

SOUTH ATLANTIC FISHERY MANAGEMENT COUNCIL

SCIENTIFIC AND STATISTICAL COMMITTEE

Webinar

April 28-April 30, 2020

SUMMARY MINUTES

Scientific and Statistical Committee Members

Dr. George Sedberry, Chair	Dustin Addis
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Dr. Jared Flowers	Dr. Churchill Grimes
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Dr. Brian Cheuvront	Dr. Mike Errigo
John Hadley	BeBe Harrison
Kathleen Howington	Allie Iberle
Kim Iverson	Kelly Klasnick
Dr. Julie Neer	Christina Wiegand
Dr. Chip Collier	John Carmichael

Other observers and participants attached

The Scientific and Statistical Committee of the South Atlantic Fishery Management Council convened via webinar on April 28, 2019 and was called to order by Chairman George Sedberry.

INTRODUCTION

DR. SEDBERRY: This is George. According to my clock, it's 1:30, and so I'm going to suggest we go ahead and begin. I don't have a gavel to gavel us to order, but consider us ordered. This is our regular spring SSC meeting, but it's being run by webinar, and we're going to try and keep it as normal as possible, but this is new for a lot of us, and so be patient. If you have any suggestions or questions, you can raise your hand or use the question box, if you have any.

We have Chip and a couple of SSC members looking for raised hands, and so we will try and call on you as soon as possible, but, if we miss you, be patient. We will eventually get to you. I am going to ask that everyone please keep themselves muted. Again, we'll try not to ignore your raised hand, but this will all work a little better if everybody stays muted. When you do have something to say, or when you're called on by the Chair, please identify yourself as you speak. We're not going to go around the room and have everyone introduce themselves, because it's difficult to do online, and so please just identify yourself when you have something to say.

Public comment will be taken, as usual, during the comment period, and, as you've heard, or you should know, Mike is going to keep the notes on a Google Doc, which we can all access and see the notes being typed, as they're being typed. We will not be able to edit it, but we can view it side-by-side with the webinar and track what's going on, and we'll be able to make comments later in the evening, as we usually do, and so this will be just like a normal meeting, and we'll be watching the presentation, and Mike will be taking notes on another screen, and we can see those notes being taken and then offer our edits to those at the end of the day.

There are assigned notetakers from the SSC for each agenda item, and so I'll try to remind you who you are when we take up that agenda item, but, if I forget, please remember to take notes anyway. I am going to ask again that all members raise their hand to talk, and that hand-raising list will be sorted by how long you've been waiting, and we'll call on the person that's been waiting the longest first, and hopefully this will work, and it will go very smoothly, and we're not going to infect each other with anything, we hope.

We're going to try to get through the agenda items on the webinar as they're listed. For example, on today's agenda, we're going to try and get through all the Tuesday afternoon agenda items, and we might have to run a little long, but we have our presenters scheduled, and so we're going to have to try and stick to the agenda as closely as possible, but there may be times when we can, if we finish early, that we can take on the next agenda item, or we might just adjourn early, depending on how it goes. Today, for this afternoon, we would like to get through all of Tuesday afternoon's items, and we might run a little late, but we're going to try not to run late, and so keep that in mind.

Again, since we're not going through the normal introductions, I just want to welcome the entire SSC. I'm not sure that we're all here, but you can see on the attendee list who is here and who is not, and so welcome to the meeting. We also have many council staff members that are helping us out today, and you can see who they are. We have Steve Poland, who is the SSC liaison from the council online with us, and we have David Whitaker and some other council members as well,

as well as Erik Williams, our NMFS Southeast Fisheries Science Center representative. We also have Shepherd Grimes, who is General Counsel.

We are going to have some changes to the SSC membership, and we can talk about that during the election agenda item, and we have a couple of members that are resigning, and we're going to have an election for a new Chair and Vice Chair, and that's on the agenda, and we'll get to that later. As you all are aware, there's been changes in council staff as well, and so Chip Collier is now the science guy at the council, and I forget what his official title is. Sorry, Chip. He's here with us to help guide us through this meeting, and hopefully things will go swimmingly, so to speak. Before I jump into the agenda items, after that sort of disjointed introduction, is there anything, Mike, that you need to add at this point?

