be a good place to try and put the results of any sort of EwE analysis.

DR. NESSLAGE: I think the council has already -- That's already on the council's radar screen, this issue of low recruitment in the snapper grouper complex, and I think that was one of the prioritized potential options for pursuing further development of the EwE model after the last council meeting, and so I'm guessing, depending on where that working group and the ecosystem analyst group goes with that, that we might be seeing a lot more of that, Wilson.

All good discussion. Any other questions? I keep asking, and you keep coming up with good ones, and so if you have any other questions for Rob or Katie or Kyle or Erik. No hands. Okay. We have exhausted our questions for the moment, and I'm sure we'll have more as time goes on.

I would like to take a moment, at this point, to see if there are any public comments. If anyone from the public has something they would like to contribute, if you could raise your hand.

DR. ERRIGO: I am not seeing any hands at this time.

DR. NESSLAGE: No hands. All right. Thank you then. So we're at 11:30. I assume people will want to break for lunch sometime in the next half-hour or hour, and I would like to have the SSC start to have a general discussion of our action item questions, and folks who are assigned to each of these sections should be -- Hopefully you've been taking notes so far, and a lot of good points have been made that can address some of these questions.

I guess, as we're going through each -- Well, let me take a step back. I am thinking, if we could go to 12:30, we could get a good bit of discussion in and come back -- Well, let's see how far we get, but take an hour for lunch and then see if we're ready for breakouts or whether we need to wrap up the discussion and then break out, and does anyone have major concerns with breaking at 12:30? It's okay if you do. No hands. Okay.

So let's talk, and, if we finish before 12:30, that's great, but with an aim to end and to take lunch at 12:30. Let's start going through our action item questions, and so our favorite questions, when it comes to assessments, are always the first ones, the very standard ones of does the assessment address the TORs to the SSC's satisfaction, and does it represent BSIA, and does it provide an adequate basis for determining stock status and supporting fishing level recommendations?

We've had some general discussion around some ways in which the assessment could be improved, and there's always ways to improve an assessment. Let's keep in mind though that this is an update, and so, if there are recommendations that folks have, where they feel improvements could be made, is it critical, such that we need to address it in this section, or is it something that could be put in the later section for research recommendations? I will open the floor to Fred Serchuk.

DR. SERCHUK: Sorry, Chair, but I had another question related to recruitment, but I will withhold it until we come back to it.

DR. NESSLAGE: Well, if you -- At this point, we're starting to get -- Unless you think it pertains to a specific one of these questions. If it's generic, just go ahead right now.

DR. SERCHUK: My question was, Chair, that the report talks about low recruitment, but, when I look on page 72 of the report, the recruitment that has been estimated in 2018 and 2019 from the model suggests that the last two years of estimated recruitment are actually not poor. I am just wondering whether, in the projections, we could have another projection using that recruitment going forward. That's on page 72 of the report, Chair. Thank you.

DR. NESSLAGE: Was that last point actually estimated? I am not sure what's being graphed here.

DR. SERCHUK: It's a table of output from the model. Those last two points are provided at the end of the table.

DR. NESSLAGE: Rob, go ahead.

DR. CHESHIRE: The 2019 -- The model went through 2018, and so that 2019 year is the one sort of projection year at the end, but the 2018 and 2019 values are -- They are not estimated, and those are from the stock-recruit curve.

DR. SERCHUK: I am looking at Table 6 on page 35, and it says, "estimated total abundance at age", and both the 2018, which is the 2017 year class, and the 2018 year class in 2019 are given as 196. I presumed they came from the model output. It's page 35 of the report.

DR. SIEGFRIED: They are fixed to the stock-recruit relationship, and we don't allow any deviation in the terminal year, and 2019 is a projected year, and so it's estimated in terms of it's estimated from the stock-recruit, but we don't allow it deviation.

DR. SERCHUK: The same thing is true for 2018?

DR. SIEGFRIED: Yes.

DR. SERCHUK: Okay. Thank you for that explanation. Sorry.

DR. NESSLAGE: No worries.

DR. CHESHIRE: Those are adjusted for the forecasts, though.

DR. NESSLAGE: You mean which recruitment period you're using?

DR. CHESHIRE: I mean, for the age-ones and twos going forward, they don't assume that those values came directly from the spawner-recruit curve, at least for the stochastic forecasts. They go back in time and pull off the ages that would be forecast forward and apply the recruitment variability to those values.

DR. NESSLAGE: Right. Where I think Fred was going, and, Fred, correct me if I'm wrong, is that you were concerned that perhaps there was evidence that there's been a recent spike in recruitment over the last two years in the model, and therefore we should consider a higher recruitment in the projections, and I guess, in hearing the response, perhaps this is already more uncertain than you might like to use in those projections, given -- I will stop putting words in your mouth, Fred, and are you fine with the set of projections now?

DR. SERCHUK: Yes, but it's just, if you looked at the figure, and you looked at the table, one would typically think that this came out of the model output, just as any other age-one index was -- Age-one stock size was provided, and so, if one just naively looks at Figure 11, you see that, well, we've had some low recruitment, but the last two years were sort of back to quite high, but, if those are just coming from a stock-recruitment and not from the model output, per se, then they're not model-produced, and they are just estimated without any data behind it. Is that correct?

DR. SIEGFRIED: I think what we can do in the future, to make it less confusing, is to put that -- Put the years that are estimated, versus fixed to the stock-recruit curve, in the figure and table

legends, as well as in the text, because you do have to kind of pour through everything to get that point, and that's well taken, Fred.

DR. NESSLAGE: Great. Okay. Now that that's clarified, let's -- If we could go back to the question of do we feel that the assessment addresses the TORs appropriately, given it's an update assessment? I didn't hear anything that seemed to indicate that there was any grave -- Anything overlooked here, but, if anyone has anything they would like to bring up at this point, this would be the time.

DR. SERCHUK: The simple answer is yes.

DR. NESSLAGE: Fred said yes. I am not hearing any no, or at least I am assuming that there's no hands raised. I am not seeing any, and so this is a quick and easy one then, it looks like. Yay. Good job, Rob and Katie et al. BSIA. Does anyone feel that this assessment does not represent the best scientific information available, with the caveat that of course we will provide lots of feedback on ways that it could be improved in the future later in the report? Anne Lange.

MS. LANGE: I do believe this is the best scientific information available at this time, along with that caveat that we'll address concerns or suggestions later on.

DR. NESSLAGE: Excellent. Does anyone disagree with that? If so, raise your hand, please. No hands raised. Outstanding. Thank you, all. Then, of course, if we believe with the first two, we kind of, possibly, would agree with the last, does the assessment provide an adequate basis for determining stock status and supporting fishing level recommendations, and does anyone have concerns about that? We have discussed a couple of things, regarding Beverton-Holt, et cetera, but if it rises to the level of concern at this point, versus fixing it in the future or exploring alternatives in the future, and that's what I would like to hear from folks. Alexei.

DR. SHAROV: Just briefly, I think I mentioned before that the results are a little bit unsettling to me to be easily accepted. I think there is quite a bit of uncertainty in terms of interpretation of the stock status, and my gut feeling is that it might be overly conservative. I have a difficult time believing that the SSB over SSB MSY has been that low for that long and that the F over F MSY has been that high for several decades and the stock has not collapsed and seems to be actually simply stable. These sort of general points make it difficult for me to accept the conclusions on the status of the stock as the best scientific information, although I agree that it's within the realm of possibility as well.

DR. NESSLAGE: So I hear you, Alexei, and I think it's a very valid point. I am wondering though if this is part of our broader report discussion regarding uncertainty, as in, yes, we feel this is the best snapshot at this moment, but recognize, council and stakeholders, that the situation may actually be different, for these reasons, due to these uncertainties, which we would list later in the report, and do you feel, at this point, it rises to the status of, really, that you don't trust the stock status to use at this point for management? That's what this question is asking.

DR. SHAROV: Are you asking me or the group?

DR. NESSLAGE: Yes.

DR. SHAROV: Well, I guess, the way the report interprets the results, or I guess the way the results turned out and the following interpretation is at least conservative, and that is, from what I see, my concerns were that we were -- The advice and the interpretation was that the stock was doing better than what the assessment tells us right now, but, in that sense, if that is the bias that we're potentially making, at least that's on the conservative side and not on the risky side, and so, if the group feels that this is the BSIA and all other concerns should be explored through the research track later, at that time, I certainly will agree and support the consensus, but I just felt it was my responsibility to point out the uncertainty here.

DR. NESSLAGE: Okay. We're getting some comments and questions. Kyle, go ahead.

DR. SHERTZER: Well, I guess, do you want non-SSC members to chime in on these topics?

DR. NESSLAGE: How are you planning to chime in?

DR. SHERTZER: I just wanted to give some more context on Alexei's comments.

DR. NESSLAGE: Sure. Go ahead.

DR. SHERTZER: Okay. About it being on the conservative side, maybe, but the assessment also has an M that assumes a max age that's quite a bit lower than the actual observed maximum age, which was something like eighty years old, and so, if M were actually lower than the assessment uses as a base run, then it's not on the conservative side, and it's more aggressive.

On the topic of stock status through time, the overfished status across the whole time period, that's really dependent on that initial F that Rob showed, and it seems to be supported by the data, at least from the profiling, but, if F were -- If the initial F were assumed to be much lower, like it was in previous assessments, then you would see that same pattern of declining abundance early on in the time period, but then it converging to where it is in the latter half of the time period, and so I think that uncertainty seems to be much greater in the early part of the time series than it is in the latter half of the time series.

DR. NESSLAGE: Thanks for that, Kyle. I appreciate it. Anne.

MS. LANGE: I was just going to say that I think that the assessment is adequate, with the caveat that, later on, in our review, we'll have additional information that will help the council determine the uncertainties and understanding the uncertainties better, and so I think that's captured in the overall assessment, the level of uncertainty, and that we just need to make sure that we quantify that as best we can.

DR. NESSLAGE: Good point.

MS. LANGE: For our advice to them, I should say.

DR. NESSLAGE: Alexei, can you live with that?

DR. SHAROV: Yes, sure, and I understand, and any assessment update is then adequate, because it builds on the previous assessment and just extends it by X number of years, and, because it's

constrained from the very beginning, to the formulations that were used in the previous assessment, and so we are constrained, and we are supposed to look at it as, well, here it is, given all these assumptions, and, no, you're not allowed to change them, and is this the best interpretation, and, yes, it is. We certainly thoroughly reviewed and see that some interpretations and some assumptions should be revised or reviewed, but not within the update, but beyond that, and that's what will go into just research recommendations or additional comments, and so, yes, I agree.

DR. NESSLAGE: Well said. The breakout group can work with the wording on this, but does anyone have anything to add to kind of our discussion so far on this first review assessment bullet? I am going to go a little more quickly through the discussion, just because I think we're not going to try and hash out all the wording here. Okay. Thank you.

The next bullet has to do with our favorite topic, to identify, summarize, and discuss assessment uncertainties, and we've discussed several already, and hopefully the folks -- Again, I want to try and streamline this a little, and folks have been taking good notes, I hope, and a lot of good ideas and topics have been raised, and concerns, and I would like to hear anything new that folks have to say, and so if there's something that wasn't a question, but you have a comment or some additional concerns about uncertainties that have not been raised so far, I would like to hear what those concerns might be. This is factors that affect the reliability of estimates of stock status and fishing level recommendations, and let's start with that. Is there anything that hasn't been raised yet that you would like to raise? Okay. No hands.

I think we've hit on a large number of major topics, and who is in this identified group? It's Wally, Yan, and Fred, and so, if you don't think you have a good handle on it, let us know, but I think we've had some good discussion, and hopefully you've captured that and can turn that into some strawman language.

Risks and consequences of the assessment uncertainties with regard to status and fishing level recommendations, we've talked a little bit about, depending on how M -- How well estimated M is, and we could be being under or overly conservative, and we may be in a new low-recruitment period, and there's a number of different things that have come up, and is there anything new that folks would like to add? Fred Serchuk, go ahead.

DR. SERCHUK: I am just wondering whether it's possible to make a differential between short-term recommendations and long-term recommendations, given that many of the issues that were brought up are -- The developments were constrained by being an update assessment. For example, I think there are a number of issues that were brought up that suggest that we probably shouldn't wait more than four or five years before we do a more thorough assessment, and, therefore, I would -- That one might change the long-term projection for stock rebuilding, and I think they would. I'm just wondering how far -- When we say fishing level recommendations, are we talking about our long-term projections as well?

DR. NESSLAGE: I believe so.

DR. SERCHUK: My personal opinion would be I would be very cautious to follow our long-term recommendations.

DR. NESSLAGE: Well, you have me confused. What are our -- Short-term is just stock status, right, and then you're saying projections are long-term recommendations? Is that how you're --

DR. ERRIGO: It says fishing level recommendations, and, the fishing level recommendations, are they just short-term, or do they go through the whole rebuilding -- To rebuild the stock?

DR. NESSLAGE: I see where you're going. Right. Because this has a rebuilding plan, and we have to plan out to 2039.

DR. SERCHUK: Right, and I am saying that so many -- There have been a number of issues that have been raised about uncertainties in the assessment, and that suggests to me that, although we have combined those, perhaps, they are likely to be changed if we address some of the issues that weren't able to be addressed, because this assessment was just an update.

DR. NESSLAGE: Thus, that might change our ultimate -- Sorry, Fred.

DR. SERCHUK: If we want to raise that, would that be something that would affect our, quote, fishing level recommendations?

DR. NESSLAGE: Yes, I could see that, because, typically, we'll do five years out, but this one, because it's rebuilding, we have to think about what's the probability of achieving 2039, right, and so we might need a check-in sooner. Is that what you're saying?

DR. SERCHUK: Yes.

DR. NESSLAGE: Yes. I hear you. I think that would definitely fit in here.

DR. ERRIGO: Usually, for a rebuilding stock, we have settled on projections that are rebuilding projections, and so you have to project out to the end of the rebuilding time period, to figure out what the rebuilding projections are, and then we also look at, okay, do we want to follow those same projections for the next few years and then reassess, or do we want to come up with a different set of recommendations for the next few years that are at or below that and then reassess, like we did for red grouper?

Remember that, for red grouper, we -- The SSC recommended a more conservative set of projections than the actual rebuilding projections, because of recruitment, and so you can do that, or you can say, okay, yes, here's the rebuilding projections out to 2039, but we're going to give projections only out for five years, and then we recommend an assessment of some -- Whether it be -- It's not standard anymore, but operational, an operational assessment, let's say, at the end of five years.

DR. NESSLAGE: I think -- Hopefully folks can capture that concern that Fred raised under this describe the risks bullet, with regard to planning far out to 2039, given the number of uncertainties, and then, perhaps, when we get to the fishing level recommendations section, we can talk more about exactly how far out we would like the ABCs, recommended ABCs, to go and what the comment on rebuilding and stock assessment frequency is, and does that sound like a plan? Then we can discuss those options that Mike laid out. Would that address your concern, Fred, and it's a good one?

DR. SERCHUK: It would, and I am thinking, if Alexei doesn't mind, he would be welcome to participate in our chat group, since he felt very strongly about some of these things.

DR. NESSLAGE: So, unfortunately, Alexei has a meeting from 1:00 to 3:00, and so we're going to have to deal with that. When are you gone, Alexei?

DR. SHAROV: Sorry about that, but it's just another competing meeting.

DR. NESSLAGE: That being said, hopefully you'll be back by the time we reconvene and start re-discussing this, but I'm afraid we have to keep moving, and so, if there are specific things, other than what you've already said, Alexei, that you would like to see -- I mean, how do you feel about how long we should have ABCs going forward and the assessment frequency, given the rebuilding?

DR. SHAROV: I honestly have no immediate recommendation, and I think it would be better decided by the group.

DR. NESSLAGE: All right, and so that's a good -- I'm glad, Fred, that you brought that up.

DR. SERCHUK: I have one other comment, Chair, if I could, about the uncertainties.

DR. NESSLAGE: Sure. Go ahead.

DR. SERCHUK: Under any circumstance, the fishing mortality rates that we're going to be recommending are going to be less than natural mortality, for most of the age groups in the fishery, and I think we have to point out that, because fishing mortality is going to be 0.08 or whatever it is, for those age groups that are likely to be caught, the natural mortality we have assumed are constant for these ages, but, if it changes, it will have a significant impact on realizing whatever catches or whatever rebuilding we anticipate.

I mean, F is almost zero, for most of these cases, 0.08. It's far below the natural mortality rates for those first seven or eight or nine or ten age groups that constitute most of the catch, and I think that's a source of uncertainty, because, if natural mortality is not constant at age, or is not constant at the ages we assume, and it changes, our projections are going to be way off. Is that correct? Maybe this happens any time F is going to be very much lower than M.

DR. NESSLAGE: I will defer to Rob or Kyle, but I think it's worth mentioning. Would either of you like to comment, or Katie, or anyone from the Beaufort Lab? Katie, you're not at the Beaufort Lab. I don't know what's going on, but, if you all have a comment on that, that would be great.

DR. CHESHIRE: I don't know if we have -- If we have done that before, where we limit the F for the projections to certain ages, and I would defer to Kyle or Katie or Erik on this one.

DR. SIEGFRIED: I'm sorry, everyone. My VPN keeps going in and out, and I just was able to log back on, and what was the question again?

DR. SERCHUK: The projections that are done have been done at F of zero, and I think it was an F of 0.08 and so on and so forth. For most of the age groups that constitute the bulk of the catch, certainly from ages-one to fourteen, or to thirteen, the F value, and let's say it's the 0.08 that we're recommending, is going to be less than M, and that's fine, but we have to recognize that the M values at-age, although we assume they are constant, and that is we assume that we have taken out M and are decreasing it across ages, and, if M is not constant at the values we assume, then the realized projections are not going to be as certain as we thought they would be, and we may not get the results we thought. Would that be a fair statement, as an uncertainty?

DR. SIEGFRIED: So you're asking if we have time-varying natural mortality that differentially affects the younger ages, where the F is very small, but the M is very large, and, if we didn't account for it, that would change our projections?

DR. SERCHUK: Correct.

DR. SIEGFRIED: We don't split out the Fs in like the F rebuild to F at 0.08, and it isn't necessarily split out across ages in our projections, and so I'm not sure what the direction of that effect would be, and I'm sure you right that there would be an effect, but my mind is a little bit blown at the idea of time-varying natural mortality for this stock in the first place.

DR. SERCHUK: Okay, but what I'm saying is, for F as low as 0.08, for almost all the catch in the landings, or removals, in the stock, from ages-one to thirteen, M is higher than the F that we are applying, and, therefore, M will have a much greater impact on what survives than the F will, because it's a larger value. That's all. It's not a case where we're saying M is 0.2 and you can fish at 0.4 or 0.5.

DR. SIEGFRIED: Right.

DR. SERCHUK: Maybe I am making too much of this.

DR. SIEGFRIED: I don't think that you're making too much of it. I mean, we brought M to the SSC earlier, because it is a difficult topic, and so I think that -- I mean, one of the things that I look at in the MCB ensemble runs is that there is so much uncertainty and that the median result is slightly different than our base run, and that would be interesting for the breakout group to take a look at. That doesn't directly address your question, and we haven't done the work to directly answer your question, but I think the direction that you're talking about is right. I just don't think we have anything additional to show you to help resolve the question.

DR. NESSLAGE: I think, more broadly though, that's a bigger issue even than just the projections, although you're bringing this up because we're talking about fishing level recommendations, and just assessment uncertainty for an assessment, for a stock where the Fs are getting that low, is going to start to skyrocket, just based on some of the simulation studies that are out there, and so I think it's a broader topic that could be raised under uncertainty in general. Hopefully folks can capture that. Chris, go ahead.

DR. DUMAS: Fred, if you're saying that you're concerned about the size of M relative to F for this species, or this stock, are you saying that you have that concern because that makes this stock different from other stocks that we do assessments for, or other similar stocks, or different from

deepwater -- Other deepwater stocks or other sort of relatively rare-catch stocks, and is that why you raise this concern here, because you think the relative value of M of F is different for this stock relative to others, and so we have a different effect on the stock assessment and projections for this species relative to others?

DR. SERCHUK: No, that's not the reason at all, although my point is these characteristics could be shared by other long-lived stocks. My point is, in terms of the reliability and accuracy of the projections, when the driving force, in terms of mortalities, is M, and M could be considerably larger than F, then the assumption of constant M at-age becomes a bigger factor, and that's all.

DR. DUMAS: I understand that, and I was just asking about whether you had that concern for this particular stock or in general, and so I understand now that your concern is general, and so my next related question is do you have an increased concern now that M might change significantly, going into the future, and that that concern is larger now than it was in the previous stock assessment for this species, or the previous update? Maybe you have a concern about changing environmental factors and that you think now it's more likely that M could change, compared to sort of past assessments of the projections, implicit projections, of M from past assessments for this species?

DR. SERCHUK: No, and I don't have any particular concerns in that regard, and I'm just making a general observation. First of all, the measuring F at very low levels, even estimating F at 0.08, becomes a difficult issue, when the values are so close to zero, but, more importantly, I'm just making a general comment that, when you have very low Fs in relationship to M, what's controlling the stock dynamics is largely not human-induced. That's all.

DR. DUMAS: Fred, I agree with you completely, and I was just trying to understand whether you had any additional information that you were thinking about as you made that comment. Thank you. That's all.

DR. SERCHUK: Okay. Thanks.

DR. NESSLAGE: Thank you both. Alexei.

DR. SHAROV: Just on this, to maybe hopefully finishing that discussion, exactly how I hear Fred is making the point that, unlike many other species here, we are looking at a case where M is a higher fraction, or major driving force, relative to F, and, therefore, we are much less certain in the outcome, and will the projections be different from what we will see five years from now? Absolutely yes.

I mean, how many projections do we have that we have done in the past that were very successful or that the re-estimated population trends went exactly as we predicted? Even for the best species, I guess, there are not that many cases of that sort, and so certainly it will be different, but, within the framework of this assessment, that's what it says you have to keep the F at in order to achieve the result that we want, and so, theoretically, within this model, that is what is required. Realistically, most likely, we will see something different, but Fred is right, in terms of the driving force being larger natural mortality, larger than the F, and then, yes, if it's variable, then it will drive the dynamics and not the cumulative removals. Thank you.

DR. NESSLAGE: Thank you. That's a good point, and hopefully we can capture that in the wording for this response. Are there any different other concerns that folks want to raise under this particular bullet that have not already been raised? Okay.

The next bullet is about have they addressed uncertainty in a method that's consistent with SSC expectations and the available information, and I think we have some suggestions for them moving forward, but this is essentially a did they do the best they could and is it adequate for us to use, and so does anyone have any major concerns that they would like to insert here?

DR. SHAROV: With respect to what?

DR. NESSLAGE: This is about methods for addressing uncertainty in the assessment. Is it consistent with our typical expectations of what they would produce, given the available information? I am going to go out on a limb and say yes. I'm assuming, when we get to that part of the ABC control rule, we will stick it in the -- We've got some comments. Chris, go for it.

DR. DUMAS: A brief comment. I would just like to say that I commend the move the zero-inflated negative binomial distribution, using that, and I think that's a better way to go about things, and I'm glad that we're moving in that direction, and I think that's great. Thanks.

DR. NESSLAGE: Right, and that's for standardizing the indices. Good point. Thank you, Chris. Katie.

DR. SIEGFRIED: I just wanted to ask if it's clear that the way that M, the uncertainty in M, was accounted for and how it carried through the projections, because some of the previous discussion led us to believe that maybe it wasn't clear, and Fred is on the list of speakers, and maybe he could bring up what he's going to bring up and then I could respond, or I just want to make sure everybody knows how M -- How the uncertainty in M was accounted for and carried through the projections.

DR. NESSLAGE: Fred, was that what you were going to bring up, or was it something different?

DR. SERCHUK: I can touch on that. I have no problems with the way it was done. I am just -- Carrying the M so that you could reach the terminal age of fifty-six was fine, and I have no problem with that, but one should recognize that, to extend it through an age of fifty-six, it means that M at the earlier ages is going to be higher than they were at the older ages, and those M values at-age are given in the document, and what I am suggesting is that age-one, two, three, four, five, six, seven, eight, nine, ten, eleven, and twelve are all above 0.08, which is one of the recommended levels of fishing mortality, and so the Ms at-age, for those age groups, does that constitute the large portions of catch, and will it constitute a greater fraction of total mortality than the F? That was my previous point, but I have no problem with how the new M was used and then scaled to an age of fifty-six. That was done very well, and I have no problem with that. Thank you,

DR. NESSLAGE: I don't know that we have -- Katie, I don't want to create problems where there aren't any, and so, unless you have something burning that you really wanted to talk about regarding that, I think folks were comfortable with it, and I don't remember -- I mean, I raised the question of a non-uniform distribution, but it sounded like you guys couldn't estimate it, and so

we're kind of stuck there, and so we're still at BSIA, or am I misunderstanding your question, Katie?

DR. SIEGFRIED: No, and I'm good. I'm cool, and I just wanted to make sure that nobody else was burning.

DR. NESSLAGE: Good. Excellent. Okay. I am not seeing any other hands raised here, and so the next bullet is this always complicated one of list, in order of the greatest contribution to risk and overall assessment uncertainty, and comment on the effects of those assessment factors that most contribute to risk and impact status determinations and future yield projections. These are really complicated tasks. Okay.

DR. ERRIGO: This one is more of -- You have already gone through the uncertainties, and you have already hashed them out. This is just asking which ones are the most important, which ones have the highest overall effect on the assessment uncertainty, and so, given there's a total amount of uncertainty, because of these five factors, which one attributes the most to the uncertainty, and then which one next? That's what this is asking, and it's just so that, in future assessments, we might know which of the factors to focus on the most to try to reduce uncertainty.