DR. ERRIGO: Let me just, real quick, go over the Google Doc and how to get on there. Real quick, when you guys click on the link, it will bring up the document, and it will be in Word form, and, at the very top, it will say "open with". You click on "open with", and then click on "Google Docs", and then it will open up the Google Doc. Once that's open, you'll be able to actually see me typing, in real time, the notes, and then you should be able to leave suggestions or comments in there, so that, later, I can go back, if you have a comment that says, oh, you forgot this, or that's not exactly what I meant or something like that, and I will be able to go back and fix that. That's about it, and hopefully that will work.

When we're having a discussion, I'll bring the doc up and put it right on the screen, so that everyone can see it on the screen, but, when I'm taking notes on things that we don't -- Let's say a discussion is going on, and there is something else up on the screen, then you guys will still be able to see it, and so this is kind of like dual screens at a meeting. Hopefully that will work. We'll see how it goes. That's all I had.

DR. SEDBERRY: Thanks, Mike. We will make it work one way or the other, and so I appreciate, again, everybody's participation, and the planning that went into this was a little out of the ordinary too, and so I thank everybody on the council staff and the SSC that has helped us plan this as well. Our first agenda item is to review and approve the agenda.

The agenda, again, was included in the briefing book with everything else, and it's a shortened agenda, because we originally had planned a four-day meeting for this, and we had a lot to cover, but we thought that would just be too grueling for a webinar and that some of the topics really need a lot of time, and so we've shortened the agenda, but you all have it. Are there any additions or corrections to the agenda? I don't see any hands raised. Unless there are any objections, the agenda is approved.

Moving on to the minutes, the minutes of the previous meeting, the verbatim minutes, were sent out also. Are there any changes to the minutes of the last meeting, the October 2019 meeting? Again, I don't see any hands raised, and so the minutes -- Unless there is an objection, the minutes are approved. I see no objections, and so the minutes are approved.

DR. ERRIGO: George, I just am realizing that Marcel typed in a comment that made me realize that I believe this is Dustin Addis's first SSC meeting, because he wasn't able to make the October meeting, and so we may want to welcome him to the SSC officially.

DR. SEDBERRY: Thanks so much for reminding me of that, and apologies to Dustin. I am just not used to running this as a webinar, and it's kind of gotten me a little bit confused, but welcome to the SSC. We appreciate your service, and we're looking forward to working with you over the next couple of years, and so thanks for joining us, and welcome.

MR. ADDIS: Thank you. I'm glad to be here.

DR. SEDBERRY: Good. Let's see if you can still say that after these three days. We have approved the minutes and the agenda, and so our first big agenda item is Agenda Item Number 3. First, I'm sorry, and I was getting ready to explain public comments. We take public comments at the beginning of the first webinar, in this case, for the meeting, and then we'll take public comments at the end of the last webinar, and, in between, we'll take public comments, generally after the presentation for each agenda item, but for sure before the SSC starts its discussions. At this point, I am asking if there is any public comment and, if there are any members of the public that wish to comment, they can raise their hand, and we will call on you. I see Rusty Hudson has his hand raised.

PUBLIC COMMENT

MR. HUDSON: Thank you for unmuting me, Chairman Sedberry and the SSC, and I look forward to a good meeting, and I see we have some stock assessments to discuss tomorrow and the next morning, and I appreciate you all's hard work to pull this off by webinar in these tough times, and so thank you.

SEDAR ACTIVITIES

DR. SEDBERRY: Thank you, Rusty, and thanks for joining us, and thanks for that comment. We appreciate it. Any other public comments? I do not see any. Okay. Let's move on to the third agenda item, which is SEDAR Activities, and the notetakers for this agenda item are Dustin, Scott Crosson, and Eric Johnson, and so let's see. We have three sets of terms of reference that we would like to review for upcoming SEDARs, and these are Attachments 2 through 4 in the briefing book, and so I think the way this is going to work is that Mike is going to go over the --

DR. ERRIGO: Kathleen actually will be going over them.

DR. SEDBERRY: I'm sorry. Yes, Kathleen will be going over the terms of reference and the schedule, and we're going to -- After each species, red snapper, black sea bass, and Spanish mackerel, after each one, we're going to discuss the terms of reference, either approve or add our recommendations to it, and then we'll take public comment on all three of them at the end, and I believe that's the way we decided to do this. Kathleen, you're up.