DR. NESSLAGE: That makes sense. Okay. Thank you. What I would like to do is -- Since hopefully everyone has already raised their issues regarding major assessment uncertainty topics, I am going to task the breakout group with coming up with a strawman order for those, and then we can hash it out as a group. If we want to reorder later, that's great, after everyone gets a chance to see their suggestion, and hopefully everyone is -- If there's anyone who is really not okay with that, let me know now.

DR. SERCHUK: I have one comment, Chair. Can I make it?

DR. NESSLAGE: Of course.

DR. SERCHUK: From the discussions, it's clear that, from the number of issues that have been raised, that the terms of reference were -- I don't know whether you would call a benchmark or an operational assessment, but they're needed in the next assessment and not an update. Whether those terms of reference address all the issues that were covered as shortcomings or uncertainties in this assessment, but they should be covered by the types of questions, or the types of TORs, that we have for an operational assessment. That would be my feeling, in a very simplistic way.

DR. NESSLAGE: Good point, and I think we need to put, somewhere in our report here, a reminder to ourselves, because we go back -- I go back and look at these when we review TORs, and so I think we need to leave a very clear breadcrumb trail for ourselves next time we're reviewing our statements of work and TORs for this, when this comes up again in the assessment schedule. Good point, Fred.

Let's move on. We have a few minutes left before the lunchbreak, and let's move on to fishing level recommendations. Is it possible, Mike, that we could run through the control rule and see how things --

DR. ERRIGO: Yes. We should be able to do that. Hold on.

DR. NESSLAGE: Excellent. Did you want to walk us through it, Mike? You're so good at it, or did you want me to?

DR. ERRIGO: I can do that. Let me just get the other sheet here on my other screen, so that I can take the notes on how everyone answers. Okay. The first one is the assessment information, and so that has to do with the type of assessment and the type of benchmarks that were estimated. We typically -- Let's see. Steepness was not estimated in this, and it was input, and so, oftentimes, when that happens, the SSC falls on Number 2, but that's up to the SSC. We would not go down to Number 3, and it would be between 1 and 2, but, because steepness is input, it kind of -- It affects your MSY benchmarks, and it's not fully estimating the MSY benchmarks, and, therefore, we kind of look at it as a proxy.

DR. NESSLAGE: That makes sense. Does anyone have any concerns with selecting Option 2, given that justification, or have any other comments on assessment information? Fred Serchuk.

DR. SERCHUK: Sorry, Chair, to be a bugbear about this, but the assessment does depend on MSY-derived benchmarks from the stock-recruitment curve. Now, to be sure, it's correct that the steepness was input, but they're not proxy reference points, in my view, and they're not an F 30 percent or 40 percent, and they actually come right out of the stock-recruitment curve, and I mentioned this earlier on. Maybe we need another category within this, but my feeling is that, okay, we're someplace between 1 and 2, but they're called MSY-derived points. All throughout our report, it's B over BMSY, and it's SSB over SSB MSY, and it's F over F MSY. Thank you.

DR. NESSLAGE: That's a good point. Go ahead, Mike.

DR. ERRIGO: I agree with Fred that there may need to be another category, if you're going to use categories like this, because it's not exactly like using a proxy. However, if you're using the stock-recruitment relationship to estimate your MSY benchmarks, and you fix points in your stock-recruitment relationship, you are not exactly estimating your MSY benchmarks, per se, and they're reliant on wherever you are fixing steepness, and so it doesn't exactly get you to 1, but it's not exactly 2 either.

DR. NESSLAGE: So we're in the middle ground. That makes sense. Kyle.

DR. SHERTZER: It's because the MSY benchmarks need to be thought of as being conditional on the assumed steepness, and it's also somewhat similar to choosing a proxy, because, if you set a proxy at say 30 percent, then that implies a steepness value. You can back-calculate what the steepness value would be to support that proxy being equal to FMSY, and so, in a sense, these are sort of two equivalent approaches to the same problem, either fixing steepness or choosing a proxy.

DR. NESSLAGE: Thanks for that, Kyle. That makes sense, and that really makes me want to lean more towards 2. Keep in mind that it's more than just the MSY in this part of the control rule, and it also has to do with quantitative assessment, and it provides estimates of exploitation biomass, which it does, but I am babbling. Does anyone else have strong opinions about 1 versus 2? Amy.

DR. SCHUELLER: I was just going to say I think that we need to select 2 here, to be consistent with what we've done in the past. If steepness is fixed, we often, by default, pick 2, because, to me, Number 1 means that we're estimating MSY, and we're not.

DR. NESSLAGE: Maybe this is -- I am making a note to self that, when we're going through our ABC control rule discussions next time, that we suggest that the wording perhaps even just be modified for 1 versus 2, to clarify that, if that's really what we mean. Anyone else? Fred, can you live with 2, or is there anyone else who disagrees?

DR. SERCHUK: I can absolutely live with 2. I have no problem with it, but I just wanted to raise the point nonetheless. Thank you.

DR. NESSLAGE: It's a good point. Thank you, and it brings up a broader issue that we need to deal with, with regard to our control rule, and since we're going through a big amendment, we might as well. Thank you. Okay. Hearing all that, and perhaps folks can capture that a little bit in the breakout group too, and uncertainty. Mike.

DR. ERRIGO: The next one is uncertainty characterization, and complete characterization includes characterization of environmental conditions, which I don't think was done here, and so uncertainty in the environmental conditions, which I don't think we did here, and we typically, for the MCB analysis done at the Beaufort Lab, fall on high, which reflects more than just uncertainty in future recruitment with that uncertainty in natural mortality and a bunch of other key factors, and it's more than just sensitivities, but that's up to the SSC, but that's typically where we fall on this type of assessment.

DR. NESSLAGE: All right, and so this seems pretty straightforward to me that this would be a 2. Does anyone disagree? It's fine if you do, but I would like to hear from you if you do. No hands raised. Excellent. Stock status. I believe we're locked in here, correct?

DR. ERRIGO: The next two are pretty much answered for you. Hopefully, in the next iteration of our control rule, they will go away, but the stock is, if I remember correctly, both overfished and overfishing is occurring, which leads us into Category 4 here.

DR. NESSLAGE: Yes. What is our PSA? I didn't look that up. I should have looked that up.

DR. ERRIGO: I pulled that up here, to look it up. Give me a sec while I scroll through and find snowy grouper.

DR. NESSLAGE: It's in red.

DR. ERRIGO: It is in red. It's a high.

DR. NESSLAGE: It's a high. Does anyone disagree with it being a high risk? Given all this uncertainty in M, I would hope --

DR. ERRIGO: That puts us here.

DR. NESSLAGE: Yes, exactly. Okay. So we're proposing then that stock status be -- That would be 4, and Productivity and Susceptibility, Number IV, would be a 3, high risk.

DR. ERRIGO: Correct.

DR. NESSLAGE: Is there any concerns with that scoring of the ABC control rule decision tree? I think, under the comment on any difficulties, we could probably bring up the issue that we had to specify steepness, but, otherwise, it seems pretty straightforward. I am going to hold off on that last bullet about is adequate rebuilding progress being made, because I think that's going to be a bigger discussion for after lunch.

DR. ERRIGO: Now for stock needs to rebuild, what we typically do is, instead of subtracting the adjustment score from 50 percent to give you the probability of overfishing, you add it to 50 percent, to give you a possible probability of rebuilding, and you can recommend that to the council, which would be --

DR. NESSLAGE: 77.5, or 72.5. Sorry. Okay. Thank you very much for walking us through that, Mike. At this point, I want to put a pin in the discussion and go to lunch. If we could all take a break and come back at 1:35, I think we can finish up our general discussion, and then we'll do our breakout groups and then reconvene and finalize the consensus statements and then move on from there. Any concerns with that plan? Hearing none, we will reconvene at 1:35. Have a great lunchbreak, and thank you all for your work this morning. I appreciate it.

(Whereupon, a recess was taken.)

DR. NESSLAGE: Thank you all for being so timely. I appreciate it, and I hope you had a nice, refreshing break. Before the break, we were talking about the ABC -- We went through the ABC control rule, and that suggested a rebuild of 72.5 percent, and then we ended there. We did not talk about the last two bullets.

Well, first of all, I should probably stop and ask if there is any last comments or questions about applying the ABC control rule before we go on to deciding that that is our -- I would like to decide for sure, 100 percent, that that is going to be our P rebuild. If there's any concerns, let's hash it out now, because the rest of the discussion relies on that. No hands. Okay.

So everyone is still onboard. I felt like we were on the same page before lunch, but maybe you thought something different over lunch. Okay. That's good. Did we have any difficulties in applying the control rule? Is there any information not available? I think we had some discussion about clarifying our first step in the control rule, and I have made notes about maybe we can work on that in our other projects, but, as far as snowy is concerned, it seemed pretty straightforward, as opposed to some of our other species, where perhaps we disagreed with the PSA evaluation, et cetera, et cetera, and is there anything that anyone would like to add to this, any difficulties you felt that we encountered? I don't feel like it was too difficult, myself, but you may feel differently. I am not seeing any hands, and that's good.

Then the last bullet. Is adequate rebuilding progress being made? Comment on reasons why progress differs from projections, and so this is a big question, I feel. We talked about a number of different things, including uncertainty and the magnitude of the assessment, based on various

assumptions and particular M, relative M and M relative to F, as well as also the issue of recruitment declining over time. Are there other things that folks would like to bring up in this section? Fred Serchuk, go ahead.

DR. SERCHUK: Thank you, Chair. I wonder whether we could accentuate a positive thing, and that is one of the things that's clear from the report is that SSB is actually -- It has been rebuilding between 2000 and 2018, and it has actually almost doubled. Now, it's far below the targets, but the fact is that that's a positive sign, and we don't see that any place in our report.

DR. NESSLAGE: That's great.

DR. SERCHUK: You can see that on page 74 of the large assessment report, in the figure. Actually, it's been rebuilding since about 1994.

DR. NESSLAGE: I think, and correct me if I'm wrong, Fred, or anyone else, that page 74, when you look at the abundance at-age, it looks like there is some expansion into the blues and purples as well. Am I misreading that? At least in the last ten years or so. Is that worth adding, Fred, do you think?

DR. SERCHUK: I am not following which figure you're looking at, Genny.

DR. NESSLAGE: Sorry. I am on Figure 10 on PDF page 71. I guess it would be the last --

DR. SERCHUK: The problem with page 71 is the last two points are predicated on an assumed recruitment.

DR. NESSLAGE: True, but that would only -- That would affect the blue, but not the -- I mean, the red, but not the blue and the --

DR. SERCHUK: It would suggest that there's been a decline in an abundance, a trend in decline in abundance, since 2007 through the most recent period, and it's because the last two recruitments that are in 2018 and 2019 are assumed values.

DR. NESSLAGE: No, I get that, but what I'm saying is, if you just look at the proportion of blues and purples is highest at the beginning of the time series, and then you see none of it for like twenty years, and now we're starting to see some blues and purples, meaning teenagers to twenties, and I don't know if that's worth mentioning. Obviously, that's -- Part of what you're saying is that we're starting to see SSB come back, right?

DR. SERCHUK: Except we're not seeing total biomass come back. If you go back to page 74, it's been stable, which is one of the points that I've had in the past about saying, well, maybe the stock is at a new equilibrium, because you can see it's been about the same size since 1984, and it's fluctuating around, although SSB has clearly doubled, but it's still far below the benchmarks, and that's one of the reasons that I suggested earlier that maybe the stock was at a different equilibrium than -- A different productivity equilibrium than we have assumed relative to the benchmarks.

DR. NESSLAGE: Can I follow-up? I want to follow-up with a question on that though, and maybe I am misinterpreting the assessment, but it seemed like, if you looked at the F by fleet, that we were just trading commercial for recreational Fs, and is that a misinterpretation, or is that a total mischaracterization? I guess that one is aimed at Rob.

DR. CHESHIRE: Are you talking about the F by fleet?

DR. NESSLAGE: Yes, and so like Figure 19. Yes, the Fs have gone down I guess since the 2000s, but the proportion of rec has gone up, to kind of counterbalance the loss in commercial F, and do you think that that might be leading to some of the stagnation in the younger ages, the total numbers?

DR. CHESHIRE: I think the -- Especially with commercial longline, they've dropped down to almost nothing, but there is a slight increase in the recent period for handline, but, yes, overall, there's definitely an increase in the recreational contribution in recent years.

DR. NESSLAGE: Is that switching from other targeted species, or is that just increased effort across the snapper grouper complex?

DR. CHESHIRE: It's hard to say. I think there was an overall increase in effort, especially in the private sector, which I think is where a good portion of this is coming from, or a good portion of the increase, I should say.

DR. ERRIGO: That might be an increase due to more deep-dropping, people shifting where their effort is taking place, and I know that there was an increase in the effort on blueline tilefish, which is associated with snowy, and, down off of Florida -- I don't think there was much of a change down off of Florida, but mostly in the northern part of the range. Over time, there was a shift to deep-dropping and targeting bluelines, because the snowy bag limit has been very low for most of the years, but, if they're targeting bluelines, they catch their snowy limit, most likely.

DR. NESSLAGE: I guess where I'm going with that, for those who are trying to follow the discussion for the write-ups, I guess is that Fred's point was that SSB has been going up, which is positive, and we should note that, as far as the rebuilding goes, but the total abundance seems to be about pretty level, and some of that might be due to this increase in potential effort on snowy, and I'm not sure, but it's worth discussing and possibly bringing up to the council, but I will shut up. Wilson, go ahead.

DR. LANEY: Thank you, Madam Chairman. I was just going to ask if this was an appropriate place to indicate that we would be getting additional data in the future from the deeper longline survey.

DR. NESSLAGE: I think we definitely want that in the next bullet, under monitoring the stock, and I think that's going to be some pretty critical information, and I'm not sure we can use it to comment on the rebuilding progress right now, but it's certainly something that could be used to monitor the rebuilding progress, no? Is that where you're going?

DR. LANEY: Yes. Exactly, and so it may be more appropriate in the next bullet.

DR. NESSLAGE: Yes. Good point. Thank you, Wilson. Jeff.

DR. BUCKEL: Thanks, Genny. Just following-up on Fred's point, I'm glad that he made that, and I wanted to make that point, and I think, in addition, also, the fishing mortality rate has gone down, and so the management has led to this reduction in fishing mortality rate, and then we're seeing a response in the SSB, and that's most likely a result of that, and so just to link those two, if folks agree with that. I think, with the low recruitment, if fishing mortality had stayed high, then we would be in a -- Things would look much worse.

DR. NESSLAGE: Right. Well said. Thank you, Jeff. Scott, you're back.

DR. CROSSON: Yes, I'm back. Just to note that an additional comment, I guess, is that that increase in the portion of the harvest that is captured by the recreational fleet sort of coincides with the dawn of widespread adoption of GPS, which, for -- Especially since you said that a lot of it was in the northern range, where you have to go further offshore, and I'm not surprised to see that people would shift and have the ability to go out there with GPS, in places where you can't navigate by looking at landmarks.

DR. NESSLAGE: Good point. Thank you for that.

DR. CROSSON: There was one other thing, and I guess the -- Mike just said it a minute ago, but the long-time recreational limit is one per person or per vessel?

DR. ERRIGO: For a while now, it's been one per vessel for snowy.

DR. CROSSON: So there is probably -- I mean, the barotrauma, for something that's pulled up from that depth, is probably 100 percent, or close to it, right, and so there's probably a significant amount of discarding that's going on.

DR. NESSLAGE: Have we commented on that elsewhere? Is that worth commenting? I don't remember that in -- Is that discussed in the assessment at all?

DR. CHESHIRE: We combined the discards and landings into removals, since there is no size limit, but, yes, there potentially could be discards, and we get discard estimates from MRIP, and they are included, and I think that's as much discussion as there is, is that it was included. Discards are included with the landings, like they were last time.

DR. NESSLAGE: Okay, and it's --

DR. CROSSON: But there are other species that -- Somebody mentioned blueline, and so there are species that have a greater bag limit than snowy, and it seems likely that you're going to get that first snowy and then, at that point, after that, you're still continuing to deep drop for other deepwater species.

DR. CHESHIRE: Yes. Exactly.

DR. NESSLAGE: Great. Thanks, Scott. Fred Serchuk.

DR. SERCHUK: Thank you, Madam Chair. I have another issue that relates back to the fishing fleets, and, if you look at page 39 of the report, it gives fully recruited fishing mortalities for the three different fleet components, and it seems to me that the -- Based on the information here, the longlines, obviously, have a very low fishing mortality, and they've had it for quite a number of years, and they're at less than 0.01 since 2007, and so they're not having a major impact, in terms of exploitation, on the stock, according to page 39.

If you also look at the recent values for either the recreational or the commercial handlines, they are also very low, and they're below 0.08, and so I don't know whether we want to talk about relative impacts of the different fleets, but that may be important if there's going to be any changes into the management measures, and any information we can provide relative to the impact of the different fisheries, relative to contributing to the overall apical F, would be important, I would think, and maybe this is not the place to do it, but I think we should take cognizance of this page. Thank you.

DR. NESSLAGE: Thank you. I agree, and I think that's kind of -- It's better said than what I was saying before. Thank you, because I think how management is applied is going to make a big difference, moving forward. George.

DR. SEDBERRY: I wanted to go back to Scott's question. When he was talking about the discards, I think he was talking about high-grading, and I think what he was saying is the high-grading might be worse under a vessel bag limit than under a per-person bag limit, and I'm not sure what he was saying though, and I just wonder if that's true, if it went from a per-person bag limit to a per-vessel bag limit, if high-grading would increase or decrease or change at all.

DR. CROSSON: No, I wasn't -- I mean, that might be the case, but I wasn't thinking of high-grading. It's just that one fish per vessel is as low as it gets, right, for restrictions, and so, if you're catching other species in a deepwater complex, and you're fishing -- Because they have the greater bag limits, and you're not going to go out there and catch one snowy and go home. You're going to -- If you catch that snowy, you're going to keep fishing, if there's other species you're fishing for, and every additional snowy you pull up is going to have to be discarded, and he's dead, because of the barotrauma. I mean, they're pulling them from very deep water, and that's where I was headed. I am not sure that that's adequately captured with the MRIP data, but I don't know. I would doubt it.

DR. SEDBERRY: Yes. I just don't know if that behavior changes under -- Or if that would have a greater effect on number of discards under a per-person bag limit or a per-vessel bag limit. Regardless, people are going to keep the biggest fish and discard the dead small ones that they've already caught, I think, and I was just wondering if there's any data on whether changing from a per-person bag limit to a per-vessel bag limit had any effect on high-grading or discards.

DR. CHESHIRE: We had -- There is almost no information on discard length composition, and it's hard to say. You can see it in the comps a little bit, and I think it was one of the commercial gears also that you can sort of see shift after -- For commercial, you can see a little bit of a shift after 2007, I think, but, for I think it was the handline, but, for the recreational, the comps are so patchy, and, with small sample sizes, that's really hard to identify, and, without a size limit, it's hard to tell what year that would have started.

DR. NESSLAGE: I wonder if it's worth -- Is it worth something to add -- I don't know if research recommendations is the right place for it, or perhaps the uncertainties section, to say this is something we might need more information on, because it could be impacting the efficacy of management and rebuilding, or you're thinking that, because it could impact rebuilding, that it should be under here? George, is that what you're thinking?

DR. SEDBERRY: I think you're right, Genny, that it's more of maybe a research recommendation.

DR. CROSSON: I just don't know -- I mean, under fishing level recommendations, I guess, without having the data, and I don't know that we're going to get the data about what the discarding rates are happening for snowy, and I don't know that I can make a really clear statement, but it just seems to me that one -- In a deepwater fishery, with what I would assume would be a very high bycatch mortality, I would be worried about a restriction as severe as one fish per vessel, especially if it's a multispecies fishery and they're catching lots of other deepwater species, and I would be concerned about that.

DR. NESSLAGE: I think you are definitely onto something important there, Scott, and I see that you are in the group for this, and do you think you could write up something along those lines to put in there and relate it to concerns about rebuilding progress, even if we're not sure, but it's just an area that we're concerned about, and hopefully we can be proven wrong, and then, George, we can make sure that there's -- I don't know what group you're in, but if you can check in and make sure there's something under research recommendations.

DR. CROSSON: George is in with me.

DR. SEDBERRY: So we can take this offline for a while then.

DR. NESSLAGE: Yes, but it's good, and I hope you capture it. That's wonderful, and the research recommendations folks hopefully can add something about discards, too.

DR. SEDBERRY: Okay.

DR. NESSLAGE: Thank you, all. That's good. What else are we missing under rebuilding progress, big points that folks want to make? I am not hearing too much. Let's keep scrolling here to the next big bullet, to provide advice on monitoring the stock until the next assessment, and so we discussed a few things, the new survey information, deepwater -- A couple of other -- Let's see. We have indicators or metrics, and also trigger levels for those metrics. Church.

DR. GRIMES: Thank you, Genny. Well, as Rob mentioned quite a while ago, there are really only two fishery-independent indices of abundance, the MARMAP chevron trap and the vertical longline. The trap catches, as he said, are very low, and I guess variable as well, and so the vertical longline is probably the better of the ones to monitor most closely, but it's hard to imagine a trigger for something as iffy as those indices are, but, anyway, it would be interesting to hear what people have to say about that, but all points, as you were about to say, that badly need to develop a deepwater survey, although it's not clear whether that -- Has that started already, and is just starting up, or is it just planned to be started, but, in any case, it's clear that that's important. Anyway. Thank you.

DR. NESSLAGE: Thank you. Katie or Rob, when did you say that that was going to start, or has started?

DR. CHESHIRE: I believe it has started, but I'm not heavily involved, and Wally probably has a better answer.

DR. NESSLAGE: George, can you wait? Let's jump to Wally, really quick, and see if he's got an answer for us.

DR. SEDBERRY: Sure.

DR. NESSLAGE: Thanks. Wally.

DR. BUBLEY: I do have an answer, and so we did start it this year, and there's probably going to be some tweaks going into next year, but it is -- The initial year was completed this past -- In 2020, and it will be moved forward next year, and we're making plans right now for how we're going to handle it next year.

Another addition with that short bottom longline index, and while this might not help a lot, because there's not a lot of frame of reference, there was a MARFIN grant that was funded for an increase in the spatial coverage of that short bottom longline, as well as increased density, and so that will be taking place over the next two years or so as well.

DR. NESSLAGE: That's great, and, while we have you on the horn there, regarding metrics, is there anything in particular, other than catch rates, that -- I assume you're getting biological information, and what else can we use here?

DR. BUBLEY: That's correct. It's just, as they said, it's just the levels that we have fished at for at least the short bottom longline are relatively low, and so that was the impetus for trying to expand it, and the deepwater index, because it just started, I'm not sure if I can provide some sort of metric, outside of, yes, we are collecting length and age information on everything we can.

DR. NESSLAGE: Great. Thank you. Back to you, George.

DR. SEDBERRY: I think Wally has touched on one of the questions I had, but it seems to me that the longline survey is in the adult habitat, and the trap survey is in the juvenile habitat for snowy grouper, and maybe Wally could speak to this, but would the trap survey be a good -- Would it be a better recruitment index, and that a trigger might show up in that as a recruitment trigger?

DR. BUBLEY: I think you're right, George. I think that's definitely a different size class and age class that we're dealing with with the index, and it might be interesting to see what they look like, because, if you look at the indices that we produced there, there almost appears -- At least the last couple of years, we see an increase in the trap, the last two years or so of it, or two or three years, and we see one year of an increase in the short bottom longline, and so there's a potential that it could be useful as a recruitment index.

DR. NESSLAGE: Excellent. Thank you. Marcel.

DR. REICHERT: Thank you for allowing me to provide a brief comment. Given the fact that it seems like there's a little bit of confusion, I want to make sure that everyone understands that the new deepwater index is an entirely different survey than our short bottom longline index. We have collected snowy grouper, historically, on our short bottom longline, but I am not entirely sure what the number of snowy groupers was in the first year of the new deepwater survey, but I want to make sure that people understand that those are two different and separate longline efforts. Thanks.

DR. NESSLAGE: Thank you for that clarification. I am wondering, given the questions that came up earlier about discards, is there any -- We've been thinking about independent data, but I'm wondering if we should -- If the data become available, if monitoring discards would be something that we might want to consider as well. I don't know how we would get that information, but if for some reason --

DR. ERRIGO: The SSC may recommend that as a citizen science project. They're doing something like that right now for scamp, using the scamp release app, and we're developing an app that has universal applications, so that you can tweak it to gather information on whatever you would like, and you could make a recommendation that we gather information on snowy releases, maybe through a citizen science program, but just to emphasize that that's something that should be collected here and would make a big impact.

DR. NESSLAGE: Chip, would you like to add to that, or something different?

DR. COLLIER: I will just add a little bit onto that. What they're trying to do with that release app is actually get a size distribution of the released fish and not a total abundance, or a total number of fish released, and so that might be the way to think about the citizen science project, is maybe through a size distribution, rather than a total catch, just because of the amount of effort it would take to recruit people into a citizen science project in order to get a total estimate of releases.

DR. NESSLAGE: Thank you. That's good information and a good suggestion. I threw that out there as a suggestion, and how do folks -- Does anyone disagree that that's something we might want to suggest? I am happy to drop it if you disagree, but --

DR. CHESHIRE: I don't have a hand-raise button, but I just wanted to add to that. It seems like it would be really helpful, because I am looking now at the discard estimates from MRIP, and, even in the most recent years, the 2017 and 2010 and 2011, the discard estimates are actually zero, just because they're so poorly sampled, and so it would definitely help to fill in the gaps, if nothing else.

DR. NESSLAGE: Great. I think so, too. Jeff, what do you think?

DR. BUCKEL: If it's zero, then maybe this point is moot, but there is a requirement now to have descender devices on boats, and, at the shallower depths that snowy grouper are caught, you do get a benefit, an increased survival, using descender devices, and so that may be something else that citizen science monitoring could provide, if when they do -- If snowy grouper are released, if a descender device is used or not, or if venting is used, and that may help in the future for -- Instead of assuming 100 percent discard mortality, it might be less than that.

DR. NESSLAGE: Great. Any other recommendations for indicators or metrics? I will let the breakout group see if they come up with anything for triggers. If not, if it's just we need more information and then we'll worry about it, that's fine too, but we just need to let the council know that fact. No hands raised, and let's go on to the last bullet then. Provide research recommendations and guidance on the next assessment. Fred, is this for the new one or the last one?

DR. SERCHUK: It's an observation, in terms of -- Let me put it out there. As you recall, when we were discussing the stock-recruitment relationship, and I know we're going to be discussing that as one of the uncertainties, because we fixed the steepness, but, if you go to page 82 of the report, where it looks at the stock-recruit relationship, you will see that that stock size is less than 600 tons, and you have most of the points less than 600 tons are among the highest, and you can see there is twelve that are above the expected line, and only two or three below, and then, as you get to a larger spawning stock size, most of the points give you poor recruitment, at least between 500 -- At least above 600 tons of spawning stock biomass.

I am thinking that the increase that we have had in spawning stock biomass was facilitated by the lower stock sizes that we had that produced better recruitment, and now that we're at stock sizes that have been above 600 tons, we're starting to see poor recruitment again. Now, that would -- I know that's just an observation, and I know that relates back to maybe there is a negative relationship between recruitment and spawning stock size, but it's something to think about, and I think we ought to think about it in terms of examining it in the future, relative to dynamics of the stock. If you don't want to entertain that, Chair, that would be fine as well, and it's just an observation.

DR. NESSLAGE: No, I think it's fine, but you're talking about research recommendations.

DR. SERCHUK: Yes.

DR. NESSLAGE: Absolutely, and I hope that the breakout group -- Please do try to capture that sentiment, and I think you're totally on track there. The other thing that I am thinking about here -- We always get asked to review the research recommendations that are included, and I don't think you really went through them, Rob, and I always feel weird about this section, because we don't really ever give this a really close look.

Is there anything that we have brought up that -- I guess, is it possible that we could pull these up really quick, to see if there's anything that really stands out to us or that we disagree with? Otherwise, I usually just tell the council, yes, what they said, plus these extra things that we came up with, but we should probably do our due diligence and take a look. I don't know, Rob, if you want to just hit the highlights of things that you guys thought were the biggest issues?

DR. CHESHIRE: Sure. I can do that. Some of these, in the report at least, are carryovers from SEDAR 36 that just still need to be addressed. That deepwater survey is probably the biggest research recommendation that we could hope for, and then the increase in the age samples, especially from the recreational sector, since that's increasing in recent years, and we really don't have a good handle on it. Let me make sure I'm understanding our own bullets here, but the --

DR. NESSLAGE: Sorry that I put you on the spot. My apologies.

DR. CHESHIRE: We have this break at Cape Hatteras that we're using for this, or that we could use for this, and we've also -- Especially with blueline, I think we saw more increase in landings in I think especially southern Virginia, and I think re-evaluating how far north we should go would probably be a good idea, just in general re-evaluating the stock boundary. As far as the next bullet on protogynous life history, I think all of that is still needed, and I don't think we've made a lot of progress on any of those fronts.

DR. NESSLAGE: Great. What about the last one about spawning?

DR. CHESHIRE: I think we still need that one, and the recommendations in the presentation might be slightly different, and I think there might be -- I can't remember off the top of my head if there's anything different than what's on here.

DR. NESSLAGE: Okay. Thank you. I appreciate that, and so everybody has had a chance to kind of skim these over, and if you don't mind going back then, Mike, and I think we didn't really talk about some of those. If anyone has any more thoughts, now that you have seen that, on either the research recommendations suggested by the analysts or any ones that we add to this.

DR. CHESHIRE: There are a few different ones on the presentation, as far as evaluating the MRIP time series and the selectivity blocks.

DR. NESSLAGE: That's right.

DR. CHESHIRE: Then, of course, natural mortality and what we're expecting to --

DR. NESSLAGE: Yes. He had assess MRIP removal estimates and selectivity blocks for the commercial fishery and methods for estimating natural mortality and the deepwater survey, which we've already got up there, and so is there anything else that folks think is important that we're not mentioning? Chris, go ahead.

DR. DUMAS: Regarding the abundance indices and trying to make better use of the data that's coming from the abundance indices, both the chevron traps, the bottom longline, and the new deepwater index, any of these abundance indices with low catch rates, we might -- If it hasn't been investigated yet, we might want to look at what's called inverse sampling methods, to see if those could be used or adapted, to help us get better estimates of abundance from the data that we have.

These methods are used when you're sampling and you have a really low frequency of obtaining your sampling target, and so you could think of like each trip, each fishing trip, being a sample unit, and the proportion of trips having snowy would be, perhaps, low, and then the number of snowy per trip would be low, and, these methods might be useful in that situation, to help get better, or more reliable, abundance estimates, and so those are inverse sampling, and there's a paper by Holbein and Biometrika in 1945, and I can forward that site, but then the method has been sort of generalized and used in some sampling technique textbooks that I have been looking at for some other purpose, but it occurs to me that those methods might be useful in this situation, and so I can forward a reference, if folks have not already looked at that. Then I would like to just

ask a question to staff, and have you guys looked at inverse sampling methods in sort of these low-frequency abundance data type species? Thanks.

DR. CHESHIRE: I can say that, for the indices that we typically run, we have not, but Wally and MARMAP did these indices, and so I'm not sure if they have looked at those or not.

DR. BUBLEY: We have not gone in-depth looking into that, and so that would be interesting to read up on that that you're sending out, Chris. Thank you.

DR. NESSLAGE: Great. Thank you, Chris. Wilson.

DR. LANEY: Madam Chairwoman, I think this is probably already covered in the list, but I just wanted to reiterate that we had a lot of discussion about the fact that the shore mode -- I think this was for snowy grouper, that the shore mode -- Or I may be confusing it with another species, but, in any case, because of my direct hands-on experience with seeing little tiny snowy groupers being caught on the screens at the Brunswick Steam Electric Plant in the Lower Cape Fear River, it just reminds me to say that some of those very young fish are probably caught in the shore mode and that it would be useful to try and get more information on that length frequency, but that's already captured in another recommendation in the assessment document itself, I think, but I just -- I think we discussed that back in August, if I remember correctly.

DR. NESSLAGE: Well, it can't hurt to reiterate though, because, every time the council hears stuff, or every time we say it, the stronger it becomes. Thank you, Wilson.

DR. CHESHIRE: There isn't a shore mode included in this assessment, and the estimates weren't provided, and I don't know if that's because they weren't provided for SEDAR 36 or if there just aren't any because there are so few that they don't show up.

DR. LANEY: I think, now that I think about it, Rob, I think that was gag grouper that we were having that discussion for, but, again, just for the record, I will say that we did catch snowy on the screens at the Brunswick Steam Electric Plant back in the 1970s, and that would be from around 1974, when they cranked up the pumps, until 1978 or so, when those of us who were working down there finished our field research. Yes, they are being captured inshore, to some extent, but not nearly as frequently as gag, and so you're right. I think, maybe as a research recommendation, we could, in conjunction with the citizen science project that Chip mentioned, and it would be interesting to know if anybody is catching any really, really far inshore. Probably not.

DR. NESSLAGE: Great. Thank you. Any other things that we haven't mentioned yet that should go in this section for Wilson and Alexei and Jie? Fred Serchuk.

DR. SERCHUK: Thank you. I have a question that didn't come up before, and this may relate to the model formulation. The model gives twenty-four age groups, and then as a plus-group at twenty-five, in terms of the way the model was run, but, after age-thirteen, all age groups above fourteen to twenty-five have the same estimated fishing mortality rate, and I am just wondering why that occurred. Is there just a dearth of data, or did you assume that, after age-fourteen that all of the older components in the population were going to be exploited at the same rate? It's a model formulation question, really.

DR. CHESHIRE: The selectivities were fixed at the fourteen-plus group for the age-fifteen to twenty-five that were in there, mainly because there is still life history things happening, like growth and some other things that haven't sort of reached an asymptote until age-twenty-five, but the selectivities were fixed, because there was very, very little information beyond that, which is why we chose the fourteen-plus.

DR. SERCHUK: Okay. Would the model have been any different if you only ran it through fourteen age groups and added a fifteen-plus?

DR. CHESHIRE: I think it's possible, and it might have a slightly different result, but I think we -- That was done in SEDAR 36, to capture the life history components.

DR. SERCHUK: Okay. It's just worthwhile investigating, because I know you have the twenty-five, but, when I looked at the output, everything from fifteen on had the same exploitation rate in every year, and they differed between years, but the age groups in any one year above fifteen all had the same values.

DR. CHESHIRE: I think that's just based on a lack of information to inform anything different.

DR. SERCHUK: Thank you.

DR. NESSLAGE: Fred, do you think it's worth recommending that more work be done looking at the age class plus group definition?

DR. SERCHUK: Whether it makes any difference by cutting it off at fourteen with a fifteen-plus age group, as opposed to a twenty-five, and some -- Even at twenty-five, some assumption had to be made about what the Ms were on those older groups and whether the oldest age group, twenty-five, captured any of the natural mortality that was available for the older age groups, and it's very low, of course, at those older age groups, or whether you could just use an average value from fourteen on. Again, I don't know whether it was evaluated, but it just seems we have a model that now goes to twenty-five, even though we use an M for fifty-eight age groups, and then we have an output that, after age-fourteen, the output from the fishing mortalities are the same from fifteen to twenty-five.

DR. NESSLAGE: Good suggestion, Fred. Jeff.

DR. BUCKEL: Thanks, Genny. On the research recommendations, I'm just thinking more about M has such a big impact here, and on other assessments, and I wonder what the analytical team thinks about a research recommendation to do some simulation -- Build some populations and test BAM with trying some of these different things that we have listed here and see which one helps with the estimation of M. I know that, often, you get post-docs in to do some of these projects, and so I'm just curious what the analytical team -- If that's a worthwhile endeavor that it would help for the SSC to list as a research recommendation here, and so I guess that would be simulating datasets and using BAM to see -- Looking at some tradeoffs, is it worthwhile to put more money into the deepwater survey, or is it better just to get -- You get a bigger bang for your buck getting better age comps from the fishery, for example.   Thanks.

DR. NESSLAGE: Great suggestion, Jeff. I like that. I hope that others do, too. If there's anything on this list that folks don't want to see, speak up. I think we're getting close in our brainstorming session here. I am not seeing any hands.

The last thing we need to chat about, before we break, is guidance on the next assessment, addressing its timing and type. We started that discussion a little earlier, and Fred had said that we might need to take a look more frequently than the rebuilding projections would suggest, out to 2039, and so do we want to work with the normal perhaps five-year ABC setting, and then suggest an operational assessment at the end of that? I will just put that out there as a strawman recommendation, and feel free to pull it right back down. What do folks think? People like this. Well, that was easy. Okay.

Well then, I appreciate all that, and these are all excellent ideas and contributions. Unless there are any other outstanding concerns or comments, I think this might be the time to try our fancy new breakout approach to generating strawman language, and so Mike has already done some of this work, and I don't know, Mike, if you want to just email this document around to everybody real quick, in case they didn't catch everything on the screen.

DR. ERRIGO: Yes.

DR. NESSLAGE: That would be great. Just to reiterate, the links have been placed to the -- There is five breakout groups that the SSC members will break off into, to come up with strawman language. Public, if you are interested in joining any of those groups, the links are in the question/chat section on your right, in your control panel, and feel free to join any of them and listen in. If you have comments, please save them for the general group, when we reconvene.

I am going to ask that everyone try to do this in about a half-hour, and so let's break and come back at 3:00. In the meantime, as folks finish up generating their strawman language, if you could email that to Mike, so he can start compiling it and putting it on the screen, and then we will all reconvene on this webinar around 3:00 and start to finalize that wording. Thanks for posting that again. Any questions or comments before we break? Does everyone know what they're doing? It sounds good?

DR. SEDBERRY: Do we have to sign out of this webinar before clicking the link?

DR. ERRIGO: Yes, you do, and then you will have to sign back in.

DR. SEDBERRY: Okay.

DR. COLLIER: So the plan is to come back together at the big group at 3:00?

DR. NESSLAGE: Yes. Thank you. We will see you back at 3:00. Good luck. I appreciate your willingness to try this.

(Whereupon, the SSC went into breakout sessions.)

DR. NESSLAGE: What I would like to do is have everybody -- I will just kind of briefly read what's on the screen, very quickly, and then, as we go through, I'm going to take public comment,

because this will be the first time the public has seen all of these, and then we'll take that all into consideration as the group finalizes the consensus statements.

Again, this is strawman language provided by these breakout groups, and it's meant to just kind of expedite the process, but we want to make sure that we have a good discussion, but keep in mind that I really don't want to do a lot of wordsmithing. I want to make sure that the content is all here, and we can add proper sentences and better wording later, but, if you think the content might change with any future wording changes or suggestions that you might have, then I would like to hear it now, but, again, let's just go through this.

With regard to overall review of the assessment, the assessment does address the TORs to the SSC's satisfaction, and the SSC concurs the assessment represents BSIA. It concurs the assessment provides an adequate basis for determining stock status and supporting fishing level recommendations.

When we get down to uncertainties, we have listed several areas of uncertainties that might affect stock status and fishing level recommendations and recreational data, in particular the 2012 data point, and there was a sensitivity analysis conducted to deal with that, and hopefully it's not -- That addresses the issue, although this doesn't really clarify -- Folks who were in that group, do you think additional work needs to be done? Is that what you were suggesting, or --

DR. BUBLEY: We realized, as we were trying to get it, that a little more time would have been helpful, and so it was kind of basically trying to get everything, at some point, on here and then we didn't really get to expand on it nearly as much as we had hoped to.

DR. NESSLAGE: That's fine. Are you thinking that more work needs to be done to examine the 2012, or do you think the sensitivity analysis addressed it? Is that what your group was thinking?

DR. BUBLEY: At this point -- Fred, were you the one who brought that up? Like I said, we were just trying to tick off things at the point at -- Things that could potentially affect it. I mean, we did the sensitivity analysis, or the sensitivity analysis was done, and so it should have looked at it, and I think we were just trying to figure out anything that potentially could have caused those uncertainties and not necessarily if they needed to be addressed moving forward.

DR. NESSLAGE: I see what you guys are saying, because I guess I'm confused, because I thought that it didn't have too much of an effect, if I remember correctly.

DR. BUBLEY: Correct. I mean, that was my understanding as well.

DR. SERCHUK: It still begs the question of why it's such an outlier. We think it should be looked at even closer, even though it had little impact on the estimates of stock size.

DR. NESSLAGE: Right, and Rob had mentioned they were hoping to look more carefully at the MRIP estimates anyway, right? Okay. We can flesh that out. Large uncertainties around individual data points in the indices, the Beverton-Hold stock-recruitment curve to fit data points, including the fixing of steepness in the model fit, natural mortality, and that's a big -- Definitely natural mortality. Increase in -- Is that the chevron?

DR. BUBLEY: That's the short bottom longline. Fred Scharf brought up that that one year, in 2000, had a really -- It differed a lot from the initial assessment in SEDAR 36 versus the update, due to potentially standardization or other factors.

DR. NESSLAGE: But did it have an impact on stock status and fishing level recommendations?

DR. BUBLEY: We don't know, I think, and so I can go with that, but that was a change that someone had mentioned, and it was an uncertainty, basically.

DR. NESSLAGE: Sure. Okay. I see where you're going. Use of apical F, the stock has been stable well below the BRPs since 1984, suggesting the stock may be in a different productivity regime. At very low fishing mortality values, lower than the natural mortality, the stock dynamics may be controlled by more natural processes than fishery processes. Autocorrelation in the residuals of the abundance fitting, which is not currently considered in the current model configuration, and they are not fit well, those indices. Those are all good points. Again, we can flesh out the wording, but is there anything -- Well, actually, let's just keep going, and we'll go through each of these, and then we'll come back.

Risks and consequences, M could be under or overly conservative, which could affect the stock status and fishing level recommendations, as shown in the sensitivity analyses. Recruitment could be lower than earlier in the time series. If changes occur in natural mortality at earliest ages in the fishery, then the projections will be quite different. This happens when F is lower than M. Long versus short-term effects on projection outcomes. The uncertainty of assuming a steepness using a Beverton-Holt curve has impact on BRPs and status determination and fishing level recommendations.

With regard to uncertainty, consistent with expectations, and suggestions moving forward, but no concerns based on previous methods. Consistent with what is typically produced, and that's good, and then you guys suggested an overall list of what contributes most to risk and overall assessment uncertainty, starting with, of course, big natural mortality, a Beverton-Holt stock-recruitment curve fit to the data points, including steepness and fixing of steepness.

DR. BUBLEY: With these, we basically -- Natural mortality and the recruitment curve were the ones that we kind of listed up towards the top, and everything else we just kind of put below that one, those two, I mean.

DR. NESSLAGE: So I wonder if we can say that in the report too, that the two big sources of uncertainty are those two, natural mortality and Beverton-Holt. Everything else is kind of more minor, and like those are the two major, and then everything else is more minor, and is that fair?

DR. BUBLEY: Yes, and that's the conclusion we came to, and so, I mean, the rest of the panel can agree or not, but that's what we thought.

DR. NESSLAGE: Okay. Is there any way, Mike, that you can scroll down, or are you in the middle of typing?

DR. ERRIGO: Sorry. Hold on one sec.

DR. NESSLAGE: No worries. Thank you. Down to fishing level recommendations, no difficulties applying the control rule. Changes in M regarding -- This is regarding rebuilding progress, and changes in M, M relative to F, and changes in R.

DR. ERRIGO: That's what I had there. They didn't capture it in their bullets, and so that might be able to be removed.

DR. NESSLAGE: Okay, and so natural mortality may have changed over time and may exceed fishing mortality more recently, because younger year classes now compose a larger portion of SSB, and that's good. Spawning stock biomass is increasing, but recreational catch is going up, which means more of the catch is composed of younger fish. Although the SSB is far below the reference points, it has doubled in size since 1994 and has been on a positive trajectory since then. Management measures have reduced F in this fishery over the period where the SSB is increasing, but discard rate for the recreational sector is unknown. The one fish per vessel bag limit may cause significant recreational discards. Excellent.

DR. ERRIGO: I will just get rid of that, because I think it's captured here.

DR. NESSLAGE: Yes. Totally. So the next section is provide advice. Continue to refine methodology for the deepwater survey to serve as an index, evaluate age and size selectivity for snowy grouper in the short bottom longline and chevron trap surveys to assess the potential for either survey to serve as a recruitment index. Recommend a citizen science project to get size distribution or other info on releases.

With regards to triggers, they came up with some ideas. Consider using the frequency of occurrence of individuals captured in the fishery-independent surveys that exceed age/size thresholds as an indicator of stock recovery. Consider monitoring the body size at sex transition. The availability of large individuals in the population influences this size metric.

Then research recommendations. Increase in fishery-independent data, expansion of age samples, evaluation of methods for estimating M, evaluating the utility of selectivity blocks chosen, and that was all under the stuff that was already in the report, and then, with regard to our additional or expanded research recommendations, type of relationship between SSB and R may not follow the Beverton-Holt curve. Then the use of inverse sampling.

Evaluate input from -- This is all what you had written down before, right, and so explore the effect of different methods used to develop indices of abundance, and so delta lognormal versus zero-inflated negative binomial for those indices. Why did they generate different trends or peaks and valleys? What caused the differences and how to deal with it. Explore MRIP data in greater detail to understand what causes some of those outliers, like the 2012 year, and how best to deal with it.

Explore potential autocorrelation in the data and the ways to correct for it within the model, including accounting for temporal autocorrelation in recruitment. Consider not specifying stock-recruitment relationship and model recruitment as an average value with random annual residuals. Rather than calculating MSY and BMSY from the stock-recruitment curve, consider alternative proxies. Better estimates of M. Subset species used in Then et al. analysis to include only grouper snapper or similar species, empirical tagging studies, et cetera, and use them to come up with field-based estimates of M at-age, and conduct simulation studies.

Improve indices of abundance. A deepwater survey is highly desirable. Yes. Investigate further whether there have been shore-mode captures of snowy and look at using different plus groups, given the age up to eighty. These are some notes that we'll have to take out, but Alexei noted the proportion of females to males will change as the fish age, and it's important to have big females. If the peak of the biomass is age-five to six, that will be the peak of the spawning potential, and that is most important to control. I think we had inverse sampling, or are you just taking that out and putting it down there? Okay.

That's the overall result of the breakout groups. While everyone is kind of digesting that, I would like to take a moment to open the floor for public comment. Is there anything in here that the public saw during the -- If they joined one of the breakout groups that they would like to talk about or that they see on the screen that they would like to comment on before we finalize these? No hands raised. Last chance here on this agenda item. Okay. Thank you. Wilson, go ahead.

DR. LANEY: Thank you, Madam Chair. I just wanted to note that we kind of ran short on time in our group, and so please, when you're reviewing the research recommendations -- We tried to go back and capture those that had been mentioned earlier, but we may have missed some, and so everybody please take a close look at those and make sure to help us capture any that we may have missed.

DR. NESSLAGE: Agreed. Thank you, Wilson, and we'll talk about whether thirty minutes is enough or whether we need to do this in a different fashion later, and I think we all learned something from this exercise, and so if we could start at the top again, real quick, Mike, and then let the SSC just kind of stare at that first set and see, again, content-wise, is there -- We can refine wording later, but is there anything in the first big black bullet there that anyone disagrees with? This one is pretty easy, I think. Okay. No hands.

You will have an opportunity, always, to review this after it goes out, but the second big bullet is identify, summarize, et cetera, the first -- We need to number these, I think, in the future. Review, summarize, and discuss, that bullet, and so these are factors that affect reliability of estimates of stock status and fishing level recommendations, and so are there any -- I am looking for content here, and is there anything here that folks disagree with or that they would like to -- That they don't understand or they would like to see changed? Okay.

Let's move down to b, 2b, describe the risks and consequences of the assessment uncertainties with regard to status and fishing level recommendations. The long versus short-term effects on projection outcomes, is that -- That is the issue that Fred raised earlier about whether we're talking about five-year ABCs or success out to rebuilding target year, and is that -- Can you guys talk a little bit about that?

DR. BUBLEY: Fred can talk about it more if he wants, but it was more a placeholder than anything, I think, as we were just brainstorming at that point, but, yes, that was the point.

DR. NESSLAGE: Excellent. Anything we need to change or add there? Does that capture the main points we had? Okay. Then c looks like we're feeling comfortable with what was done. We have some suggestions, and then, with regard to the order of -- I think can we add in here, Mike, if you don't mind, and just have like a major sub-category and have natural mortality and Beverton-

Holt and then minor and have the other ones under it, and I think that's kind of -- Just as a placeholder for that discussion.

DR. SERCHUK: Can I make a comment, Genny?

DR. NESSLAGE: Yes, please.

DR. SERCHUK: I know I -- It's not because I made this comment, but I think the apical F should be looked at and other possible metrics for expressing exploitation on the stock, and I only mention that because, if you go to the table in the document, page 39, you can see that -- I understand that, because of the different selectivities of the different fleets, the apical F that's used is vastly different from those of the selectivities of each of the fisheries, and it's not clear to me that a single datapoint for a single age group should be used, because it's the highest.

I think, when someone looks at this table, they're going to be very confused, quite frankly. They are going to see very low values, let's say in 2018 of 0.07, 0.006, 0.047, and then we have an apical F that we use that is multiples of the Fs that we're using to characterize these fisheries. I understand it's because of the selectivity, but it's very confusing, and I don't think it's representative, quite frankly. If you're going to use an F, maybe you should use an apical F from a number of ages, maybe a weighted F of some type, but that's food for thought, to be looked at as a research recommendation.

DR. NESSLAGE: So are you suggesting we move it up to the major section?

DR. SERCHUK: My personal opinion would be to move it up. It's not uncommon to use an average F over a number of age groups, rather than a single age group.

DR. NESSLAGE: Absolutely. Good point. Does anyone disagree with that? Alexei.

DR. SHAROV: I am not disagreeing, but I think it depends on what becomes some sort of a standard that is used for the species, or maybe a group of species. I am familiar with some assessments where the full F, or the apical F, is used, and it's been used for like decades, and so everybody is well versed and understands it, as long as the selectivity is remaining more or less the same, but, of course, if the fishery changes, selectivities change, and then the meaning of full F may be sometimes misleading or confusing, because people will forget that what other age groups are experiencing is very different from what we report, but it's -- The weighted ones are also used, but they are not universally standard, and so I guess we'll have to come up with something that is, in the view of the SSC, and, of course, the analysts, the most useful and telling, and so I'm not necessarily objecting to Fred's idea, and I think he is right that -- The analysts and us, we should look at it and decide on which way we think is the best.

DR. NESSLAGE: So maybe just modify the wording a little to say explore alternate indicators of exploitation besides apical F, something like that?

DR. SHAROV: I would agree with that.