MS. HOWINGTON: Thank you very much, Mr. Chairman. Good afternoon, everyone. My name is Kathleen Howington. Like George said, we have three upcoming assessments that I would like to review the terms of reference, the schedules, and request participants for. Unfortunately, due to the COVID-19 outbreak, the scheduling call was postponed, and so the schedules that I'm going to give you are -- They are very much a draft, and they're not as detailed as I would normally like

to be able to give you, but I can give you a general idea of what these assessments are going to look like.

The first one is actually -- It now has a number. It's SEDAR 73, South Atlantic red snapper, and it is going to be an operational assessment, and you're going to, of course, see the terms of reference in front of you. It's update the approved South Atlantic red snapper SEDAR 41 model with data through 2019, provide a model consistent with previous assessment configuration. George and Mike E., do you want me to read these out loud, or do you want to just let everyone read a little bit, and then we can discuss it afterwards?

DR. ERRIGO: Hopefully everyone has gone through these, at least quickly, unless there is anything major that you think everyone really needs to pay attention to.

DR. SEDBERRY: Yes, that sounds good. These were sent out with the briefing book, and so people had access to them for a while, and so, yes, kind of the major points would be important.

MS. HOWINGTON: All right, and so, major points, there have been -- Due to the timeframe necessary for this assessment to get to the council, or to you, by next April, and what the Science Center is capable -- There were some changes from the statements of work, and there is going to be a workshop held. If it does occur, it will occur December 2 and 3 of this year.

Also, for the SSC selectivity concerns, there is hopefully going to be a workshop that is separate, and this was determined because they're hoping to actually be able to address some selectivity concerns, not just for red snapper, but for other species as well, and then I believe there was also a removal of a discard mortality point. Otherwise, this is exactly what you guys asked for during your statements of work that we looked through during the last SSC meeting. does anyone have any questions or comments, following that?

DR. REICHERT: I have a question. I have like a logistical question, and I just typed it into Mike. Since I'm an organizer, I don't see an option for me to raise my hand, and so maybe you can let me know how I can raise my hand, because I don't want to start talking while people have raised their hands, but one of the questions I had was I saw that, at the last -- Number 5 was convene a panel of several SSC representatives to meet via webinar and in-person, as needed, to review model development and provide guidance, and perhaps you can remind me why that is, why we convened a panel, because I thought there were SSC members part of this entire process.

One of the concerns I have expressed in the past is that, if we do that, and that's similar to what currently is occurring in the research track, it kind of looks a little bit as if the SSC members are participating in the decision-making process and then, later, the SSC is asked to review the stock assessment, and, especially in the operational assessments, it's only the SSC that reviews the assessments, and so I want to make sure that we as an SSC are not making decisions in the process itself and then reviewing our own decisions later on, and so maybe you or someone else can address that.

I think myself and other SSC members have brought that up in the past, that we need to be very careful that we are not reviewing ourselves, and I think that's more like a process question, and I'm happy to table that for another SSC meeting, but perhaps you can address this a little bit. What

is the background of that panel that seems to be an additional panel to what's already happening within an assessment?

MS. HOWINGTON: I apologize. That is -- Apparently the way that I wrote that seems to be confusing, and so the convene a panel of several SSC representatives, that Number 5, is in normal terms of reference as just a normal point. The holding an in-person data workshop, that was specifically requested, to make certain that we do have a data workshop, akin to what we had with SEDAR 60, where we had an in-person workshop, and so that's just -- That would be considered, for me, normal SEDAR procedure of I'm going to request SSC panelists, or SSC members, to be panelists at this workshop, and then the assessment report will then come to you, and you will review during the next April SSC meeting, and does that make sense? I think the way I wrote it makes it more complicated.

DR. REICHERT: Then I think, and we don't have to wordsmith that now, but I think it would be good to -- Because this is a point that comes back, I think, at every terms of reference, and I think it would be good to clarify that language, that that is basically the equivalent of assigning SSC members to participate in the process.

DR. SEDBERRY: Right, as we would normally do. We would have SSC representatives on the panel, yes.

DR. REICHERT: Yes. As it reads now, it looks like there is a separate panel that meets via webinar and in-person to review model development, and, as it reads now, it's basically that we are reviewing this while it's ongoing and then re-reviewing it when it comes to the SSC.