DR. NESSLAGE: Okay. Thank you, Mike. Thank you, Alexei and Fred. Amy.

DR. SCHUELLER: I was just going to agree with Fred and with what Alexei just said and say I know we aren't wordsmithing here exactly, but, yes, the way it was worded before, I was -- It was unclear to me what was meant, and so thanks for the recommended change.

DR. NESSLAGE: Thank you, Amy. Any other comments? Fred Scharf.

DR. SCHARF: Just a quick correction under the minor one for that increase in the VLL index. It's actually the chevron trap index, and it was just a shift in where the -- It's not really an increase, and it's more a shift in where the peak occurs, and so, in the SEDAR 36 data, the peak occurs in 2001, and, in the update, it occurs in 2000, and so it seems to be shifted back a year, and I'm not sure whether that's just a graphing issue or whether that's really the adjustment that occurred. That's just to make that clarification.

DR. NESSLAGE: Thanks. I think it might be above as well, and so, when you're done there, Mike, if you could paste it, that would be great. I think, but check me on that. Wally.

DR. BUBLEY: Just to that point, I think it was actually -- I'm not sure it was a shift, because the 2001 peak is still fairly high as well, but it's just the 2000 peak goes above it, if you look at it, and so I think it's just that one year that's actually being affected.

DR. SCHARF: Thanks, Wally.

DR. NESSLAGE: Thank you, Mike. Thank you, Wally. Thank you, Fred. Other comments on our list here? Anything else? No hands. Excellent. Thank you. Let's see what we have below. No difficulties in applying the control rule. Adequate rebuilding progress. We've got some nice wording here. Any opposition to the content of these as consensus? Amy.

DR. SCHUELLER: While I agree that we didn't necessarily have difficulties in applying the control rule, I think that it's worth noting here that we had a discussion between the MSY versus proxy and how that -- For I think it's Tier 1, the language there needs to change when they're looking at revising their ABC control rule.

DR. NESSLAGE: Yes. Thank you. Let's put a note in there, if we could put another bullet underneath and just a placeholder in there for something about the need for clarified wording regarding MSY estimation versus proxy, something along those lines. I think we're recommending that that be reconsidered during our control rule amendment process, right, that we revisit that.

DR. SCHUELLER: That's what I think we're recommending, is that, when the ABC control rule is going through the amendment process, that language needs to be fixed.

DR. NESSLAGE: Yes. Excellent. Thank you. Anything else under either difficulties in applying the control rule or rebuilding progress being adequate? No hands. Okay. Let's keep rolling. Indicators or metrics, longline survey, potential abundance indices, short bottom, chevron, citizen science. Any objections to the content or changes or edits that you want to make to the content here? No hands. Excellent.

Trigger levels. Frequency of individuals captured in the surveys that exceed age/size thresholds as indicators, monitor body size at sex transition, and anything you object to or want to add or change, content-wise? Looking good. I like it. Let's keep going. Research recommendations, we've got the ones that were in the assessment, which we seemed to agree with, and, largely, we can expand on summarizing that in the report, but then our additional, or expanded, ones. I think that first one is still your summary, Mike, and is that correct? Have we captured all of those below?

DR. ERRIGO: This one was my summary, yes. I think relationship between SSB and R, yes, and the Beverton-Holt is captured and stock recruitment and consider alternative -- Was it this one or -- I swear I saw it somewhere.

DR. COLLIER: It might be good to have the group talk about that one, to see where they would put it, or if they addressed it.

DR. ERRIGO: Here it is. Consider not specifying stock-recruitment relationship and model recruitment as an average value with random residuals.

DR. NESSLAGE: I think they're all in there.

DR. ERRIGO: That I believe is addressing this. What I meant by this is Fred Serchuk's comment about the spawner-recruit curve not following Beverton-Holt and possibly being linear or negative in nature or non-existent.

DR. NESSLAGE: Right, and I think -- Well, I shouldn't speak for it, but maybe Alexei or Wilson or Jie, and I think the alternative then though is to estimate average recruitment and annual -- Or are you thinking of an alternative stock-recruitment relationship? Is that what you're suggesting, or what you think Fred was suggesting?

DR. ERRIGO: That's what I was looking at down here. They suggested not specifying a stock-recruitment relationship and the model recruitment as an average value with random residuals.

DR. SHAROV: Yes.

DR. ERRIGO: Or to use alternative proxies rather than a stock-recruitment to MSY proxy.

DR. NESSLAGE: That's a little bit farther than we went in the discussion, but, personally, I like it. I don't know, and what do others think? Alexei, go ahead.

DR. SHAROV: I was not raising my hand, but we have already said what we wanted, and now it's up to the group whether you agree or disagree with it.

DR. NESSLAGE: Right. Fred Serchuk, what do you think? This was your baby.

DR. SERCHUK: I think that we need to think about this a little bit more, just because there's a lag. We have data that goes through 2018, and here we are in 2021, and we're applying recruitment that we got off a stock-recruitment curve that we know -- We have assumed a recruitment value, in one of the projections, that is very much higher than what recruitment had been before that, and

then we have probably two more years after that before any management action, based on our current situation, can be effective.

I think we need to think about what is the best, of the most appropriate, estimator of recruitment, particularly in the short term. My feeling is, if recruitment in the short term is high, and it's been high recently, then we ought to go with a high value and not take it off the stock-recruitment curve, particularly if it's an assumed steepness.

I am wondering if we can have some flexibility, in terms of evaluating that, with respect to how previous projections were done, to see whether the projections that might have been used in the past, when using an assumed steepness for estimating future recruitment -- Whether they actually bore out or whether it might have been better to take an average, and I think that's the sort of investigation that I think would be very worthwhile, and it would help us in the future, particularly because we have a long lag between when we do the assessment and when the assessment series ends and then between when we provide our advice and when it's actually considered in terms of the regulatory framework. Thank you.

DR. NESSLAGE: That's good, but I am going to task you with give me some wording. Do we just modify this bullet below of consider not specifying and add something to that and maybe broaden it, to say evaluate -- I don't know. Give me some wording here, Fred.

DR. SERCHUK: I think the second part is okay, about the BRPs, but the first part really relates, more or less, to the projections that I am concerned about, and I am thinking that we can do a post-hoc analysis, and let's go back and find out, if steepness was estimated for recruitment in the previous assessment, how well did that recruitment actually now align with the recruitment value that we think, and I know there's a difference in natural mortality, but there must be cases where we've used the same model framework, and we can evaluate whether it might have been better to just use an average recruitment or use an estimate from the stock-recruitment relationship. I think this is a research recommendation, in terms of an exercise.

DR. NESSLAGE: I feel like that's way broader than --

DR. SERCHUK: I know I didn't give you specific language, but I think the thought could be encapsulated by somebody that could craft the language, as long as you understand my idea.

DR. NESSLAGE: I think I do, but I guess I'm thinking this is a much broader question though than snowy, and I would love to hear from maybe Kyle or Erik or someone as to whether you guys have done some of this. You have thought through this, in how you present BRPs and how you structure most of the BAM assessments, and is this based on -- Go ahead, Kyle.

DR. SHERTZER: We didn't go back to the previous forecast specifically for snowy, but we have looked some at how well they perform after we have more information. In this case, the previous snowy forecast used expected recruitment from the Beverton-Holt, and then this assessment shows that the recruitment was lower than expected in the last few years. I guess, if that is a concern going forward in these forecasts -- That's the reason that Rob provided the forecast that used the low recruitment going forward, and so could be something to consider for setting short-term catch advice, similar to what was done with red grouper. I think that approach also addresses the concerns about autocorrelation in recruitment.

In other words, if you think recruitment now is an indicator of what recruitment will be next year, then those low-recruitment forecasts account for that idea.

DR. NESSLAGE: I totally see what you're saying there, and I think there's a broader issue though of the indices not being fit well that goes beyond the projections, and so I think it was really two different issues. Maybe I am wrong, but I agree with you that, if we are that concerned about it -- I am not sure are. Erik, what do you have to say about this?

DR. WILLIAMS: I think it's just the larger topic is really forecast models in general, and, more specifically, to I think what Fred was getting at, is forecasting recruitment specifically, and there's a lot of work to be done in that area, and we haven't done a lot of what probably could be done, and part of the reason is we don't have time, usually, within assessments, to do that level of exploration, but the other is we're just now getting to the point, honestly, with a lot of our assessments, where the time series are long enough that we can do things like look at ARIMA models or any kind of autocorrelation models, and we're also getting to the point where we're just now starting to pull in things like environmental factors and that. I mean, I see that as the larger subject, and it's finding a better way to do our projection analyses or, more specifically, forecasting recruitment, because, yes, admittedly, we do it in a very simplistic way right now.

DR. NESSLAGE: So maybe what we need to do is get rid of that first bullet, because that one is distracting me, the type of relationship, because it goes beyond that, I think, and we have the simulation thing below, and, under the consider stock-recruitment relationship, have a separate one that says something about being -- You can just put it as a placeholder, but just something about more broadly consider alternative methods for addressing recruitment assumptions and projections, something along those lines, but that would be across species in the South Atlantic, and that's probably not the best wording.

Even if that captured the idea, Kyle has brought up something that we have not discussed, which, if you could scroll up to the ABC control rule, and so we've got this P rebuild, but do we -- Are there numbers using that P rebuild based off of the recent R or the average R?

DR. ERRIGO: I was going to bring us back to this, after thinking about it some more, and P rebuild is something you can recommend to the council. They don't have to follow the recommendation though. What you do need to do is decide on which projections you think are best suited for snowy grouper for these next five years.

You can use the rebuild, the F rebuild, projections, which use the long-term average recruitment, or the average recruitment, because you wouldn't be able to get F rebuild if you didn't use the recruitment from the stock-recruit curve, and so you can use those, or you can use the projections at the lower recruitment values to get your ABC value for the next five years, and so that's up to the SSC, and there is a lot of uncertainty there, and so you have a lot of wiggle room to discuss.

DR. NESSLAGE: Let's have Fred Serchuk kick that off, because this was related to your concerns, I believe, Fred.

DR. SERCHUK: Sorry, Chair, and I was going to address another issue related to what Erik said. Could you allow me a second to do that?

DR. NESSLAGE: Yes, please.

DR. SERCHUK: One thing you can do, when you have a long time series, and I think we have what I would consider a long time series, and it's not a hundred-year time series, but it's forty-five years, and, particularly when you have the spread in recruitment that we've had over that period, you could divide your dataset and take the first half of that, or the first three-quarters of it, and then use whatever stock-recruitment relationship you get out of that and then predict what recruitment would have been in the years where you have data, but you have excluded it from the shorter model, and then see how robust that is.

That is a pragmatic thing that could be done very easily, I think, in terms of the adequacy of using a model-based approach for recruitment, rather than simply saying the best projector of tomorrow's weather is the weather we have today, which is what we tend to do when we say, well, we don't believe the stock-recruitment curve projection, and we're in a period of low recruitment, and let's consider low recruitment, and so I just offer that as something that could be looked at, I don't think with too much difficulty from an assessment point of view, particularly when you have a long time series. Thank you.

DR. NESSLAGE: So, scrolling back down then, because that's regarding the consider alternative methods for addressing R, give me some wording, Fred. I'm going to put you on the spot. You can't keep bringing up issues without giving me wording.

DR. SERCHUK: Evaluate the efficacy of estimation of recruitment from a model-based estimator by sub-dividing the data, the assessment dataset, assuming that you have a long enough time series that you can allow methods to be done and then evaluate the estimates of recruitment from the estimator, versus the realized recruitment that was seen in the full model, the full dataset.

DR. NESSLAGE: So it's kind of like a retrospective, if you will.

DR. SERCHUK: Exactly.

DR. NESSLAGE: That's assuming that the time series was long enough, which was Erik's point.

DR. SERCHUK: I only mention this because there was low recruitment, according to -- Well, not as low as it is now, but there was low recruitment in the late 1970s and the early 1980s, and perhaps not as low as it is now that we've seen, but it was low. Thank you.

DR. NESSLAGE: Okay. Thank you. Now back up to projections. Do we use recent, because we're not going to get that analysis before we have to make this decision. That's more of a future recommendation, and it's a good one, but, in the meantime, we have to decide do we recommend that recent recruitment levels be used or not? Fred Serchuk.

DR. SERCHUK: My recommendation, looking at the graph on page 72, and seeing that the last two datapoints that came out are way above the recent recruitment, poor recruitment, I would go with -- Those last two datapoints on the top that are close to the line there, those don't come out of the model, and they're estimated. I think they're so far above where recent recruitment has

been, and, looking at the progressions of most of the recruitment interannual variability, I would be more inclined to use the recent recruitment estimates from the model itself.

DR. NESSLAGE: All right. Let's hear from some other folks. There is a whole bunch of people who have not spoken much this meeting, and I would like to hear from you. This is a very important topic here. Everybody was so chatty in the breakout groups, and now you're quiet. This is really important, guys. Scott, please.

DR. CROSSON: What are you asking? I don't understand. Are you asking about whether we agree with the sentence? This is fine.

DR. NESSLAGE: Yes. Does anyone disagree? Okay. All right then. I believe that addresses it. Is that correct, Mike?

DR. ERRIGO: We need to know what values to project at. Is it the -- Do you want to use the calculated P* value at these low recruitment values? Do you want to use the F at 75 percent FMSY? I don't think you can use F current, but what values would you want to use? I think you can use this P* value at the low recruitment levels, and I think that would come out to be less than the F rebuild.

DR. NESSLAGE: Let's put that up as the strawman, and so P* 27.5 percent, with R from the most recent years being this 2011 to 2017 average, right, that's in the report, and see what folks have to say. Fred Serchuk.

DR. SERCHUK: Thank you, Chair. This gets back to your last point. I'm in the slow group. Is there any indication from any of the fishery-independent indices that either have taken place or will take place this year or next year or from these size/age compositions from the different sectors that there is any large pulses of small fish, indicating that recruitment is better than it has been? Do the people that collect the data have any idea on that?

DR. NESSLAGE: Rob or Wally?

DR. CHESHIRE: I haven't seen any of the more recent composition data, at least, and maybe Wally has an idea from maybe the MARMAP longline.

DR. BUBLEY: I can look really quickly. Nothing that I recall indicates a pulse recently, but I can double-check and see if anything has come up from that.

DR. ERRIGO: Wally, didn't you say earlier that the chevron trap was showing higher catches in the last two to three years and that the longline started to show a higher catch rate in this last year, something like that?

DR. BUBLEY: If you look at where it was, there was an uptick, and I don't know if they're related or not, but it just has potential to look like that, but it was just the start, and there was only one of increase in the longline, and it was a three-year increase in the chevron trap, like two or three years prior to that.

DR. ERRIGO: The chevron trap captures smaller individuals?

DR. BUBLEY: Correct.

DR. NESSLAGE: So I'm just going to throw in something. First of all, thank you for that, Wally and Rob and Mike, but correct me if I'm wrong, but we failed to rebuild the last time, right? Now we think the animal lives even longer than we did last time, and it's not like we're swimming in juvenile snowy grouper right now, and so, just taking my Chair hat off, I would say recent recruitment might be more appropriate for now, to actually try and achieve rebuilding, but that's just my guess. Does anyone disagree? I don't want to put a lot of hope in data we haven't even seen yet, to be honest, or vetted.

DR. ERRIGO: I just wanted to clarify that it's not that we failed to rebuild the last time, and we were making good progress towards rebuilding, which we weren't at the end of the rebuilding time period yet, but then everything changed with this new assessment, and everything was rescaled.

DR. NESSLAGE: Good point. We're not at 2039 yet, and that's true, and we are making progress, which is good, but we might want to let a few good year classes go through before we ramp up the ABCs, but I don't want to be heavy-handed. What do other folks think? Do they disagree with what's on the screen? I am fine if you do. Dustin.

DR. ADDIS: Can we be specific about recent years in this statement? Can we write it out explicitly?

DR. NESSLAGE: I was assuming that we would use the 2011 to 2017, which is what Rob had in his report, and is that correct, Rob?

DR. CHESHIRE: Yes. Those are the years that I used to get the average.

DR. NESSLAGE: Good suggestion.

DR. ADDIS: Thanks.

DR. NESSLAGE: Are you comfortable with that range, or you just wanted to clarify?

DR. ADDIS: I just wanted to clarify.

DR. NESSLAGE: Cool. Any objections or clarification or accolades? Okay. Mike, does that cover this section, do you think? Am I forgetting anything?

DR. ERRIGO: I think we now have it covered, and then as long as we're under the understanding that the OFL will be at the same low recruitment projection levels at a P* of 50 percent. Then this would be the ABC.

DR. NESSLAGE: It might be good to just jot that down, if you don't mind.

DR. ERRIGO: Okay.

DR. NESSLAGE: Excellent. Thank you. Okay. I think we're getting close. If we scroll back down, we were almost there, and we were at the research recommendations. We have cleaned up language on the various stock-recruitment issues, and then the last bit was operational assessment in five years, and you all seemed onboard with that. Is there anything here that folks disagree with or would like to see clarified, because they're not sure what exactly it means, and we can flesh out the language? Wilson.

DR. LANEY: No disagreement, but I just wanted to note that, with respect to the one that says investigate further whether there has been shore mode captures of snowy groupers, Chip did check the MRIP, and there were a couple of captures in there, and so we left it in there, for that reason.

DR. NESSLAGE: How about we change the wording to say investigate shore-mode captures, now that we know there have been some?

DR. LANEY: I will defer to Chip on that point, and that's fine with me, since there were some, but the scale-up on those was -- We were very skeptical that there were actually as many as MRIP scaled them up to be.

DR. NESSLAGE: Right. It's a good catch though.

DR. COLLIER: Just to let you guys know, what I did was I just looked at the MRIP data from the public website and looked at the shore mode and looked for B2s in there, and that's what I had sent to Wilson.

DR. NESSLAGE: Thank you, Chip and Wilson. Anything else outstanding on snowy grouper? I will do my best to flesh out the wording on this, so they're a little clearer statements, and you will, obviously, see this when you get a chance to review the report. No hands raised? No screams of protest or accolades at this point, and everyone is exhausted? Okay. Jeff.

DR. BUCKEL: Thanks. Just so I remember when we go to flesh it out, the point I brought up about simulation studies was with respect to BAM, and it just says "simulation studies" here, and I will forget when we go to flesh it out, and so if Mike could add -- If that captures what the group was getting at here, and maybe they were thinking broader simulation studies, but I definitely was thinking of one with respect to BAM. Thanks, Mike.

DR. ERRIGO: You're welcome.

DR. NESSLAGE: That's having to do with -- Remind me again? Now that I'm looking at it, you're right. I'm not sure.

DR. BUCKEL: Mike, do you still have the text from your first bullet?

DR. ERRIGO: No, but what I remember is it was a simulation study to look at which factors most influence the estimation of natural mortality in the BAM model.

DR. BUCKEL: Right.

DR. NESSLAGE: There we go.

DR. BUCKEL: Thanks.

DR. NESSLAGE: Sorry to be a jerk, but is it past tense? They didn't estimate M, right? So to do that in the future, if possible, and so do we want to just say "influence" instead "influenced", past tense? Is that what you're getting at, Jeff?

DR. BUCKEL: The estimation of M or if it's not actually estimating it, but the likelihood profiling that was done. It's something to help better -- I think that gets it.

DR. NESSLAGE: Okay. Now I am putting on my assessment hat. Now I'm more confused about what this actually means. What were you suggesting exactly again? I'm sorry. It's getting late.

DR. BUCKEL: Rob showed the likelihood profiling of M, and it was -- I think the negative loglikelihood was suggested that an M could go somewhere between 0.08 to 0.12, and those were all within the -- All had low negative loglikelihood, and so is there something that you could bring in? What data inputs could help improve reducing that uncertainty to -- Because it's broad enough now that that's a huge -- 0.08 to 0.12 has a big effect on the model results, and so we would be simulating input data, and would you be getting a better estimation of M if you had better age comps, or would it be better to have a better index, or do you really need both, just knowing that there's always a limited amount of money for the input data, and so which one would give you the best value to help the BAM model specifically better estimate M?

DR. NESSLAGE: I guess I'm getting hung up on the fact that BAM is not estimating M.

DR. BUCKEL: So whatever the correct language is for that. In the future, it might be estimating M, and I think that was mentioned for the likelihood profiling, to give the model more information for that likelihood profiling of M.

DR. NESSLAGE: Instead of influence the estimation, maybe better inform selection of M, or choice of M, for use in BAM, something like that.

DR. ERRIGO: Maybe that will have reduced the uncertainty in M?

DR. NESSLAGE: There you go. Sorry to belabor it, but I think I just wanted to make sure. Okay. Thank you.

DR. BUCKEL: Thanks for the clarification.

DR. NESSLAGE: I like that. That's good. Thank you, Jeff, and thank you, Mike. Okay. Any other last outstanding issues? I am not sure that we gained much in the way of time. We gained more in detail in our report though, which is interesting. We will have to revisit this tomorrow. I see the clock says 4:15 now, and we still have about forty-five minutes left before 5:00, and do folks -- I assume -- Kyle, are you the one who would be presenting on red snapper?

DR. SHERTZER: Yes.

DR. NESSLAGE: Okay. Do you still have the energy to at least get started on that presentation, and we can break and then revisit in the morning?

DR. SHERTZER: Sure.

DR. NESSLAGE: Would everyone like to take a five-minute biological break?

DR. SEDBERRY: Yes.

DR. NESSLAGE: All right. Thank you, George. That sounded definitive. All right. At 4:20, let's meet back, and Kyle will inform us about decisions on red snapper. Thank you, everyone.

DR. ERRIGO: Genny, one other suggestion, if you want to mull it over during these five minutes, is we could -- I think we can move the mutton snapper to right now and then have all of the rest of the time tomorrow for red snapper.

DR. NESSLAGE: We could do that, and that wouldn't take forty-five minutes.

DR. ERRIGO: Probably not.

DR. NESSLAGE: Then we could talk about the breakout groups too and see what people think, because we might be able to modify it for tomorrow.

DR. ERRIGO: Yes.

DR. NESSLAGE: This is a trial-and-error business. Okay. Kyle, would you be upset if you're bumped?

DR. SHERTZER: No, and I wasn't planning to talk until tomorrow morning anyway, and so that would be just fine by me.

DR. NESSLAGE: Okay. Then you think we can -- Hopefully we can get that all in, and we'll start right away at 9:00 and just start plowing through that. Great suggestion, Mike. Okay. Thanks.

(Whereupon, a recess was taken.)

MAKE APPOINTMENTS FOR THE UPCOMING MUTTON SNAPPER ASSESSMENT

DR. NESSLAGE: In the interest of time, I think we should probably keep moving here. Do we have Julie to walk us through this, or is this -- Who is the mutton snapper guru?

DR. ERRIGO: She's not online, but it's okay. We're really just looking for participants right now, and there's a memo that I can bring up. We're not going over the schedule right now, and we're just looking for people who are interested in participating in the SEDAR 79 mutton snapper assessment.

DR. NESSLAGE: Okay. So that's Attachment 4, and there's a number of different panels, correct?

DR. ERRIGO: Yes.

DR. NESSLAGE: But the schedule is starting in 2022, and is that right, if I remember correctly? Yes.

DR. ERRIGO: Yes. So the data workshop will be the end of January and the beginning of February, that week, in 2022, and so that's the data workshop. The assessment webinars are going to be held between April and September of 2022, and then the review workshop will be held the following January of 2023.

DR. NESSLAGE: Mike, I should stop you there, really quick. For those who didn't hear the final discussion before the break, we're changing up what we're going to do here, and we're going to try and knock off some of these smaller issues, and we'll do mutton snapper and a discussion of the breakout group effectiveness, and then we'll start fresh and furious first thing in the morning with red snapper and Kyle's presentation, and so just to update folks who might have missed that discussion. Sorry. Keep going, Mike.

DR. ERRIGO: No problem. So those are the approximate dates of when things are happening for mutton snapper. I see Julie Neer is here, and so she can help us with this, but I will just finish going through this, real fast. The data workshop panel is going to have ten participants, and we're probably looking for a couple of SSC members for that, and the assessment panel is six technical participants, and so probably two. Mutton snapper is not joint with the Gulf, I don't think -- It is. Julie, maybe you should do this then.

DR. NEER: All right. Hi, guys. I thought we weren't doing this until tomorrow, and so I had logged off to get on another call, but I'm back now. So mutton snapper is joint with the Gulf, but we have split the participants list, and so this is what we are looking for, a total of ten data workshop participants appointed by the South Atlantic, about six technical expert technical participants for the assessment stage of the process, plus a couple of industry reps or others to participate, and, for the review workshop, we need two SSC members, plus potentially up to two other industry reps, official observers.

One note about the review workshop is it is the South Atlantic's turn to provide the chair, and so, of those two SSC reps, we need a volunteer to serve as a reviewer and a volunteer to serve as the chair. The chair does organize and run that meeting when we're actually there and then help write the executive summary for that thing, for the review workshop, and so that stage. So that's -- I guess you probably want to know what the timing is for these, before you can all volunteer. We're looking for data workshop, and we'll be in late January or early February, January 31 through February 4 of 2022.

That week might slide a bit, because the Gulf Council has not set their 2022 meetings yet, and we obviously can't overlap with that, but that's what we're looking at, is basically the last week of January or the first week of February timing, and the assessment webinars will happen between April and say September of 2022, and the review workshop is January of 2023, and so it is a way

out, but we're starting in August, and so we couldn't wait until your October meeting to get feedback.

DR. COLLIER: Julie, just to clarify a little bit, when you say you're starting in August, that's starting with the original -- Like putting together the data and all those pieces, correct?

DR. NEER: Yes, and data scoping is the week of August 9.

DR. COLLIER: Thank you.

DR. NESSLAGE: Thank you, Julie. Fred.

DR. SERCHUK: Thank you, Chair. I just had a comment. When was the last time the mutton snapper assessment was conducted?

DR. NEER: That's a fine question. The last one was an update, and I believe it was -- Maybe it was 2018, or it could even be 2017, but the last actual full assessment was, obviously, even farther back than that, and I can look it up for you though.

DR. SERCHUK: Okay. Another question. Could this wait until our April or May meeting?

DR. NEER: Unfortunately, no, because, if you guys don't have your -- If you guys make recommendations in April, the council won't -- I guess, technically, if you wait until April, because then the council could make appointments in June, and the council is striving to only have SEDAR meet twice a year, in March and September, to help streamline their processes, which is why we were coming to you now, but, if you absolutely can't make recommendations now, then we'll do it later.

DR. SERCHUK: The reason I mention that, Julie, is I'm not quite sure what commitments I have, or many other people have, without looking at what we've signed up for already, and so I am reluctant to toss my hat in the ring right now without finding out what I have forgotten that I have tossed my hat in the ring for before.

DR. NEER: I understand.

DR. SERCHUK: I just think, if we wait until our next meeting, we would be in better shape, or I would be in better shape, for sure. Thank you.

DR. NESSLAGE: Although I think the list of who is on what so far is in our last report. Can we pull that up? Would that be helpful? I don't want to punt anything to April. Our April meeting is so packed to the gills that it's scaring me, and I don't -- No offense, Fred, but I really think we can't afford to do anything else at our April meeting. We already can't afford to do what we're planning to do, I think, and I'm worried, but it's a good point that we should look at what we've already committed to before we commit to anything else. Thank you. Dustin.

DR. ADDIS: The last mutton assessment was an update, and it was in 2015. The terminal year was 2013.

DR. NESSLAGE: Excellent. Thank you.

DR. NEER: Dustin got to it before I did, which clearly means that the last full-blown assessment, where discussions were had, was quite a bit before 2015.

DR. NESSLAGE: Indeed. Kathleen, do you have something to add?

MS. HOWINGTON: I just wanted to clarify that the SEDAR 76 black sea bass is no longer going to be complete in 2022, and it's going to begin in 2022 and end in 2023, and that was the assessment that got postponed due to COVID. So just keep that in mind while you all are looking at your schedules, is that's a year back now.

DR. ERRIGO: So there may be some overlap with the black sea bass assessment and the mutton snapper assessment, depending on the dates.

MS. HOWINGTON: Depending on the dates, yes.

DR. NESSLAGE: So be careful and don't double-book yourself, if you're already on those.

DR. NEER: The last full-blown mutton snapper was in -- It had a terminal year of 2006, and that was also not done through the SEDAR process. SEDAR only provided the review for that, and the State of Florida organized the other steps of that process.

DR. NESSLAGE: Okay. Does everyone have a snapshot understanding of remembering what they've committed to and have an idea of when that's probably going to pan out, regarding your time commitments? Any questions about that? If not, we can maybe go back to the -- You needed ten participants? No. That's total.

DR. NEER: Ten participants is total for data. That's also your state reps and stuff as well. Basically, we're looking for who might be interested, and the council, obviously, is the one who makes the ultimate appointments. It doesn't sound like we're going to have a problem with getting more people involved than we need right now.

DR. NESSLAGE: So we definitely need two though for the review workshop, correct?

DR. NEER: Yes. That is in January of 2023. I know it's far away, but --

DR. NESSLAGE: I am probably booked, Julie. I am sorry.

DR. NEER: That's why I've got to get in early.

DR. NESSLAGE: That's good. Alexei, what do you have to say?

DR. SHAROV: I would like to volunteer for the review workshop. I haven't done that many of them, and I liked the last one that I participated in.

DR. NESSLAGE: Great. Thank you. Amy.

DR. SCHUELLER: I was just going to say the same thing.

DR. NESSLAGE: Now who knows a lot about data for mutton snapper? We need some good folks on the data workshop, too.

DR. NEER: In particular, Florida knowledge would be extremely helpful. Just saying. Just so you know, both the data workshop and the review will be held in St. Pete, if that sways you. It's a trip to lovely St. Pete.

DR. NESSLAGE: Scott.

DR. CROSSON: You know, I've never done a data workshop, and I have done assessment and review workshops, and so I will volunteer for this, and maybe I can run over to the Salvador Dali Museum.

DR. NESSLAGE: Excellent.

DR. NEER: It is within walking distance of the hotel and the bar.

DR. ERRIGO: And it's either free or super cheap on Wednesdays, I think, just so you know.

DR. NEER: And it's open late on Thursdays, usually, and it's got a night show on Thursdays, I think, usually. Of course, you know, we'll have to check, but I can get that information.

DR. NESSLAGE: We still need a couple more folks for the data workshop and the assessment workshop. Eric Johnson.

DR. JOHNSON: I will do either of them, the data or the assessment. The assessment works better with my schedule, but the data is close and interesting, and I'm from Florida.

DR. NESSLAGE: It wouldn't hurt. Let's see where everyone else wants to go here. George.

DR. SEDBERRY: Well, I have no schedule, and so I'm available for either the data or the assessment, and I've done a little tiny bit of work with mutton snapper, mostly visual census kind of stuff, but wherever I am needed, in either of those two, I can help out.

DR. NESSLAGE: Jie, are you thinking assessment or data?

DR. CAO: I am happy to participate in the assessment panel.

DR. NESSLAGE: That would be fantastic, and so we've got at least one. Can people do both? Are people interested in doing both? It would be nice to have some continuity.

DR. NEER: Yes, people can do both between data and assessment, but just review participants can't participate in the other stages.

DR. NESSLAGE: Right, and so how does that affect your decision, Eric?

DR. JOHNSON: Sign me up.

DR. NESSLAGE: For both? Seriously?

DR. JOHNSON: Sure.

DR. NESSLAGE: All right. George, are you up for one or two?

DR. SEDBERRY: I can do two.

DR. COLLIER: Julie, did you need that many SSC members, or were you just looking for one or two for each of these stages?

DR. NEER: Probably usually a couple, and two or three is probably good for any of the stages, and I am assuming that Dustin will probably be there, though he may not have his SSC hat on, and I'm assuming, since it's his staff --

DR. ADDIS: Yes, and I will be there. I will be at all the workshops, but I will probably be wearing my FWRI hat.

DR. NEER: I am just saying that you'll have another set of SSC eyes on the whole process as well, and so this is probably a good list to work with.

DR. NESSLAGE: All right. Is that everything you need then, Julie?

DR. NEER: That would be great. If anything changes, we'll have to come back to you, and I know it is quite a bit out, and schedules and things change, but at least we can have something to bring to the council in March as a starting point, and, again, if something happens, we can always -- We have done the SSC shuffle on many assessments when we schedule this far out. I appreciate it. Thanks, guys.

DR. NESSLAGE: Understood. Thank you for your time, Julie. Sorry to drag you away from your other meeting.

DR. NEER: No worries.

DISCUSSION OF BREAKOUT GROUP EFFICIENCY

DR. NESSLAGE: Okay. Thank you, all, for your agreement to participate. We appreciate it. In the last few minutes then, I just want to talk about our attempt at increasing efficiency this week, or today, I should say. It seems like it's been a week already, and it's only a day. We had breakout groups, and I would love to hear people's opinions of how you felt the process went and if you have alternate suggestions or suggestions for improvement. The idea was that we would not spend so much time wordsmithing and we would get everything down on the paper faster, and was it enough time? Not enough time? Do you feel the product was better? Did we save any time? George.

DR. SEDBERRY: Well, you know, this was our first shot at this, and I thought it worked really well. I think we got much better active discussions in the smaller group and a lot more written down. As you already noticed, the report is going to be longer than it has been, but I think that's a good thing, and I think you have more to work with coming out of these breakout groups.

You can always edit it down, but it's often hard, after the meeting, to come up with details that you might want to remember, and so I think this provided a lot more detail and a lot more participation by everybody, and, to me, it worked out really well, and I think, as we found, we probably need more time in the breakout groups.

DR. NESSLAGE: Great. Thanks, George. Tracy.

DR. YANDLE: First, I completely agree, and I think the quality of what we're getting down is much better, and I think it's worth it for that alone. One thing I would suggest is, if we want to be getting more efficiency out of it, that's also going to require some discipline among all of us about focusing when to be making what type of comments and having what kind of discussion, and I think that was more a factor of this was our first time going through it, and we all need to learn the process, but I think it has potential, but it's also going to require us to impose some discipline on ourselves.

DR. NESSLAGE: Well said. Thank you, Tracy. Wilson.

DR. LANEY: Thank you, Madam Chairman. Yes, I think it worked fine, and I think our group came up a little short of time, and I think we actually had sort of a benefit in that respect, in that Alexei very graciously provided all of his recommendations to us in writing ahead of our breakout group, which really helped, because we had a list that we could go through, and we were fortunate in that he was able to join us too, earlier than he anticipated, and so that was good, but, in terms of going back over the notes from our discussions all morning long, I'm not sure that we caught all of them, and I still feel a need to go back through my notes and make sure that we captured everything that had been previously discussed, but, given that the entire SSC had a chance, as we were going through the details, to capture anything we missed, then I'm feeling pretty good about them, and so I like the way it worked, and maybe we could use a little bit more time the next time, and we'll see.

DR. NESSLAGE: So, Wilson, to that point, I guess I'm wondering -- I think George, or maybe it was Marcel, added this last section of our agenda, where we re-review the consensus statements at the end, just to make sure we're all on the same page before we break for the meeting, and I am wondering how much of that -- Do you think it would be worthwhile still doing that, or is it going to be overkill? I would love to hear from folks. Like, if you think we missed something, is it worth having a chance to compare with our notes overnight and look at it one more time, or is it just tedious to do that again if we've had this process and it goes similarly in the future? I guess I'm asking Wilson, but everyone in general, too.

DR. LANEY: Well, I don't know. Given -- I would defer to the rest of the group on that. Given the fact that we did have a good bit of time in the breakout groups, and then we went through them all again, once Mike had compiled them all on the screen, I would think at least that, if we elect to keep that piece of the agenda and go through them one more time, at least it should go a lot more quickly than maybe it has in the past, but I will defer to everybody else on that point.

DR. NESSLAGE: Thank you. Fred, go ahead.

DR. SERCHUK: Thank you. I am with the others, and I think it worked out well this time. We could have used a little bit more time, but I think we hit all the major points. What I am concerned about is that I think it worked well because we had one item on our agenda today, one assessment. In future meetings, we often have more than one assessment, and I am not -- I'm uncertain whether we'll get the same level of detail with multiple assessments as we've had today, and I say that for a couple of reasons.

Going through a large report takes time before the meeting, and it also takes time during the meeting, because you go back and refresh yourself. If we had more than two assessments at our April meeting, I don't think it would work. I just think the amount of time that you have to spend, and also the amount of information in these reports to digest, that might be a limiting factor on whether we could go to the breakout groups.

In this meeting, we have this big report, and we have this item that we just took, and then we have a workshop report, which doesn't really require the same amount of interaction, but, if we have to go through more than two reports in a meeting, even in a full two or two-and-a-half-day meeting, I think that might tax our abilities. Thank you.

DR. NESSLAGE: I am glad that you brought that up. I was thinking the same exact thing, and so an alternative that had gone through my mind is that that breakout group work kind of becomes homework, and so, right now, I had you guys individually address sub-sections of this agenda item. In the future, like at our April meeting, where we have I think three assessments, plus one or two other issues we have to deal with -- Chip, correct me if I'm wrong there.

We may have to do this offline, via email, or sections get assigned to individuals, and they have to send that back to Mike or me for compilation and discussion at the end, and I think that's the only way we're going to get through this, and so that's -- Think about that, and I would love to hear your feedback on that one, and I don't like to give people homework, but I feel like there's no way that we're going to get through our April agenda if we don't do that. Wally.

DR. BUBLEY: I mean, I am basically just saying what everyone else has been saying, and I think it's a good idea. Obviously, a little discipline would have helped, being this is the first time, but we were about twenty minutes in and realized that we had a lot to cover, and so there was a little scramble at the end, but, in terms of the agenda item at the end to go over consensus statements, I mean, I think it's good to have it there, and not necessarily that we actually have to go over them statement-by-statement, but just an opportunity, if someone has looked over their notes and to address it, they can do that, but we don't necessarily have to go through it step-by-step, if we've already gone through it a couple of times already at that point.

DR. NESSLAGE: True. Thank you, Wally. Anne.

MS. LANGE: I agree with Wally on that. It's good to have that at the end, in case people haven't had a chance, as Wilson indicated, overnight to think about something to add in, but we don't have to have it, and so it shouldn't be considered by the public that every single bullet is going to be hit upon, but it's just one last chance for the committee to go through and add any updates.

The other thing, relative to what you just mentioned, Genny, about assignments is, back in the day, years ago, what we did was each person was assigned a section, to provide the summary, and it was a lot more written, and it wasn't the bullets that we use now, and it was actually paragraphs and full sentences, and so I don't know that putting together a bulletized section as homework, in those times where we have more assessments or more discussion topics, would be out of the norm. Anyway, I would be amenable to that.

DR. NESSLAGE: So you're saying it's a little bit easier, given that you don't have to write an epistle, and you just have to come up with these bulleted statements, but it's worth pursuing the homework idea?

MS. LANGE: Yes, if we have more than can be accomplished reasonably during the meeting, and I think that's a reasonable thing.

DR. NESSLAGE: Okay. Thanks. Fred.

DR. SERCHUK: Thank you. The comment that I am about to make may sound like I am ungrateful for serving on this committee, but I am not, and I, along with many others, spend many hours before the meeting going over the documents, and, of course, we spend hours after the meeting, or some time after the meeting, reviewing the reports and so on and so forth.

I was a little bit taken aback when we got this memo about we're not going to be paid for any time outside out of the meeting hours, and, again, I am retired, and I've got enough to get me by, but I am just feeling that, if you give us homework, and we have to do it on our time, and it's critical, then you either should pay us by the meeting, and not by the hour, because, quite frankly, we all put in many more hours at the meeting and preparing for the meeting and after the meeting than we're compensated for the meeting time.

If we're not going to be compensated for working after the meeting, during the meeting itself, then I feel a little bit that we've been taken for a ride here, and I know that sounds like I'm not -- That it's out of place, but I was really taken aback by the memo that we got back some time ago about a chance in practice about how we account for our time, and, when you mentioned that you were looking for us to work explicitly on homework during the meeting, it just raised an issue in my mind. Apart from that, I agree with everybody else, and I'm sorry if I offended anybody. Thank you.

DR. NESSLAGE: You have not offended -- Well, you have not offended me, and I'm glad that you brought it up. This is the time for us to have these open discussions, and I think we need to be frank, because, ultimately, if people feel they are being abused, they are going to quit, and then we won't have anyone to do this work. I think staff and council, and hopefully Steve is still on, will hear that.

I think we need to be careful, and I will tell you all that it was made very clear to the council, and Steve Poland is very much onboard with trying to communicate to the council, that we are at max capacity right now with regard to what we're being tasked with, the number of assessments, in particular coming up in April, the number of items that we're asked to review. While we definitely appreciate being asked, and we like providing this type of feedback, and we appreciate being asked

by the council, but I think we don't have enough time right now to deal with all of these issues adequately, and, when we start to rush, we miss things, and these are very important stocks and very important issues, and so there needs to be a balance.

At some point, like there was at the last meeting, we just simply did not get to certain agenda items, or portions of the agenda, and our report went short, and so it was very clear to the council that that's happening, and will probably happen again in April, regardless of whether we have homework or not, and so that's just something that I wanted to share with you all. They are aware of the issue, but, as far as I know, nothing has been taken off our plate.

As Chair, I am struggling, but, if there is significant pushback, and I would totally understand if there was, I would like to hear that, and when you tell me this on the record, or offline, just as a member, or if you talk to staff, I think that all of that feedback is welcome and appropriate. George.

DR. SEDBERRY: I just wanted to say, to Fred, that, no, I'm not offended, and I agree with what he's saying, and with what Genny has said too, and I'm not a mercenary either, and I've got time to do this, but our time should be valued, and keeping track of it to the minute, or quarter of an hour, while things are being moved off the schedule, or off the official webinar time, and the work is increasing, but the official time is not changing, it just strikes me as a little strange, and I know the council doesn't have any bad intent with this, but the appearance of how it values our time -- It rubs me wrong, and I'm not saying this very well, but I am glad that Fred brought it up, and I think the council needs to be aware of these things and think about it.

DR. NESSLAGE: Thank you, George, and, honestly, this was just me brainstorming, and I will say that I'm guessing that John and staff will push back, because then it won't be, as we did today, available for the public to attend, and we won't have recordings, et cetera, and we'll just have the output of the individuals, but, honestly, I don't see how we're going to get this done otherwise, and so this is the tension that we're dealing with, and I don't want people to become miserable either. Anne, what do you have?

MS. LANGE: I just wanted to concur with both Fred and George. I was a bit taken aback by the -- For those who are full-time employees with either a state or the federal government, you may not have heard what has happened, but, about a month-and-a-half ago, we got a letter saying that, instead of being paid the $125 per four-hour or three-hour session, it was going to be done based on an hourly rate, based on up to fifteen minutes per -- It was a breakdown of quarter hours, and so it significantly impacts the actual amount, and, again, none of us are looking for big amounts of money from this, but it did strike me as sort of like what have we done that -- We seem to be doing our work, and trying to contribute, but it was a surprise, and so I agree with what both Fred and George have said.

DR. NESSLAGE: Chip.

DR. COLLIER: Well, I see John has his hand raised, and so he's probably going to be the better one to respond, but I will let him go ahead.

DR. NESSLAGE: Sorry to put you guys in the hot seat. John.

MR. CARMICHAEL: Hi, Genny. Thanks. I will say, folks, that I appreciate the feedback, and I will say it was certainly not the intent of the council, in doing any of this, to imply that your efforts weren't valued and your time wasn't appreciated and you weren't due what you are due for this. Really, the whole intent of it was just to make it a little bit simpler in terms of dealing with these webinar meetings of various periods of time that we're doing so many more of now.

There was not really a clear policy from the council in dealing with the shorter meetings and the partial-day meetings, and an informal decision was made just to draw a line at four hours, and we realized, well, that kind of made a difference with different-time meetings and such, and so we felt the simplest way was just to say, look, it's an hourly rate. If your meeting is three hours or two hours or five hours, et cetera, then just put down for that amount. If it's a matter of the quarter-hour seems too fine, I think we can easily change from that and let you round it to the hour, if you would like, if that would make a difference.

The idea of homework was not something that was happening when this was made, and that seems to be a new thing that's coming up, and so perhaps that can be factored into some way of looking at this, and I'm not sure how, and, as we do these breakout groups and such, as mentioned, that's all regular time, regular meeting time, and that's all done on the record and available to people in regular webinar, and so I just want to try to reassure folks that it wasn't intended in any way to be a slight to members, and, if we need to do some things to tweak it to avoid that, then certainly we can do that.

MS. LANGE: Thank you, John.

DR. SEDBERRY: Thank you, John, and, again, as Fred said, I enjoy doing this, and, really, it's an honor to be chosen by the council to do this kind of work for them and to really help out in managing our fisheries, and it's an enjoyable task, and so that's what made it all a little more -- I don't know, but a little more shocking to me when this happened, is that -- This, to me, is a valuable thing for me, and for the council, and it just kind of struck me wrong, and so, anyway, I appreciate John's comments there, and so it helps me to understand what the council's thinking was on this, rather than for it to just suddenly come out of the blue, and so thanks, John.

DR. NESSLAGE: Steve.

MR. POLAND: Thank you, Madam Chair. Good evening, everyone. I just wanted to follow-up on John, and, from the council's perspective, we certainly appreciate the work that the SSC does and the integral part you all are to the fishery management process here in the South Atlantic. The council had no ill will or ill intent from modifying our reimbursement policies, or our pay policies, and, with the transition to more webinar meetings, and just the world of COVID that we live in now, we felt like some changes were made, and the same kind of pay structure for the SSC is the same pay structure that the council members get paid, or paid under. Not all council members get paid, and the state reps, including myself, it's all part of our job, but the appointed members do get paid, and so it's the same for them as it is for you guys.

The concept of homework, and I do apologize, and I've been in and out of the meeting all day, with other meetings here for my day job, but I'm kind of at a loss from where the homework idea came from, and that's not something that the council has discussed, and I'm not sure, and, again, John can step in and respond if staff had discussed this, but the council didn't get to the level of

detail, as far as not necessarily dictating, but suggesting to the SSC on how to streamline meetings or be more efficient like that, and so I will just say, from the council, I certainly don't expect SSC members to do a significant amount of work off hours, and air quotes on things like homework, and not be compensated. Certainly, if there's a way to do the work that you do during SSC meetings, and we have no expectation or intent to ask you guys to do anything more or anything less. Thank you, Genny.

DR. NESSLAGE: Thanks, Steve, and so just to clarify. The homework idea was my off-the-cuff statement that I made earlier today, and no one has talked about it, or discussed it, at the staff or council level, but I brought it up because I honestly don't see any way that we're going to get everything done in April without some work outside of the meeting.

Now, we already do an extensive amount of work outside of the meeting, both prepping for the meeting and in revising the report. If we don't come up with consensus statements at the meeting before we break, we're doing it over email, and that is far less transparent, and it sounds like -- Correct me if I'm wrong, Anne or George or anybody who has been on the SSC for a long time, but, previously, you would have drafted this stuff on your own. It sounds like you did some of this drafting on your own time, either after the meeting or in the evenings, and is that correct? I mean, there's -- You weren't drafting while you were sitting at the table at the meeting, were you? Anne, can I call on you?

MS. LANGE: No, we would do it at home. We would have all of our notes and everything, and it would be reviewed by everyone, to ensure that, as here, there weren't things that the writer put down that weren't actually discussed, and so each person would draft their section and turn it in, and then the whole document would go out for review, and staff, as well as other members, would note if that wasn't actually discussed, and we would put that in for future discussion, and so it wouldn't be included as part of the meeting report if it hadn't actually been fully discussed. Does that answer your question?

DR. NESSLAGE: Yes, it does, and so I see that -- At least, when I first came on as Chair, there were questions about what was discussed at the beginning and what wasn't, and we ended up having over -- What was it? Church counted them up, but like a hundred emails within the week after the meeting, just resolving that one or two issues that we had, as to whether we agreed on certain points about the consensus statements.

I don't want to go there, with the volume of work that we have to complete for this upcoming report, and so doing that stuff offline, without checking base with each other before we break at the end of our April meeting, is nightmarish. I can't imagine trying to do that, personally, and so that's where I came up with the idea of homework, and it is not something that has been vetted by anyone other than my little brain, and so please don't get worked up, and maybe even just strike it from the record there, because I am worried that people are going to get too worked up about it, but, Chris, go ahead.

DR. DUMAS: A few things. One is the issue of time, and the other is the issue of compensation, and, on the issue of time, it seems like these meetings are taking more and more time, and there's only -- For those of us, most of us, with other full-time jobs, then the time I spend on these meetings ends up being taken away from family time, and I am getting firm pushback from family that no more family time is available, and so one is just the issue of time.

On the issue of compensation, if you wanted to have some of the time be homework time, is it possible to define the meeting time as also including the homework time, and then the time you want people to do homework -- Say that that's time where people will meet in individual breakout rooms for that period of time, and so, if you want an eight-hour day, plus two hours of homework, just define the meeting time as ten hours. I'm not sure if that's administratively allowed, but, if so, let's just estimate how much time we're going to spend on homework and add that time to the meeting and define the meeting as that amount of time. Thanks.

DR. NESSLAGE: That's a great suggestion, Chris, but it just means we have to be brief in our discussions, which we tend not to be, and I think we have really good discussions, and some of our best comments come out of discussions with each other and challenging each other, and so I worry about that, but I agree with you that we could simply just say, okay, break, and go to breakout rooms and come -- Make that part of the meeting time, less discussion and more drafting. That's definitely an option. George.

DR. SEDBERRY: I just want to elaborate a little bit on what Anne said about consensus statements and recommendations and the really meaty parts of the report, and it has varied over the years, how we did the report, and Anne is right that I can remember years that we -- That assignments were made and we wrote those sections of the reports on our own after the meeting was done, or we wrote them during the meeting, but they really weren't compiled until after the meeting was done, and so, a lot of times, I think it fell on the Chair to decide what was actually discussed at the meeting and what was actually decided at the meeting, versus what was kind of added in as afterthoughts by some of the participants, and so that falls on you, Genny, which makes it more difficult.

I think that, the more we can done at the meeting, on the record, in front of everybody, the better off we are, and, if that takes more time -- It doesn't matter whether the time is spent on your own or in the meeting, but it's just time, and so we may as well spend the time at the meeting, even if it makes for a long meeting, to get those things done in a public forum on the record.

DR. NESSLAGE: Thank you, George. Fred.

DR. SERCHUK: Like Anne and George, we are delighted, and perhaps Church and other retirees, we are delighted to serve on -- One, to be selected and serve on the SSC, for two reasons. One, we believe we have a lot to contribute, based on our experiences in our careers, and, second of all, we have been around a long time, and we've seen a lot of things, and council members can serve a maximum of nine years at a time. Somebody that spent their whole life in fisheries can spend up to forty or fifty years interacting with management councils, and so we've seen many things, and we hope we have learned from our experiences, and we hope that those experiences can be brought to bear on the events that the SSC has been tasked to provide their input on.

None of us do it for the money, believe me, and we do it because we feel we have to give back, and to parse things out as if we were sort of common laborers -- I know that's not what we are, but to talk about fifteen-minute intervals or talk about, well, we can't -- Unlike other SSCs, which get paid by half-day meetings or full-day meetings, a certain rate, to get the accounting on a short-term hourly-by-hourly, or fifteen-minute basis, is something that we're not used to, quite frankly.

None of it includes, as I said, the amount of preparation time beforehand, particularly because it takes quite a bit of time to go through the documents, but we've never asked to be compensated for that time, and then to come to an accounting system that looks like you have to cross the T's and dot the I's is simply a bit unsettling, and I know there are probably good reasons for doing it, but it's a way that I have never been compensated before, and I am not compensated that way on other SSCs, and I just feel that -- It's something that professionals are not used to facing. Now, maybe the economics of it require it, but it was a bit of a -- A bit of unexpected communication. Thank you.

DR. NESSLAGE: Thank you, Fred, and I recognize that we are now at 5:12, and we have gone over, and we are now firmly into family time, as Chris would put it, and so I think we should probably wrap up, and I think we have hit on some key issues and feedback, and staff, the Executive Committee, and I will definitely work with council members and council staff to come up with a plan of action for April and hopefully continue to improve our efficiency and respect everyone's time at the same time, and so I do appreciate everything you guys have done today.

Thank you so being so game in trying this alternate method. We'll take another crack at it tomorrow morning with the red snapper issue, if we can, and see how it goes, and we will see you all back here at 9:00 a.m. to launch into our discussion of red snapper. Thank you, all, very much. Have a good night.

(Whereupon, the meeting recessed on January 11, 2021.)

- - -

JANUARY 12, 2021

TUESDAY MORNING SESSION

- - -

The Scientific and Statistical Committee of the South Atlantic Fishery Management Council reconvened via webinar on January 12, 2021 and was called to order by Chairman Genny Nesslage.

DR. NESSLAGE: Good morning, everyone, to day two of our January SSC meeting. Before we begin with our last agenda item, which will be red snapper, Agenda Item 4, I just want to do something that I should have done yesterday, but I was remiss, and I completely forgot, and I apologize for that, and that was I did not thank Rob Cheshire for all his hard work on the snowy grouper assessment and in answering all of our questions yesterday.

You probably couldn't tell, but that was his first assessment, and I thought he did an amazing job, and I think we should all give him a round of thanks and virtual applause for his great work, and so thank you, Rob. I hope you're on. At least I thought I saw you. Yes. So thank you very much, as well as Katie and the rest of the Beaufort Team, for supporting that assessment.

Without further ado, I believe we are on to Agenda Item 4, and so we are to receive an update on some of the data workshop decisions that have been made in SEDAR 73 for the red snapper assessment, and I would draw your attention to Attachment 3, which is the progress report, and

the PowerPoint that Kyle will be presenting to us, and I'm going to hand this over to staff to give us a little bit of background.

UPDATE ON DATA WORKSHOP DECISIONS IN THE SEDAR 73 RED SNAPPER ASSESSMENT

DR. ERRIGO: Thanks, Genny. Kyle is going to give a presentation on the data decisions that were made during the data workshop for red snapper, as well as the final decisions for the selectivity workshop that we had before the data workshop, that was conducted before the data workshop, for the trap and video indices. After that's all done, we'll have our discussion, and I will help frame the discussion, after that's all done, so it's fresh in our minds, of what exactly it is that we're supposed to discuss and comment on, if that works. Right now, I will pass control over to Kyle, if he's ready.

DR. SHERTZER: I'm ready.

DR. ERRIGO: All right Here it comes.

DR. SHERTZER: This is not exactly review, but more of a discussion to let you know about the decisions that have been made by the assessment and data panels so far, so that we can get your feedback and input before it actually comes to you for review in your spring meeting, and this is not a usual thing to do, but red snapper is not our usual species.

Just to put it in context, I thought I would first review the SEDAR 41 red snapper data, and then discuss the new data considerations, which is really the meat of this talk, and then let you know about a few of the modeling efforts that have happened so far and some of the modifications to the model, but, starting with the review, the SEDAR 41 assessment had three different growth curves. It used a population growth curve to describe life history characteristics, and then it had two different fishery growth curves, one to represent fish during the twenty-inch minimum size and another to represent fish from outside of that twenty-inch minimum size limit, just to account for the differences in the sizes of fish that would be kept inside and outside of that size limit and how that might also differ from the population at large.

It used an age-based natural mortality, which I will talk about a little bit more later, during the new data section. It modeled the spawning biomass as population fecundity, and so it used the time of spawning at the peak spawning in mid-summer and applied a 50/50 sex ratio, which was supported by data, a logistic model for female maturity, batch size as a function of body size, of length, and then there was an age-specific number of batches produced per year.

The fleet structure used in the model, there were three different fleets that were modeled, commercial handline, which also included commercial landings from any of the other commercial gears, and there are two recreational fleets, the headboats and then general recreational, which included private and charter boats. Then landings and dead discards were modeled separately, and so there were six fleets that were modeled in the assessment.

These are the commercial landings in thousands of pounds from SEDAR 41, and then also the SEDAR 73 landings. In the plot, you can only see the SEDAR 73 landings, because it overlaps

precisely with the SEDAR 41 landings, and so it just adds the additional years, and the terminal year in SEDAR 41 was 2014, and the terminal year in SEDAR 73 is 2019, and so it includes the additional five years, but is otherwise identical to the previous time series.

The commercial discards are very similar, with the addition of the new years, and they are a little bit different, because these are estimated as a model-based values, and so there's a little bit of a difference here, but it's very similar to what we had in SEDAR 41. The general recreational landings are, like most of the revised MRIP landings, larger than they were previously, and so that's just the revision to MRIP that accounts for most of the difference here.

Going back in time, the historic landings, prior to 1981, were computed using the FHWAR method, and then that was just for total recreational landings, which included headboat, and then headboat and general recreational were split using a ratio of the landings in the early part of the time series, from 1981 up through I think the first three or five years. Then the general recreational discards, again, are larger, because of the MRIP revisions, but then there's a marked increase at the end of the time series.

Headboat landings, aside from the historic period, are identical to what we had for SEDAR 41, and you can see a little blip in the terminal year of 2014, and I think that was a transcription error in the previous assessment, and so the true value is what is shown for SEDAR 73. The difference in the historic period is due to applying that ratio, and so, now that the MRIP landings are quite a bit higher, the ratio tilts the historic landings more towards the general recreational fleet, so the headboat landings in the historic period are lower. I will come back to this later, to show you that it doesn't matter. Then the headboat discards are very similar to what we had before, with the addition of more years at the end.

SEDAR 41 applied two different time periods for discard mortality, and the first period was for j-hooks, and then, when it was believed that circle hooks became more prevalent, the discard mortality was reduced, and we had separate values for commercial and for recreational, but, in this case, the recreational was applied to both headboat and general recreational discards, and we do revisit this with new data, and so I will talk more about this later as well.

The comps that we have available for SEDAR 73 are following what was done for SEDAR 41, with a few exceptions. We have length comps for commercial landings, 1984 through 1992, and commercial discards, which, because of low sample sizes, were pooled into the periods prior to 2010 and then after 2010, when the moratorium started, and then headboat discards we have starting in 2005 through the end of the time series, and then we now have some information on general recreational discards, the lengths, and that is a data source. There are a couple of new data sources that I will discuss later.

For ages, we have commercial landings, ages starting in 1990, and headboat landings are starting in 1978 and general recreational landings starting in 2001, and then we have a SERFS chevron trap ages starting in 2010. A decision was made to give preference to ages -- If we have years where there is both ages and lengths, to give preference to the ages, and that actually dates back to SEDAR 24, and that was carried forward in SEDAR 41, but that was a recommendation from one of the CIE reviewers that we had for SEDAR 24.

We had three fishery-dependent indices of abundance, the headboat logbooks that runs from 1976 through 2009, and the commercial handline logbooks that are starting in 1993 and ending in 2009, and then we have the headboat discards from observers, and this is just for fish that are smaller than twenty inches, and that is -- That extends from 2005 through 2019, and so we think of that one as more of a recruitment index, because it applies to the smaller fish.

The logbook indices, the headboat and commercial logbooks, were truncated in 2009, because of the regulations that started in 2010, and then, for this assessment, as in the last one, we have two fishery-independent indices of abundance from SERFS, and we have the chevron traps and the video index, and, in the previous assessment, those were combined, because of the non-independence in sampling, and the videos are placed on top of the traps, and that is something that we're revisiting for this assessment, and I will talk more about that later as well, but, for the videos, for SEDAR 41, it started in 2010, but we reconsidered that here and started the index in 2011, and that's another thing that I will discuss later.

Then this shows all of the indices plotted together, and it's fairly high correlation among the indices, and so that's nice to see, and also unusual in the Southeast, and even the peak in the headboat at-sea observer, around 2008, that precedes the peak in the headboat and handline index indices, are in agreement, because the headboat at-sea observer data is the smaller fish, and so there's a bit of a lag between those indices, but they are correlated. I think that's maybe a good place to pause and see if there's any questions or comments on SEDAR 41 data sources or any of the new pieces that I have mentioned.

DR. NESSLAGE: Great. Thanks, Kyle. Are there questions for Kyle on data sources and decisions?

DR. ERRIGO: I am not seeing any hands. Chris Dumas.

DR. NESSLAGE: Chris, go ahead.

DR. DUMAS: Good morning, folks. Kyle, for the revisions in the MRIP data, the changes in the MRIP numbers, are those available by mode, and do you recall -- Were there particular modes that drove the large changes?

DR. SHERTZER: They are available by mode, and I don't recall. My gut is telling me that it's the private sector that is driving the changes, but I haven't verified that.

DR. DUMAS: Thanks.

DR. ERRIGO: We have Jeff Buckel.

DR. NESSLAGE: Great. Jeff, go ahead.

DR. BUCKEL: Kyle, could you go back to the commercial discards, please? I am curious about your thoughts on -- When you look at the discards in the recreational fishery, and I think that's just showing an increase, similar to the other fishery metrics, and I am curious your thoughts on this decline with the commercial discards from it looks like 2016 forward.

DR. SHERTZER: I guess I can speculate that commercial fishermen are better able to target red snapper, and the flip side of that is better able to avoid red snapper when they're not in season, or after the quota has been met.

DR. BUCKEL: Okay. Thanks. I don't know if there's any commercial folks online, and I would be interested in their feedback. Locally, I have heard commercial folks -- That they're trying to target certain species and can't get away from the red snapper, and so I was just interested in that disconnect, or maybe you're right that they're able to -- Even though it's lower, and you're not seeing an increase, maybe they are still getting quite a few, but I'm just surprised at the number. It's lower than I thought, and the trend is opposite of what I thought.

DR. SHERTZER: It's an interesting pattern, I agree.

DR. ERRIGO: Fred Serchuk has his hand up.

DR. NESSLAGE: Great. Go ahead, Fred.

DR. SERCHUK: Just a question on the all indices, the last graphic put up. I am presuming that each of the series has been scaled against its mean, and is that correct?

DR. SHERTZER: Yes.

DR. SERCHUK: Because there is nothing on the Y-axis.

DR. SHERTZER: Yes, that's right.

DR. SERCHUK: Okay. Thank you.

DR. ERRIGO: Fred Scharf has his hand up.

DR. NESSLAGE: Fred, go ahead.

DR. SCHARF: Kyle, I was just wondering. In the most recent SERFS fishery-independent data, is there -- Are they looking at sex ratio in that data? I just wonder if there's recent information to inform the assumed 50/50 sex ratio.

DR. SHERTZER: We did not revisit that during the data discussions. I guess I'm not sure if any of the MARMAP folks are online and if they have any comment on that, if they've re-looked at those data.

DR. BUBLEY: Offhand, I can't say, but we do collect the sex information. We typically collect histology samples, and, for some years, we haven't collected those, but we do look at sex and go from there, and so it's available, but I don't think there was anything that was leading us to think there was any difference, but I would have to go back and look.

DR. SCHARF: Okay. Thanks.

DR. ERRIGO: I am not seeing any more hands raised.

DR. NESSLAGE: All right then. Kyle, do you want to keep going?

DR. SHERTZER: Yes, I do. For this next bit of discussion on the new data, it might be better if we take it piece-by-piece, and, if there are questions or comments, to take that as they come, rather than wait until the end, because there's quite a bit to discuss here.

The first part was life history, and batch fecundity and natural mortality were revisited, and then indices of abundance, and there was discussion about trap and video as separate time series, and also the video start year. Then we had a new index to consider, which was the FWRI repetitive timed drop survey, the hook-and-line survey, that they conducted, and then we have quite a bit of new information on discard length compositions for commercial and headboat and recreational, and then we also revisited discard mortality and the effect of descender devices on discard mortality, but, starting with life history, batch fecundity was revisited, and there was an additional twenty-eight fish that were added to the dataset.

SEDAR 41 used a two-parameter model, a power function, to describe fecundity, based on mean length at-age, and that relationship was updated, and there was also a three-parameter power function that was considered, and the plot here is just showing the two-parameter and three-parameter fits to batch fecundity as a function of total length, and they are fairly similar. The differences are primarily for the smallest fish and the largest fish, and the three-parameter function -- It's hard to see in this plot, because of all the data points at the smaller fish end, but it did a better job at describing fecundity of the smaller fish, around the 400 to 500-millimeter range. The three-parameter model had a better pattern in the residuals there.

There is a working paper, if you care to look further into this, that was provided by MARMAP, where they fit the three-parameter function, and they recommended that three-parameter function of length, because of that issue and better describing residuals discussed in the previous slide, and they also provided a two-parameter function, based on age, and the working paper recommended using, for the smaller fish, a mean observed batch fecundity, and that's for sizes less than 400 millimeters, because the three-parameter model predicted negative fecundity for those smaller sizes, but otherwise described fecundity better for the larger fish, larger than 400 millimeters, and so that really only applied -- That cutoff really only applied to age-one fish, given the growth of red snapper.

The panel decision, the assessment -- The data and assessment panel, during the workshop, decision was to use the three-parameter function of mean total length at-age for ages-two-plus and then apply the observed batch fecundity to age-one fish, or fish smaller than 400 millimeters. This isn't a large change, really, and it's a small change from SEDAR 41, but mostly it just has some new data and a slightly improved model, a three-parameter model instead of a two-parameter model, for batch fecundity. Any comments or questions on batch fecundity? Then moving on to natural mortality --

DR. COLLIER: There is Chris Dumas and then Fred Serchuk had questions.

DR. SHERTZER: Okay.

DR. DUMAS: Hi, Kyle. Yes, on the batch fecundity, could you back up one slide to the graph with the data points? Looking at that, it appears there is clearly heteroscedasticity in those data points, and so you might want to estimate those functions in logs, to help reduce that, or you can weight -- You can use a weighting scheme to correct for heteroscedasticity.

In the next slide, and so the second BF function, the second BF function, you could log both sides of that and do that, as an OLS regression on log BF, and it would be a two-parameter function, but add in logs on both sides, and that would help with the residual slide, and you may have done that, and I don't know, but I am just suggesting it, or you can adjust for heteroscedasticity in other ways, but that's something you probably want to look at, because that's sort of got the classic pattern in the data points. Thanks.

DR. SHERTZER: I will have to look back at the working paper to see if they took logs for fitting, and it's a good point that you make, and they may have, and I don't recall off the top. Wally, do you happen to remember?

DR. BUBLEY: I know we've done it for other species, and so I'm assuming we did, but I would have to go back and look as well.

DR. DUMAS: Thanks.

DR. ERRIGO: Then Fred Serchuk has his hand up.

DR. SERCHUK: I am just wondering, and were the batch fecundities also scaled by the relative maturation ogives? That is, you talked about these are the fecundities based on size, but are all fish age-one and older completely mature?

DR. SHERTZER: It does get filtered through. When it gets applied to compute population fecundity, the way that operates is to take the abundance and divide it in half for each age, to represent the 50/50 sex ratio, and then that gets multiplied by a vector of maturity, and then the maturity vector gets multiplied by the batch fecundity, and then it gets further multiplied by the age-specific number of batches per year.

DR. SERCHUK: Great. Thank you.

DR. ERRIGO: I am not seeing any more -- Fred Scharf.

DR. SCHARF: Kyle, just for the age-specific number of batches, is that sort of an asymptotic relationship that's assumed for red snapper?

DR. SHERTZER: Yes.

DR. SCHARF: Okay.

DR. SHERTZER: I didn't show that, just because the panel decided to stick with the length relationship, but those thoughts are shown in the working paper for the other relationships.

DR. SCHARF: Great. Thanks.

DR. ERRIGO: I am not seeing any other hands raised.

DR. SHERTZER: Okay. Then one of our favorite topics, natural mortality, and SEDAR 41 used the Charnov curve age-specific mortality, and that was scaled to the Then et al. estimate of M equals 0.13. That M equals 0.13 was based on all of the fishes that were in the Then et al. paper. The scaling provided the same cumulative survival of ages-four-plus between the age-dependent curve and the age-independent curve.

The panel had discussions amongst ourselves, but we also had Kai Lorenzen join us for part of the workshop, to discuss a paper that he has in the works that's not published yet, but I think it's either under review or ready to be submitted, and so, after discussions with him, we decided to use the Lorenzen age-dependent curve and also scale to Then, but not to all of the fishes, and only the estimate that is obtained when you constrain to just snappers, and, when we do that, then the constant M is a little bit lower, at 0.11, instead of 0.13, but then the scaling was done similarly, where we provide the same cumulative survival of ages-four-plus between the age-dependent and age-independent curves.

This is a change from SEDAR 41, where we're changing to the Lorenzen from Charnov, but then also scaling to only snappers, rather than to all fishes that were in the Then et al. paper, and I will also mention that this is the approach that was taken in the current SEDAR 68 research track assessment of scamp.

Then to show the difference in the two curves, and the blue curve is the Charnov curve that was used in SEDAR 41, and the Lorenzen curve is in red, or orange, or burnt something or other, and so it's a little bit lower natural mortality for SEDAR 73, and you can attribute the shallower slope from the younger fish to the change to the Lorenzen curve, and then the lower saturation value of the older fish is mostly from scaling to snapper data, rather than to all data. Are there any questions or discussions on natural mortality?

DR. ERRIGO: I am not seeing any hands raised. Alexei.

DR. SHAROV: I couldn't miss my favorite topic. Looking at these curves, it seems that, by the age of twenty, the population with one curve will arrive to a cumulative number very different from the one based on the Lorenzen or not? What is the survival by the age of twenty, comparing the two curves?

DR. SHERTZER: I mean, yes, they are different, and that difference is due to the scaling of M equals 0.13 versus M equals 0.11.

DR. SHAROV: Yes, and I am just trying to sort of get an idea of how much different are they, and are we aiming to a certain standard? I mean, essentially -- Are we just, at this point, just simply assuming that the 0.11, based on the data, is sort of the more appropriate M, or just is it shaping it by the Lorenzen?

DR. SHERTZER: I mean, that summarizes a discussion from the workshop participants that it is probably more accurate, for red snapper, to use just the snapper data, rather than data from all fishes.

DR. SHAROV: Okay. All right. Well, thank you. I was just curious as to what would they end up with in the end, by age-twenty or thirty or forty, how much of a difference between the two, the two different ways of -- Two different curves portraying the natural mortality, because they should end up with the same maximum age, but, obviously, the maximum age is way beyond the age plotted. That's just thoughts. Thank you. I don't want to distract you from the rest of the presentation.

DR. ERRIGO: I am not seeing any other hands raised.

DR SHERTZER: The next topic is the video index, and specifically the start year. SEDAR 41 started it in 2010, and this was -- That decision to use 2010 dates back to our first SEDAR 41 workshop. If you remember, there were two data workshops, because it was delayed by a year, and it started in 2013, and, at that point, there would have only been three years, 2010, 2011, and 2012, and so there was a really strong desire to keep 2010.

In other assessments, we don't typically use 2010, and red snapper was the only species where the index started then, and the reasons for not using 2010 is because -- One is the first year that SEFIS started, and so there was some learning -- There was a learning curve, I think, in the sampling, and among them was a camera change that started in 2011, and so the camera was different in 2010, and we can try to adjust for that in an index, but it is another source of uncertainty in the 2010 estimates.

It also had much more restricted spatial coverage in 2010. The sampling was primarily just off of north Florida, and it didn't have nearly the coverage that started in 2011 along the whole southeast Atlantic, and then, also, there's a longer time series now, going through 2009, and so that incentive to include the first year isn't nearly as strong as it was in 2013, when we were first making these decisions, and so, based on that, the panel decided to exclude 2010 from the video index and start it in 2011. I should also mention the chevron trap still starts in 2010, and that wasn't affected by this discussion. Any questions or comments on this?

DR. ERRIGO: Wilson has his hand up.

DR. LANEY: Kyle, you said you could do that adjustment, but do you gain anything if you did the adjustment and added 2010 to it?

DR. SHERTZER: I mean, I don't think it's -- Given that we have the longer time series, we don't really gain much by having 2010. In addition to that, the camera change, there's also that different spatial coverage in 2010, which is, in my mind, more of an issue than the camera change, and we can't really -- We can't adjust very well for the smaller spatial coverage, or really the expanded spatial coverage, in 2011 that could affect an index. I think we're less able to account for that than we are for the camera change.

DR. LANEY: Okay. Thanks. That makes sense.

DR. ERRIGO: I do not see any other hands raised.

DR. SHERTZER: Okay. The next topic is the SERFS trap and video indices. Like I mentioned before, for SEDAR 41, they are combined, using the Kahn method, prior to fitting in the assessment, and that was to account for the non-independence of sampling. There was a selectivity working group that met in the late summer or early fall that discussed these two indices and the selectivities from the two different gears, and there's a working paper, Number 14, that discusses the findings from that group, but the two primary ones that affect the trap and video indices for this assessment are the finding that that trap gear is dome-shaped, and this was shown by Florida in a special study, but it also bore out in a special study that SERFS did by putting stereo cameras on the traps in 2019, and so it's a couple of different data sources that are showing the trap gear for red snapper is dome-shaped, but it's believed that the video gear is flat-topped, and so, if the selectivities are different, combining the indices is not advisable.

A second finding from the selectivity workgroup is that the ascending limb appears to be similar for the two gears, and so this primarily an issue of availability to the gears, and the smaller fish on a site will tend to be trapable. They will go into the traps, and those fish will also be seen by video, and so that's very helpful, because we don't have a lot of stereo video observations to use for estimating selectivity of the video gear, and so it allows us to estimate selectivity.

If we keep these indices separate, we can estimate selectivity of the chevron trap gear, because we have the age comps, and we can estimate a dome-shaped selectivity, but then we can assume a selectivity for the video gear by mirroring the ascending limb of the trap selectivity, but then fixing it to be flat-topped after it reaches its asymptote.

The panel for SEDAR 73 decided to keep these indices separate in the assessment, rather than combine them, but we still wrestled a bit with this issue of non-independence of sampling, and a sort of a simple approach to accounting for that is just to, after fitting them, multiply the likelihoods, each of the likelihoods, by 0.5, so that they're each getting half of the weight of the total.   We did discuss the idea of a joint likelihood, which may be statistically a better way to handle this, and, if there's enough time to try to develop that approach, we will give that a shot, but we weren't certain that we would have time to develop that method and test it for this assessment.

These two bullets I've already said, that the trap is assumed to be dome-shaped, and the selectivity can be estimated with the age comp data. The video is assumed to be flat-topped, with the ascending limb mirroring the trap selectivity. Are there discussion or questions on the SERFS indices?

DR. ERRIGO: Genny has her hand raised.

DR. NESSLAGE: Regarding the using both of the indices and the assumption of independence and -- I'm excited to hear that you guys are considering exploring an alternative, and I assume kind of multivariate lognormal was -- Some kind of correlation term, or is that what you're talking about when you're talking about a joint?

DR. SHERTZER: Yes, the multivariate lognormal.

DR. NESSLAGE: Yes, and I just -- I worry a lot about correlated error with those two indices. They are so similar, and so I don't know -- It's still unclear to me how we're supposed to advise

you guys, but I would really like to see that, personally, and so I don't know how things will be prioritized in your work for the -- All the work you have to do for this assessment, which is pretty enormous, but, personally, I would think that would be important. Thanks.

DR. ERRIGO: Jie has his hand raised.

DR. CAO: Kyle, if I remember correctly, there is a paper published in *Fisheries Research*, naming you, about integrating those two indices, and is that approach going to be applied in this case?

DR. SHERTZER: No, and that approach didn't address the selectivity issue, unfortunately, and so sort of an implicit assumption in that approach was that the selectivities were the same between the two gears, which I think that may be true for some species, and black sea bass is one that comes to mind, but, for red snapper, that doesn't seem to be a same default assumption.

DR. CAO: Thanks, Kyle.

DR. ERRIGO: I see no other hands raised right now.

DR. SHERTZER: Okay. The next index consideration was a new data source that we didn't have for SEDAR 41, and this was the FWRI repetitive timed drop survey. Again, there's a working paper describing the methods and results, but they had a stratified random design, and they used the NMFS zones of 722, 728, and 732, and so it's right off of north Florida, and then they had two depth strata, less than thirty meters and greater than thirty meters.

The study was conducted in five years, 2012, 2014, 2016, 2017, and 2018. Using hook gear, and they did keep fish for ageing, and so we have the age compositions to go with the index, and they standardized the index using a GLM with negative binomial error.

The panel recommended using this index, but in a sensitivity run and not in a base run, and part of the reason for that was concern that the very restricted spatial coverage might not be representative of the population as a whole, but we did want to use these data, and so the recommendation was to use them in a sensitivity run. There were age comps for 2014 that we decided to exclude, because the sampling then targeted deeper water, and so the greater than thirty meters was really targeted in that case, for a special study on spawning, and so we didn't think that the age comps would be representative, but that the index value could still be used in 2014, because the standardization accounted for depth. Then the plot here just shows what the standardized and nominal index values were. The standardized is in red. Any question on this new index?

Then I will move on to commercial discard lengths. For SEDAR 41, we had lengths from the South Atlantic Fisheries Foundation Reef Fish Observer Program, and we have additional samples from that program, but there's also a new data source, which is the shark bottom longline observer program, run out of the Panama City Lab. It's, I guess, an unfortunate name, because it doesn't only sample sharks, and it samples gears other than longline, and so we do have red snapper samples from vertical line here, and those were the ones that were included for the commercial discards, and they were pooled with the samples from the South Atlantic Fisheries Foundation program.

The panel decided to use all the data that we could coming from observers, and so they seem like they're probably reliable data, and the table here at the bottom shows the sample sizes that we have, and it's still low sample sizes, especially for the first time period, and remember that these are pooled, because of the low sample sizes, but, for the second time period, we do start to have reasonable sample sizes, and they are still pooled here, and the number of trips, in this case, would be thirteen for the earlier period, but fifty-two for the later period.

For recreational discard lengths, SEDAR 41 only had lengths from headboat observers, and they did not have lengths from the general recreational fleet, and so there was an assumption made that the selectivity of the general recreational fleet mirrored that of the headboat fleet.

For this assessment, we do have some new data, both for headboat and for the general recreational, and we have additional samples from the headboat observers, but we also had brought to us a dataset that was collected by Steve Amick, from his headboat samples, and so we considered those, whether to use those, and pool them with all of the headboat observer data. Then we had general recreational lengths from charter boat observers off of Florida, and then also the MyFishCount data.

The Steve Amick dataset, and there is, again, a working paper describing these data, but this is a headboat that's run out of Savannah, Georgia, and so the area is a specific area, but he did have good sample sizes, and, in the working paper, it says that the lengths of most discarded red snapper were recorded for the years 2010 through 2013, and it provides an additional 1,400 fish to our length comps in the headboat data, and so we decided to pool the data with the headboat, but also to continue with weighting by state, and so these got weighted by the discards that were from Georgia, state-specific discards.

Then, for the general recreational, these are new data sources that came from the FWRI MARFIN study, and these are described in Working Paper 12, where they put observers on charter boats, and, when we looked at the compositions, which is shown in this plot, comparing the charter boat length discards to the headboat discards, it appeared that the charter boat discards were larger than the headboat discards. Had they been nearly the same, we may have made a different decision, where we may have pooled all of the length, discard length, comps together for the recreational and still allowed the same selectivity in discards between the headboat and general recreational, but, because these appeared to be different, these length comps appeared to be different, we decided to use these comps from the charter boat and then to estimate separate selectivity, which is a model change, for the general recreational fleet.

The MyFishCount data, there was a desire to use these data, but there was some concern that the vast majority of it, 85 percent, were coming from mini-seasons, and there was a belief that discarding behavior during the mini-seasons would be quite different than it would be during the rest of the year, and this sort of bears out in this plot, that you can see the different size distribution of the discards during the mini-season, and it's mostly the smaller fish, and then, during the closed season, you can't keep anything, and everything is being discarded.

For the model, we want the discard lengths to be representative of the entire year, and so we didn't think using discards from the mini-season to represent the entire year would be a wise thing to do, but, again, there was a strong desire to use these data, because of the large efforts being put forward by the council to collect these data, and also by the fishermen who are providing them, and so we

decided to use them in a sensitivity run, and they would be -- In this fleet, the private recreational fleet, for which the MyFishCount data are coming from, it's pooled with the charter boat fleet, and so these data -- You have just a few years at the end of the time series that would be used in conjunction with the FWRI charter boat observer data to estimate discard selectivity of the general recreational fleet.

This table shows the sample sizes from the different recreational discard sources that we have, and the column on the left is the headboat at-sea observers. The ones in the middle are the charter boat observers, where we just have the three years, 2013 through 2015, and we have the headboat observations that were collected by Steve Amick and then the MyFishCount sample sizes on the right for the 2017 through 2019 years.

Then, to look at these graphically, this just shows, by year, what we have available. From 2013 through 2015, we have headboat and charter, and so you can see here, again, that the charter boat is discarding larger fish than headboat, in general, and then headboat continues all the way to the end of the time series, but then, starting in 2017 to 2019, we have the MyFishCount data. I think that's it for the new data on discard lengths, commercial or recreational, and it's a good place to pause.

DR. ERRIGO: I am not seeing any hands raised.

DR. SHERTZER: Okay. Then this is the final bit of new information, new data, that we had to discuss, is on discard mortality. SEDAR 41 had two time blocks, which I showed previously, and that was primarily to reflect differences in discard mortality from j-hooks and circle hooks, and then the values differed for commercial and recreational, but, again, we assumed the same value for headboats and the general recreational were applied, and so the recreational -- The two recreational fleets had a cutoff in 2011, where there was a switch, and that was believed to be a year when circle hooks became more prevalent than not in the recreational fleet.

For the commercial fleet, that transition was believed to have occurred earlier, starting in 2007, and there's a Working Paper 15 that summarized new information on discard mortality and the utility of descender devices, and that was put together by people from the State of Florida, and it was really, really helpful information, and they really did a lot of work to summarize sort of the current state of the knowledge of discard mortality on red snapper.

The panel decisions that came out of reviewing that working paper is that we would use -- Instead of two, we would use three time blocks for the assessment period, and then we had a fourth block for forecasts, and I will describe that in a little bit more detail in a moment, and then we separated the estimates for headboats and general recreational, starting in Block 2, when there was the switch to circle hooks. That was based on information from this working paper that did find subtly different values between the two, but we thought, since we had that information, we might as well include it in the assessment.

Then we did have information on descender device -- Effects of descender devices from recreational fleets, but we didn't have it specifically for commercial fleets, and so we made an assumption that the reductions that we see from descender devices and discard mortality from the charter boat would also be applied to the commercial fleet, and so we assumed that there was a 25 percent descender usage in this Block 3 and that that increased to 75 percent in Block 4 after the

requirement to have descender devices onboard. Then we assumed the same level of uncertainty from SEDAR 41 in the point estimates of discard mortality.

This top table is the one that I showed previously, which were the discard mortality rates that were used in SEDAR 41, and then the bottom table shows the finer resolution discard mortality rates assumed for SEDAR 73. Again, Block 1 is sort of that first block that's just representing j-hooks, and Block 2 is when we believe there was a transition to circle hooks, and so there's a decrease in discard mortality, and then Block 3, which starts in 2017 and goes through 2020, and so through the end of the -- Through the terminal year of the assessment, and it has a reduction for some usage of descender devices, and we didn't think it was very prevalent, but it was being used by some people. Then, post-2020, which would primarily affect the -- Well, it would only affect the forecast, but we would have a further reduction, to account for more widespread usage of descender devices. That's it on discard mortality, and is there any discussion here?

DR. ERRIGO: Wilson has his hand raised.

DR. LANEY: Kyle, you may have said it, and I may have missed it, but do we have any actual, on-the-water survey data to sort of give us a validation on these percentages of use?

DR. SHERTZER: There are some data, and I think it's described in that working paper that I mentioned, the SEDAR 73 Working Paper 15. In that working paper, they were more estimating -- They were estimating the proportion reduction in discard mortality based on -- Conditioned on an amount of descender usage, but there are some estimates, and there was a lot of discussion with the fishermen about whether they thought that descender devices were being used and coming onboard.

DR. ERRIGO: I can help some with that, too. I know that Florida had some data from their observer program on the use of descender devices, because they collected data on discards, and the MyFishCount data also had data on the use of descender devices when discarding fish, and so there was some actual data to inform this.

DR. LANEY: Okay. Thanks, Kyle and Mike. Yes, that's good. It's good to have that sort of independent confirmation.

DR. SHERTZER: And a lot of it came also just from discussions with the fishermen during the workshop and their impression of usage and how much usage, at their least in their area that they fished.

DR. ERRIGO: I do not see any more hands raised.

DR. SHERTZER: Okay. That's it on the new data. The last few slides are just some model modifications that we had a little extra time in the workshop, and so we discussed, and the first one is this idea of pooling the historic recreational fleet, the headboat and general recreational. If you remember, the landings themselves are estimated from the FWHAR method, and then they were split out by using a ratio method, but the selectivity was assumed to be the same between the two fleets.

Given that, there is nothing really gained by separating them, and so I looked into sort of the pooling the landings in the historic data, but then also pooling the fleets in the model, so they wouldn't be separated, and then separating, starting in 1978, which is when we have the first year of the headboat length comps. The benefit of this is say there are twenty-three parameters, which are all these estimated Fs from the headboat fleet, and this plot shows the landings. On the left are the SEDAR 41 values, as they were used in the SEDAR 41 assessment, with general rec on the top and the headboat on the left, and then the alternative that we considered was to -- It's shown on the right, which has all of the general recreational landings pooled with the general recreational fleet, including the headboat landings, and then the headboat landings, that bottom-right panel, start in 1978.

I built this alternative into the SEDAR 41 assessment, and this is really just to demonstrate what I said previously, that it has absolutely no effect on the outcome, because of the assumed -- Because of the assumption that the selectivity is the same for these two fleets, and so these plots show estimated recruits over time, using the SEDAR 41 model with this alternative, and the top-left is the recruits, and the bottom-left is the abundance, and the top-right is the fishing mortality rate, and the bottom-right is the spawning biomass, and the effect on the model is zero, but it does save twenty-three parameters, and so it seemed worthwhile to do. The panel agreed with that and made the decision to pool headboat and general recreational landings for the historic period and separating headboat starting in 1978 with the length comps.

The next topic was the spawner-recruit function, and a lot of this was addressed yesterday during discussions about the snowy grouper spawner-recruit relationship. In SEDAR 41, and also in SEDAR 15 and SEDAR 24, steepness would go to the upper bound, and so it was fixed, and it couldn't be estimated. SEDAR 41, because of that, fixed it at 0.99, which is essentially to approximate a mean recruitment model, but the current version of BAM has an option to just use the mean recruitment model and to not assume a Beverton-Holt relationship with a steepness of 0.99, and so it's a little bit more direct at what we're trying to capture.

These plots show the SEDAR 41 Beverton-Holt model, where steepness is set equal to 0.99, and you can see in the top-left panel that, over the range of data, it's fairly flat, which is the behavior that we're trying to capture, but you can also see, on the right-hand panel, that there is a little bit curvature on this, and it's unavoidable, because the Beverton-Holt function passes through zero, and so, at some point, it has to decline.

This is the fit when we just use the mean recruitment model, which does not assume any relationship between spawning biomass and recruitment, but rather it just estimates an average recruitment and then variability around that average, and you can see, in the right-hand panel, that it has this feature that we're looking for, that there's no curvature at all. If this is the model that we prefer to use, it's more direct, although using Beverton-Holt, with a steepness of 0.99, is a good approximation, and we don't actually need to approximate that, if we want to choose this option of the mean recruitment model.

These are the comparison of the assessments run from SEDAR 41 with Beverton-Holt, versus making this change to a mean recruitment model, and you can see that there may be a few subtle differences in a few years, but, by and large, the Beverton-Holt steepness of 0.99 is a good approximation of the mean recruitment model, and they are very similar. There's not a large effect on the results of making this change.

The panel decision was to use the mean recruitment model for SEDAR 73, if steepness cannot be estimated, but, first, we wanted to evaluate that, and, if steepness could be estimated, we would use the Beverton-Holt model and estimate steepness. I can tell you that, since putting these slides together, that I have evaluated that, and it appears that steepness is still trying to go to the upper bound, and it's still, not surprisingly, not estimable in SEDAR 73, since the previous three assessments have found that same result. This hasn't been presented to the panel yet, but I suspect the plan will be to use the mean recruitment model, rather than fixing steepness at 0.99. Any comments or questions?

DR. ERRIGO: I do not see any hands raised at this time.

DR. SHERTZER: Then the final model change that was discussed by the panel was this change in the likelihood for fitting composition data. SEDAR 41 used a robust multinomial, but BAM has the option for the Dirichlet multinomial, and so we've been using this option in just about all of our recent assessments, because it better accounts for correlation in sampling, and it has the self-weighting feature, and so we don't need to do the iterative reweighting of comp data like we would with a multinomial likelihood, and it also allows for zeroes in the observations, but, in my mind, the biggest advantage is the self-weighting.

Then this is a comparison from SEDAR 41, and it's just switching from the robust multinomial to the Dirichlet multinomial, and I did want to note that I just did this simple replacement of likelihoods, and I did not explore in depth, using the likelihood profiles, whether the parameters that are describing the Dirichlet multinomial are estimable or they need reweighting of indices, and so this is just a very simple substitution, but you can see how it affects the results. In general, using the Dirichlet multinomial leads to predictions of more fish and lower fishing rates. I don't think that's a general conclusion from one likelihood versus the other, but you see it here, and it's similar to what we saw for snowy grouper, and so the panel decision was to use this option for fitting the comp data.

Next steps, we are incorporating the new data, which that's been done, into the updated model, and we're now examining behavior, and we have our first assessment webinar tomorrow. I think that's the last slide.

DR. ERRIGO: I don't see any hands raised for this section.

DR. NESSLAGE: Do folks have any other -- We can go back to anything and open up the floor to any questions that you might have for Kyle regarding what you've seen.

DR. ERRIGO: I don't see any hands.

DR. NESSLAGE: You rendered them speechless, Kyle.

DR. SHERTZER: Red snapper has that effect on a lot of people.

DR. NESSLAGE: Oh, but wait. I think it's time for public comment. While the SSC is mulling things over and seeing if they have any other questions, I would like to open the floor for public comment. If anyone from the public has something they would like to say, please raise your hand.

DR. ERRIGO: I do not see any hands raised.

DR. NESSLAGE: All right. One more chance for the SSC. Do you have any other clarifying questions at the moment? If not, Mike, I will let you describe what exactly we're supposed to do.

DR. ERRIGO: Let me just take control of the screen back here. What I did was I laid out the notes here, just like Kyle had his presentation laid out, with each of the sections, and what was under each of those sections and what changes were made, just to remind everybody, since there was quite a lot that happened.

What we would like to do, for this discussion, is we don't really want to change any of the data workshop decisions at this point, because that could affect the timing of the assessment, but, if there's anything that the SSC let's say is concerned about or would like to have more information on for the review, that's something that we would like to know about now. If there's anything that requires more clarification or anything like that, that's something that should be brought up now, so that the analytical team can get that together for you by the review period, which is in April.

DR. NESSLAGE: Fred Serchuk.

DR. SERCHUK: Thank you, Chair. Just before we get into our discussion here, I just want to thank Kyle for a very thorough and very clear presentation. I mean, he's taken us through all the steps of the workshop, and I think it was just a wonderful presentation, and so my hat is off to him. Thank you.

DR. NESSLAGE: Thank you indeed, Kyle.

DR. SHERTZER: Thanks.

DR. NESSLAGE: Is there anything else?

DR. ERRIGO: That's all I had for trying to frame the discussion here.

DR. NESSLAGE: So I will open the floor. Is there anything that folks would like to see or make sure that there is more information provided? I guess, just to give you another little bit of background, as we've discussed already at this meeting, our April meeting is going to be pretty packed, and we will not revisit these -- Unless things change, we will not go into this level of detail on the data decisions and modeling decisions again, and is that right, Kyle?

The idea was to kind of expedite this a little bit, with the idea that, if you have questions, ask them. Ask them now, or, if there's things you want to see, let him know now, so that we aren't trying to make decisions after the meeting is over or scrambling to put things together for later in the meeting, and so, if you have any burning questions or concerns, this is the time to bring them up.

I will just say that I agree this is very thorough, and you guys have done a great job thinking through all these issues. I mentioned before that I would really -- If you can get to it, I would really like to see a likelihood explored for the chevron trap index, but I recognize that you will be

time crunched as well, and so I don't know. What's your thoughts, Kyle, on how high on the priority list that might be, or is that a panel decision?

DR. SHERTZER: I wasn't placing it very high, because we had a workaround, but it's certainly something that I want to tackle, and it's something that -- I think it's the better approach that we should be doing, and so I think I would really like to tackle that, but I'm just really not sure if we'll have time for this assessment, and part of it is, one, doing the coding, but two is running it through the wringer and doing sufficient testing of it before it's used, especially for a species like red snapper.

DR. NESSLAGE: Yes, and I guess I'm thinking even beyond -- Well, given these are going to be, I am guessing, the most important indices going forward, and people are relying and putting a lot of faith on the information in those indices, I think it's really important that the way those indices are treated in this model is clean, as clean as possible, but that's my personal opinion, and, if anyone disagrees, I would be happy to -- We can change this wording or modify our comments.

DR. SHERTZER: I completely agree with that, and part of this discussion also is the overall weighting of indices and that often get reweighted, but that, because these are fishery-independent indices, we often give them extra weight, and so dealing with -- Sort of including them both in the assessment -- If we didn't down-weight them, they would sort of be giving extra weight to this information, and so we did discuss if that's a bad thing. Because it's fishery-independent indices, and presumably providing very useful information on abundance, maybe it's not a bad thing to give extra weight to the fishery-independent indices.

DR. NESSLAGE: Yes, but, theoretically, they are so not independent.

DR. SHERTZER: Exactly, yes.

DR. NESSLAGE: It's just -- It's like nails on a chalkboard for me, but that's just --

DR. SHERTZER: Exactly, but doing it correct is also important.

DR. NESSLAGE: I just worry should this -- Well, I guess this is an -- This is an operational, right? What type of assessment is this?

DR. ERRIGO: Yes, this is an operational.

DR. SHERTZER: It's an operational plus.

DR. NESSLAGE: Operational plus, right, because it's red snapper, but I guess this approach -- The chevron trap, that index will be used for other species too, and so figuring out how best to deal with that probably would have benefit for other species as well, and is that correct?

DR. SHERTZER: Yes, it's very correct, and the ongoing research track for scamp is dealing with this same issue, and so that might also be a good place to introduce the multivariate likelihood.

DR. NESSLAGE: Yes. Good point. Anyone else on the SSC have feedback for Kyle? Jeff.

DR. BUCKEL: Kyle, can you remind me -- To this point, was one of the sensitivities to -- I realize the base is going to have a trap and video index, but did we have a sensitivity to just pick one of these, which would also deal with the issue, right, that you just go with the video index and drop the trap?

DR. SHERTZER: Yes, we did. That's right. I guess we're lucky, in this case, that the two are in strong agreement, and so, in this case, I don't think it's going to matter too much how we treat them or weight them, but that won't be true for other species.

DR. BUCKEL: I agree with Genny and you that the best way forward is the joint likelihood, but, in the meantime, having that sensitivity may help folks that are worried about the correlation. Thanks.

DR. NESSLAGE: Any other feedback for Kyle or questions of things we should consider or that we would like to see? Unless staff are seeing hands that I cannot see, which I can't see, and I am relying on you all. No hands. Thank you. I think, at this point, we don't have that much feedback for you, and I think Mike did a nice job of taking notes there, and so our breakout groups might be off-the-hook, unless the SSC feels we need to have extensive additional discussion, and I'm not getting that feeling. Unless I see any hands raised in the next few seconds, I am going to move on to our second bullet.

That is consideration of the formation of a workgroup to develop the P* value for red snapper, and this is for our April meeting, and so our April meeting, I believe, is the 27th to the 29th. Staff, correct me if I'm wrong, and I think I have the dates right there, and the idea here being that we want to try and make sure that we have as much of the red snapper assessment reviewed and discussion, discussion of all the points of our report, on record at that meeting as possible, and so what we kind of had thought about was that we typically go through our ABC control rule in a pretty, I guess, predictable fashion, and there is very few times when we deviate too far from that.

With snapper, we've seen it so many times that it's probably not too surprising what our decisions would be for each of the P* decisions in our control rule, and so the idea being that perhaps, if we could get a group of maybe like three volunteers who would be willing to take an early look, as soon as the assessment is available to us, and then meet as quickly as they can, maybe within the next week after that, to come up with kind of your strawman recommendation for what you think the P* would be, going through our control rule, and any alternative recruitment scenarios, whatever you think you -- Whatever extra runs you think might pop up in discussion, if they haven't been done already, and suggest those to Kyle, so that he can have those front-loaded, if at all possible, and we can see those at the April meeting.

We're trying to avoid inserting stuff in the report later, or trying to review stuff over email later, and so are there any questions about that proposed formation of a very small, very brief and quick workgroup, or maybe not even calling them a workgroup? Scott.

DR. CROSSON: My question is who are the SSC representatives on this SEDAR, and would it be appropriate for them to be the ones to do this?

DR. NESSLAGE: So who do we have? Jeff, George, and Anne? Is that right? Who is on that? Speak now if you are --

DR. BUCKEL: Yes, I think that's right.

DR. SEDBERRY: I am.

DR. NESSLAGE: Okay. Kathleen, did you want to confirm for us?

MS. HOWINGTON: The SSC members for SEDAR 73 are Wally, Anne, Jeff, and George.

DR. NESSLAGE: Sorry, Wally. I forgot you. I don't know. You guys have devoted a lot of time to this assessment, and how would you all feel about possibly taking a crack at a strawman P* for us?

DR. SEDBERRY: That would be okay with me.

DR. BUCKEL: It's okay with me, too.

DR. BUBLEY: I am fine with that.

MS. LANGE: I am willing to give it a try.

DR. NESSLAGE: You guys are fantastic. Great suggestion, Scott, and thank you all for your willingness. If there's anyone else who has a burning desire to join these folks, speak now, but I think Scott is right that this is a good suggestion, and they are the most intimately familiar with the assessment at this point, as well as Jeff was talking about alternate runs, et cetera, and you guys know what you're talking about. Okay. Well, that was great.

Thank you so much to the four of you for being so game to try this, and we'll see how it works. We might come up -- The SSC still has the purview, if we change our minds when we get to the meeting and as a full group on the record, but this will hopefully avert any delays and get us ahead of schedule, which we will need.

All right. Are there any other questions or comments about the red snapper update on the assessment? No hands. Excellent. Again, I guess it was Jeff, Chris, and Amy, and I'm sorry you didn't get a chance to try the breakout rooms, but I appreciate your willingness and readiness to do that.

That brings us to -- I would like to go back to our consensus statements from yesterday, and Wilson remembered another potential research recommendation that his breakout group had failed to add to their list, and he wanted to suggest that. Wilson, do you want to talk us through that, really quickly?

CONSENSUS STATEMENTS AND RECOMMENDATIONS REVIEW

DR. LANEY: Yes, ma'am. I will be happy to do that. Mike, did you pop that in there somewhere? We discussed it during our discussion yesterday, and then our team just forgot to stick it in here,

and I have not consulted the other team members, and I did email it to them, and so we just need to discuss it as an SSC and make sure that everybody is comfortable with the language.

My shot at a draft was use the South Atlantic Fishery Management Council Ecosystem with Ecopath model to explore hypotheses regarding snowy grouper and its ecological relationships with other species. These could include exploration of why recruitment has been low, predator-prey relationships, dietary overlap, and other factors. That's it, Madam Chair, and I would welcome critique and discussion by the full SSC.

DR. NESSLAGE: Thank you. I appreciate that, Wilson. The one comment that I would have, right upfront, is can we actually recommend the use of the EwE model, or do we have to -- The reason I am bringing this up is I remember, at the council meeting, it sounded like there was going to be a workgroup formed to decide the priorities for the use of EwE. Chip or someone else, can you refresh my memory on that? How do we best word this?

DR. LANEY: I think you're absolutely correct, and I think there is discussion, or there was, about the decision to form a working group, and so maybe we could -- Instead of saying "use", we could just say "consider use of". Just change it to consider the use of, and I think that will fix that.

DR. NESSLAGE: I agree. That helps me. Any other thoughts on this addition, pro or con or editing content? No hands. Excellent. Thank you so much, Wilson. I appreciate it. I thought we spent quite a good deal of time looking through these consensus statements yesterday, and I don't personally feel as though we need to belabor them any more, but, if there are any other concerns or things you feel -- In looking at them, and if you want to just maybe -- Maybe if you could just browse through them real quick there, Mike. If there's anything else that you think we missed, this would be the time.

Are there any outstanding concerns or things we have forgotten to add, content-wise, to our consensus statements for snowy grouper? All right. No hands. Yan, go ahead.

DR. LI: Thank you, Genny. I just would like to point out that, here, for the major and minor in those rankings, shall we say, within each like major -- Like, for example, within the major category, there is no particular order for these three items listed under the major category, and just because the task, as this to in order, and just make it less confusing.

DR. NESSLAGE: That's a great point, and we were asked to order them, but the ones in the minor category are kind of -- Some of it will depend on priorities and funding, et cetera. Yes, and can we add that caveat? Are you suggesting to both categories?

DR. LI: I think for the first one, for the major category, I think we have the idea of about order, as listed here, but, for the minor category, I feel we don't -- We haven't discussed like which one is more minor than others. I was thinking if we don't want to order, or have the information to order, the minor category yet, then we just say, for the minor category, we don't -- It's not in any particular order yet.

DR. NESSLAGE: I can add something, but are folks comfortable -- I don't want to put too much of my own emphasis on this, but I feel like the minor ones will depend on funding, and are we going to get more recreational data, because some of it is exploring additional data or -- I guess

that was exploring the 2012 data point, wasn't it? Some of this is work not necessarily done by the analysts, but by the data team, but that's still part of what can be included here, and I guess how important, staff, is it that we actually prioritize this list?

DR. ERRIGO: Remember that these aren't research recommendations. These are the uncertainties within this particular model, and the -- I think it's -- I like the way you have it with major and minor, and, if the major ones have more of an order to them, I think that's good. What it does is it tells the next set of analysts and SSC members and panelists that here are the things that had the biggest effect on the outcome of this assessment, and so anything that can be done to reduce these particular uncertainties would have the biggest bang for the buck, or the biggest impact, and then the research recommendations are separate from this, and then that would help you to say, okay, then this research recommendation and this research recommendation will give us the biggest bang for our buck, because they address these particular uncertainties, which are high up on the list, or are of major concern.

DR. NESSLAGE: Right, and I guess can -- I will wordsmith later, but I would like to add something about uncertainties we believe or suspect have a major effect, and we don't know for certain, other than natural mortality. I don't know. Some of those minor ones, if they were explored thoroughly, might end up being major, and we're not really sure, and so I guess, if folks are comfortable with that, I can massage the wording later, and you can edit it when you see the report, as you see fit, if you're comfortable with that concept. Chip.

DR. COLLIER: Chris had a couple of comments that he added to the question box, and I don't think everyone can see those, and so he was talking about some of Kyle's presentation. Chris, if you want to go over those, to make sure that they get on the record with everyone else.

DR. DUMAS: Thanks. Those were just notes for Kyle and staff about the heteroscedasticity issue, and so that's just for their information. That's why I didn't sort of post it to everyone, and it's just background information and sources, and that's all.

DR. COLLIER: All right. I will send it to Kyle right now.

DR. NESSLAGE: Okay. Thanks, Chip, for catching that. Have we gotten all the way to the top? Also, Yan, thank you for catching that. That was a good contribution.

DR. LI: Genny, do you mind if I add one more thing here?

DR. NESSLAGE: Of course not. Go for it.

DR. LI: Like under the minor, the first one of recreational data, the 2012 data point and the sensitivity analysis, I am thinking like we want to explore more of the potential outliers in the recreational data.

DR. NESSLAGE: Yes, and I'm trying to figure out where --

DR. LI: Under the minor, the order, the list, for snowy grouper.

DR. ERRIGO: I'm sorry, but which one were you talking about? Was it this one here?

DR. LI: Yes.

DR. NESSLAGE: Can you just repeat? Sorry. We were trying to figure out where you were, and then I think I missed the content.

DR. LI: Right here on the screen, under the minor category, the first one, the recreational data, that one, I think that we really want to say this, that we would like to -- There might be potential outliers in the recreational data, particularly the 2012 datapoint, and we want to verify if that's a real outlier or not, and that's uncertainty, and so I would suggest that maybe we can consider to change this bullet item to be potential outliers for the 2012 datapoint in recreational and then delete the "sensitivity analysis", because Rob already did the sensitivity analysis, and it shows very little impact on the outcome of the stock assessment. I am thinking that, really, we are not looking at a sensitivity analysis, because Rob already did it, and what we are really looking into is a potential outlier, particularly that datapoint, in the recreational data. Thank you.

DR. NESSLAGE: Excellent. Thank you. Fred Serchuk.

DR. SERCHUK: Thank you, Chair. I wanted to get back to the research recommendations, because I think those should be prioritized. This section talks about major and minor impacts on the assessment, but it really doesn't say, okay, what should be done, and I think that's where the research recommendations probably should be prioritized to say, okay, from this perspective, in terms of -- You may have a research recommendation for reducing the uncertainty and risk in the assessment, and we believe that these things should be done in this order.

This just talks about those that contribute most to the uncertainty in the assessment relative to the estimates of stock status and fishing level recommendations, and I think we've done a good job here, but I think we have to follow it up with some sort of prioritization, at least in one of them, by basically saying, with respect to reducing uncertainty in the projections and evaluation of stock status, these items should have the highest priority, in terms of future research.

DR. NESSLAGE: Okay. Can we scroll down? Good point, and can we scroll down?

DR. SERCHUK: We have the recommendations, but we don't really have a priority, and that's my point.

DR. NESSLAGE: I mean, if we want to make them match the major and minor uncertainty ones, that would -- That would probably be the easiest, and I don't know that, and we've got a lot of research recommendations here, that we could really nitpick exactly the order that all of these should be in, but we could certainly pull up to the top the ones that address natural mortality and Beverton-Holt. What was the other one? Exploitation and apical F issues, and were those the top three? Maybe how would you feel about doing the same thing, where we have high priority and medium priority, something like that, or lower priority? Then it would match, and is that kind of where you're going, Fred?

DR. SERCHUK: Yes, and I think perhaps maybe we could, under improve future stock assessments, talk about, okay, with respect to reducing uncertainty in the assessments and better estimating reference points, the following are considered -- The following research

recommendations are considered of highest priority, and then you could list two or three in there, and then you can go down to other ones and explaining methods for addressing R and so on and so forth, and it would be under different categories with respect to improving the assessment. That's just a suggestion.

DR. NESSLAGE: You just said there would be different categories, and so what do you mean by that?

DR. SERCHUK: Improving future stock assessments, you can do that in a number of different ways, and there might be some elements of the stock assessments that we feel are a high priority because they affect either the estimation of stock status, particularly, and the evaluation of reference points, and that might be a category unto itself, and then outliers may be another category, and addressing R assumptions in projections may be another category.

DR. ERRIGO: So you're saying, within each of these categories, to have -- To prioritize the research recommendations within each of these types of categories that have an impact on the assessment?

DR. NESSLAGE: They're all going to impact the assessment if they actually end up being important, and so that's where I'm kind of stuck. I am not sure how fruitful this will be.

DR. SERCHUK: If we're concerned that we don't really know what the reference points are, because we can't get it from a stock-recruitment curve, then we can either two things. We can try to better fit a stock-recruitment curve, and we just got through with a presentation where basically it was decided that there was no information in the existing stock-recruitment curve that differed from a flat line, and, therefore, that takes care of that one, or we can try to -- If so, maybe we should estimate our reference points from proxies, and then that's different from exploring the MRIP data.

DR. NESSLAGE: I get that, but I'm not sure how categorizing them is going to help prioritize things, and I am getting very confused by what you're suggesting, and I'm not sure that we know the ultimate impact of some of these things.

DR. SERCHUK: Our job is to make recommendations on, one, the adequacy of the assessments and to provide recommendations on ABCs and ACLs. If we can't -- If we feel that we cannot do ABCs and ACLs, because we cannot reliably estimate biological reference points from a modeling exercise, that's fine. Then we'll just go to proxies, and maybe the uncertainty in the assessment says, well, we won't bother about trying to improve our understanding of stock recruitment or find out whether there's some way we could do a better job on it, and we'll just go to proxies. I was just trying to connect the uncertainties with the research recommendations, and that's all I was trying to do, but, if it's too complicated, or we feel it's too confusing, I understand, and we can keep things just as they are. Thank you.

DR. NESSLAGE: I hear you, and let me try to organize them when I work up the report, and I think it's going to be too hard to do, given how many we have right now, and I think it's going to be rough with this venue, and let me -- I think I get what you're saying, and let me see if I can try to make them match, in priority, the uncertainty comments above and maybe categorize them a bit, like you were suggesting. When I send out the first draft, tell me if you think I'm on the right

track and provide some feedback, but I'm not sure, given that I can't even see the whole list right now, that it's going to -- It's going to be hard for us to do this.

DR. SERCHUK: I think you're right on target, Chair, and I support that. Thank you.

DR. NESSLAGE: Okay. Cool. So maybe you and I can work together on that offline, and we're not changing the content, but we're just kind of moving them around, and, if everybody is still in agreement that what Yan had suggested of prioritizing the uncertainty, sources of uncertainty, is correct, and that's what everyone agrees with, then we should be able to make them match, and you all will see that in the draft text. Wilson.

DR. LANEY: Thank you, Madam Chair. I was going to suggest what I think you just suggested, which is that, from my perspective, it would meet Fred's objective if we just divided the research recommendations up into major and minor and then ensured that those that were related to the things that we had identified above, under the risk of uncertainty in the major category, match up with those research recommendations, and I think that's what you just said, if I followed you correctly.

DR. NESSLAGE: Yes, and I think we're finally on the same page. I think we can make this work, as long as everyone is in agreement with what Mike has got on the screen, this major and minor categorization. Anything having to do with natural mortality or stock recruitment or indicators of exploitation and how we characterize exploitation, that that affects our ability to set fishing level recommendations and that those would be prioritized, essentially.

DR. ERRIGO: Real quick, you may want to reword this to not be a research recommendation and move this into the research recommendations and just have this be an uncertainty is the use of apical F in the assessment, rather than other indicators of exploitation.

DR. NESSLAGE: Yes, that makes sense, and so that's the actual to-do, but the source of uncertainty is the appropriateness of apical F in characterizing exploitation for the stock, something like that.

DR. ERRIGO: Yes.

DR. NESSLAGE: Got you. Yes. Can you just paste that down below, and I will make sure it happens?

DR. ERRIGO: Yes.

DR. NESSLAGE: Cool. Any other thoughts on our -- Did we make it all the way to the top, and I forget now, on the consensus statements? I am not hearing or seeing any hands raised. Anything that folks would like to bring to the committee's attention? If not, we will consider these our consensus statements, and we'll wordsmith a little to make it clearer, but the content should stay the same, and you will see the revised report within the next week, I hope. Then, just to -- I apologize that I did not look ahead on the -- Jeff, go ahead.

DR. BUCKEL: Mike, if you could search for "discard", real quick. Just seeing the red snapper presentation this morning reminded me that that may be a research recommendation, is the survey

of descender use in this fishery, because my guess is the ABC is going to drop, and there's going to be -- If folks are targeting blueline tilefish and catching snowy, there's going to be increased discard numbers, and having an idea of if folks are using descenders, and that would help, given recent research that shows there is an increase in survival when using descenders with snowy.

DR. ERRIGO: I don't see that here in the research recommendations. Is that what you're talking about?

DR. BUCKEL: Yes, and I know we touched on it yesterday, but it doesn't -- I don't see it in the research recommendations, and so, if others agree, then maybe the MyFishCount or some citizen science or other survey could try to get an idea of the prevalence of descender use, and that was really helpful with the changing the discard mortality in the red snapper assessment.

DR. NESSLAGE: It seems reasonable to me. Folks, if there's anyone who disagrees, this would be the time, and raise your hand.

DR. BUCKEL: Thank you, Mike.

DR. ERRIGO: You're welcome.

DR. NESSLAGE: Okay. Any other last-minute thoughts on our consensus statements?

DR. ERRIGO: Just so you guys know, what's written down here under the red snapper assessment is -- There is no consensus statements here, and they are just -- In preparation for the breakout group and for the discussion, I simply wrote down all the changes. Under life history, there was batch fecundity, and the change was they used the new three-parameter model, and then, for natural mortality, they used a Lorenzen and the Then et al. for just snappers.

DR. NESSLAGE: I hate to say this, and you did all that work, but we'll probably remove that.

DR. ERRIGO: That's what I was going to say, that you may want to remove all of this, and the only thing you might want to keep is like this.

DR. NESSLAGE: Yes. Can you just highlight that, and then I can do that later.

DR. ERRIGO: Yes.

DR. NESSLAGE: So I just remember that that's the important one that we provided feedback on. Thanks. Fred Serchuk.

DR. SERCHUK: Thank you, Chair. Could we just add a sentence that the SSC agreed with the decisions that were taken by the workgroup in preparation for conducting the assessment, be it that the group talked about our endorsement? That's the feedback that I think is missing here. Thank you.

DR. NESSLAGE: This is where I am torn, because I was told that, if we do disagree, we weren't really allowed to say that, and so I'm not super excited, and not to be difficult, but I am going to be difficult, about saying that we agree with them either. We were told that we were allowed to

ask for additional information or analyses or whatever things we want to see, and not necessarily that we were giving the rubber stamp or thumbs-up, whatever you want to call it, to the decisions that were made. If we don't have the ability to say no, I don't think we should be forced to say yes either.

DR. SERCHUK: I am just reading the action item to provide feedback and guidance to the modeling team on any decisions regarding new data source or other model updates, as appropriate. Now, they have given some model updates, and they have given some data sources that they're using, and my feeling is that we agree with their decisions.

DR. NESSLAGE: I hear you, but I have been told offline that that is not the direction we should have taken.

DR. ERRIGO: That is something for the review. If there are issues that you have with the final decisions that were made, or if you agree with all of the decisions, that is something that should be saved for the review and not for this action item.

DR. NESSLAGE: So sorry, Fred, but I'm going to push back on that one, although I agree with you, and I didn't see anything, personally, that startled me, or that worried me, other than what we've already discussed, but I am not super excited about including that statement in our report, but, Anne, go ahead.

MS. LANGE: If we can't say that we agree with what they do, can we say at least that we found no concerns? It does say to provide feedback and guidance, and so our feedback was we found no issues or items of concern in what was presented to us, aside from the issue that you would like to have if time permitted, and so I think, again, the feedback -- We should have something there. I agree with Fred, and, if it's not something that we agree, then at least say that we found no concerns with what was presented. Anyway, that's what I assumed was supposed to be done.

DR. NESSLAGE: Well, I feel really stuck in the middle now, and so I would appreciate if staff would --

DR. COLLIER: Sure. I will speak up. This is going through the SEDAR process right now, and that is the way that the stock assessments are developed. What we wanted to make sure is that, if there was anything that was going to be coming to you guys in April, we didn't want any red flags to come up in April and potentially delay the assessment until October, and this is more or less -- This was giving you guys the opportunity to see if you wanted any of these issues dug into in a little bit more detail, because you guys are providing the final review for this assessment, and it's really important to the council to get this one done quickly. Hopefully that's some help. It's not necessarily to say this is the most appropriate data, and it's more or less to look to see if there's any red flags for you guys.

DR. NESSLAGE: Thank you, Chip. Wilson.

DR. LANEY: Well, Madam Chairman, I was just going to agree with what Anne said. I mean, I know it's a shade of difference between saying that we endorse the changes that were made, versus we have no objection to the changes that were made by the panel and the analytical team, and, in keeping with what Chip just said, maybe we could just note that the purpose of this review was to

try and tease out any red flags that we may see before the assessment process was completed, so that they could be addressed, and, in our review, we found no red flags.

DR. NESSLAGE: Thanks, Wilson. Anne.

DR. LANEY: Except for that one that we highlighted.

DR. NESSLAGE: Right.

DR. LANEY: Yes, the joint likelihood.

MS. LANGE: Well, again, not to keep saying it again, but there is an action item. If we just leave this blank, why is there even an action item in there?

DR. NESSLAGE: Well, we still have the one.

MS. LANGE: Again, I think just having a leading sentence on that, to say that, you know, we found no concerns, and not that we concur, or not that we agree with, but that we found no concerns, aside from this one thing, in the presentation thus far, or something like that, or of the assessment thus far. I just think -- As Fred said, it kind of begs the question of why does it say to provide feedback, when we can't even say that we heard the report?

DR. NESSLAGE: Understood. Okay. I am going to step out of the way. Can you delete the section down to the highlighted bit there, Mike, and start -- And draft up what Anne just suggested? In the meantime, Chip.

DR. COLLIER: I just want to reiterate that this is -- What we were concerned with is it is red snapper, and it can be a little bit of a sensitive species in the management realm, and what we wanted to make sure was that there were not going to be any issues that you guys wanted additional data coming to you in April, because there's not going to be a lot of time for the analysts to re-analyze, and so this is more or less getting -- It's seeing if there were any concerns that you guys wanted to see a little bit more detail.

We recognize that there is a little bit more changes for red snapper than is typically going to occur for other species, and so there could be no comments in this one, where you guys did not provide any feedback or guidance to the modeling team, because you felt like everything was done well, to your satisfaction, and Kyle has given you descriptions on how the previous model had changed in response to the modeling configurations or other data decisions that he had done, and, to me, that's fine, that there were no -- You guys aren't requesting any additional runs or to see any additional details in the data. Hopefully that helps a little bit more.

DR. NESSLAGE: Thank you, Chip. Wilson.

DR. LANEY: That was an error. I left my hand up. Sorry about that.

DR. NESSLAGE: Okay. Thanks. Amy.

DR. SCHUELLER: I have two things. One is I was supposed to be taking notes for this section, and so I think there is one other thing that could go under here, and that is Jeff mentioned a sensitivity analysis, where either the trap or the video, and I think he suggested the trap index, be dropped, in order to just see what that impact is, and so I think we do have two suggestions to include. I think that's something -- Not having been in any of the discussions with the assessment panel, I don't know if they have talked about it or not, but it would be good for us to just note that we would like to see that.

With respect to this statement, I think the way it's written here is fine, and I'm a bit concerned about saying that we have no concerns, or that we concur, because we really haven't seen model outputs or anything. I mean, this isn't like we went through the assessment and we're giving a rubber stamp. I am hesitant to do that, because we basically just talked about here's the change, and we got sort of some information on why the change was made and where the data came from, but that doesn't necessarily mean that we're rubber-stamping the data based on how the assessment is performing. I think the statement as it is now is fine, but I am hesitant to concur or give some sort of rubber stamp.

DR. NESSLAGE: Thank you, Amy. Anne.

MS. LANGE: Well, actually, I had dropped my hand, but I agree with Amy. I wasn't suggesting that we concur, but just that we had no, aside from now two items, no recommendations for further feedback, further guidance or anything, and so I agree with this.

DR. NESSLAGE: Great. Thanks. Jeff.

DR. BUCKEL: I just wanted to clarify that I asked Kyle if that was one of the sensitivity analyses, and he confirmed that it already was listed as one of the sensitivity analyses, and so I don't think we have to have that here, since I think that's already in his list. I think that's how he replied.

DR. ERRIGO: I would say that you can go either way. If the SSC is really interested in seeing that sensitivity, you can list it here, because it's an important one for you to look at, and therefore you make sure that it comes up in the review, or, because it's already been done, you could just leave it out, but I don't see an issue with leaving it here and telling the analytical team that this is an important one and we want to make sure that you present it to us.

DR. BUCKEL: That sounds good to me, Mike. Thanks.

DR. NESSLAGE: Great. So are folks comfortable with the wording as it's presented here, content-wise?

DR. SERCHUK: I am, Chair. Thank you.

DR. NESSLAGE: Excellent. Thanks, Fred. Does anyone disagree? All right. No hands raised. Thank you for that. All right. Any other last-minute comments or concerns about consensus statements? No hands raised. All right.

So, just to wrap-up here, and kind of return to Other Business really quickly, first of all, I would just like to thank council and SEDAR staff for being so helpful in setting up the breakout rooms

for us. You were game, and you did that very quickly and efficiently, and I really appreciate that. just another reminder that, folks, you have to fill out your own time sheets and submit those to Cindy, correct, and I believe that's correct, after the meeting, and so don't forget to do that.

Regarding the report, I hope to have a draft to you within the next week, and then, Mike and Chip, I'm looking at the calendar, and I don't know exactly when the briefing book deadline is, but if we had the report -- Aim for the report to be done by the 5th of February, is that going to give you -- With a little wiggle room for extra issues that might pop up, are you comfortable with that? Would that give you enough time?

DR. ERRIGO: That sound be enough time to get it into the briefing book.

DR. NESSLAGE: Great. Because that's the first week of March is the next council meeting. Anne.

MS. LANGE: I was just going to ask if there was going to be more public comment.

DR. NESSLAGE: Yes, and I'm still on Other Business, and so do need public comment before then?

MS. LANGE: I'm sorry, but I thought you were closing out the meeting. Sorry.

DR. NESSLAGE: No, and I'm just going through all of these last-minute things under Other Business, because I don't know where else to put them. Fred Serchuk.

DR. SERCHUK: There is a typographical glitch in the second paragraph under Consensus Statements, and I think we mean February 12, 2021. Thank you, Chair.

MS. LANGE: Please let's not redo 2020.

DR. NESSLAGE: Excellent.

DR. ERRIGO: Thank you for catching that.

DR. NESSLAGE: That would be the absolute drop-dead, and it's helpful for me to have all the feedback back from you guys and have some time to incorporate it all and settle any outstanding issues there might be, and so I usually try to push that up a little bit ahead of time, and so I will send out an email with that draft report soon. Is there anything else for Other Business that I have forgotten?

DR. ERRIGO: I just wanted to say, from staff's perspective, although there were some issues with logistics, we did get that all worked out, and I thought it went rather well. In the end, although you didn't save meeting time, I think it saved report time, with all the extra discussion that happened that does not have to happen after the conclusion of the meeting, and so I thought it was rather successful. I just wanted to add that.

DR. NESSLAGE: Good. Well, as time allows, we will try to incorporate this in the future, and, if anyone has any concerns, feel free to reach out to me between now and the April meeting, or

comments or suggestions. If it comes to you, let me know, and we'll start planning that -- Probably the Executive Committee will meet in March sometime. Let's see. Anything else for Other Business? Thanks for that, Mike.

Okay. I am not seeing anything, and then we will open the floor, once again, for a final opportunity for public comment. If you are a member of the public and would like to comment, please raise your hand at this time.

DR. COLLIER: Fred has his hand raised.

DR. NESSLAGE: Fred is not the public.

DR. COLLIER: Well, he had raised it right before you asked for public comment.

DR. NESSLAGE: Can you just wait for a second, Fred?

DR. SERCHUK: I sure can.

DR. NESSLAGE: Thanks. All right. Thank you for time. Fred, go ahead.

DR. SERCHUK: Sorry to be a bugbear about this, but our meeting today lasted for two-and-a-half hours. Is that correct?

DR. NESSLAGE: Is that a question or a statement?

DR. SERCHUK: A question.

DR. NESSLAGE: Well, it started at 9:00, and it will be 11:30 by the time we wrap up, and so yes.

DR. SERCHUK: Okay. I just wanted to make sure that I report the exact same time on the time sheet. That's all. Thank you.

DR. NESSLAGE: Excellent. Anything else? Any other business to come before the SSC before we adjourn? I am not seeing any hands raised, and I want to thank you all for your time and your dedication and the hard work that you put into this meeting today, both SSC members and staff. I greatly appreciate it, and we will see you all again in April. Thank you, and stay safe and healthy.

(Whereupon, the meeting adjourned on January 12, 2021.)

- - -

Certified By: _____ Date: _____

Transcribed By
Amanda Thomas
March 1, 2021

Attendee Report: Scientific & Statistical Committee Meeting

Report Generated:

01/13/2021 11:11 AM EST

Webinar ID	Actual Start Date/Time
381-590-187	01/11/2021 08:23 AM EST

Last Name	First Name
Addis	Dustin
BYRD	01JULIA
Bianchi	Alan
Brouwer	01Myra
Bubley	Walter
Buckel	Jeff
Cao	Jie
Carmichael	John
Cheshire	Rob
DeVictor	Rick
Dumas	Christopher
Finch	Margaret
Fitzpatrick	Eric
Flowers	Jared
Foss	Kristin
Gamboa-Salazar	Keilin
Glasgow	Dawn
Grimes	Churchill
Grimes	Shepherd
Helies	Frank
Howington	Kathleen
Iberle	Allie
Iverson	Kim
Johnson	Eric
Klasnick	01Kelly
Laney	Reid Wilson
Lange	Anne
Li	Yan
McGovern	Jack
Mehta	Nikhil
Neer	Julie
Nesslage	00 Genny
Poland	00Steve
Pulver	Jeff
Ralston	Kellie
Reichert	Marcel

Rhodes	01Cameron
Scharf	Fred
Schmidtke	01Michael
Schueller	Amy
Sedberry	George
Serchuk	Fred
Seward	McLean
Sharov	Alexei
Shertzer	Kyle
Siegfried	Katie
Sinkus	Wiley
Smart	Tracey
Stephen	Jessica
Tomamichel	Megan
Vara	Mary
Walter	John
Wiegand	01Christina
Williams	Erik
Yandle	Tracy
collier	chip
crosson	scott
thomas	suz

Attendee Report: Scientific & Statistical Committee Meeting

Report Generated:

01/13/2021 11:12 AM EST

Webinar ID	Actual Start Date/Time
381-590-187	01/12/2021 08:25 AM EST

Last Name	First Name
Addis	Dustin
BYRD	01JULIA
Bell	00Mel
Bianchi	Alan
Brouwer	01Myra
Bubley	Walter
Buckel	Jeff
Cao	Jie
Carmichael	John
Cheshire	Rob
DeVictor	Rick
Dumas	Christopher
Finch	Margaret
Fitzpatrick	Eric
Flowers	Jared
Glasgow	Dawn
Grimes	Churchill
Grimes	Shepherd
Helies	Frank
Howington	Kathleen
Iberle	Allie
Iverson	Kim
Johnson	Eric
LARKIN	Michael
Laney	Reid Wilson
Lange	Anne
Li	Yan
McGovern	Jack
Mehta	Nikhil
Neer	Julie
Nesslage	00 Genny
Poland	00Steve
Pulver	Jeff
Ralston	Kellie
Reichert	Marcel
Rhodes	01Cameron

Scharf	Fred
Schmidtke	01Michael
Schueller	Amy
Sedberry	George
Serchuk	Fred
Seward	McLean
Sharov	Alexei
Shertzer	Kyle
Siegfried	Katie
Sinkus	Wiley
Smart	Tracey
Stephen	Jessica
Strelcheck	Andy
Vara	Mary
Walter	John
Wiegand	01Christina
Williams	Erik
Yandle	Tracy
collier	chip
crosson	scott