MS. HOWINGTON: Got it.

DR. REICHERT: Do you know what I'm trying to say here?

DR. COLLIER: I will talk about this a little bit. The selectivity issues for the chevron trap and video indices and the FWC study, that was a big point of contention at the end of the last stock assessment for red snapper, and we want to hold a panel of members -- We want to get some of the SSC members involved in reviewing that, and we're going to try to set up a workshop that's going to include some SSC members, some members from the Southeast Fisheries Science Center, and some other folks that are experts in selectivity, and really work on how we can address some of those issues.

DR. REICHERT: Okay. Then, for these terms of reference, I think it's important to make that very specific, that that panel is convened to address these specific issues, the selectivity, for instance.

DR. COLLIER: That's correct.

DR. REICHERT: Okay. Again, because the exact terminology is in every single terms of reference.

DR. SERCHUK: I can recall that we had a discussion at one of our early meetings about this issue, and one of the comments that I made was that, because of the very high profile of red snapper, that

it would be perhaps useful to not only have SSC members on this panel, but also perhaps bring in some invited experts, particularly on those terms of reference that we know have been highlighted, to give some additional external validity to the decision-making process. My recollection as well is that the SSC agreed to that, and so, while the wording is a little bit amorphous in Point Number 5, is my recollection incorrect, because I thought we had agreed to this point. Thank you.

DR. SEDBERRY: Thanks, Fred. My recollection kind of matches yours. My recollection is the reason this was put in here was because the red snapper assessment went from being a research track to an operational assessment, in spite of having some very large issues that we needed expert input on, and the SSC specifically requested that a couple of meetings ago, and I can't remember which one, and I want to say October, but it might have been the meeting a year ago, but my recollection is similar to yours.

Now, as far as what this group is, whether it's a review panel or whatever we call it, it just needs to be made clear that this is not a panel of SSC people that's going to review something that the SSC as a whole is going to review later. There's more to it than that.

DR. REICHERT: I agree with that, but I think it's important that we clarify the language under Number 5, so we all know what the role is of the SSC is under Number 5.

DR. SEDBERRY: I see Wilson has his hand raised. Wilson, did you have something to this point?

DR. LANEY: Not to this point, George. I have a different question, and so, if you want to continue discussing this, that's fine.

DR. SEDBERRY: Let's get this one out of the way, and then we'll come back to you.

DR. LANEY: Okay. That works for me.

DR. SEDBERRY: So our action here, or our suggestion here, is to look at the wording of Terms of Reference Number 5 for red snapper and make sure that it's clear that this is dealing with some particular issues in that assessment, since this is not just a general --

DR. REICHERT: George, I may have a suggestion. Maybe we can delete that entire first sentence and just hold an in-person data workshop, including a panel of SSC members, to review the alternative recreational datasets that exist and selectivity issues regarding the chevron trap and the video index. I think that's the key of this Point Number 5, and, if we delete that first sentence, then it's crystal clear to me.

DR. SEDBERRY: I can agree with that, and, by default, our panelists -- Not by default, but our panelists are the kind of experts we were talking about, that the SSC was talking about, when we mentioned that we needed to have experts present at this review, and, generally, that's what these SEDAR panels are, is a panel of experts. I agree that just having the part about the "hold the in-person data workshop" addresses that SSC concern and that term of reference.

DR. REICHERT: Then, as a detail, depending on the complexity, it can be an in-person or a webinar, as long as it's a meeting to discuss the topics listed here.

DR. SEDBERRY: Okay. Any objection to that? Wilson, I see your hand still raised, but we're going to come back to you. Any further comment on this?

DR. BUCKEL: I think, getting back to what Kathleen was talking about, I think the first sentence is the standard TOR that there needs to be several SSC reps identified for the model development review, and so all the different webinars that take place as the model is developed, and I think that's what that is part of, and then I see this data workshop for the selectivity bit as a separate thing, and it may be the same SSC reps, but I just wanted to clarify that I think what Kathleen was telling us before was that the first sentence in 5 was dealing with just you've got to assign some SSC reps to be a part of the assessment process, and that's -- Even if you don't have a workshop to deal with selectivity, you still have to do that. I guess what I'm trying to say is I think there needs to be two -- There is the assigning the SSC reps to review the different steps in the assessment, and then there is the specific data workshop to deal with the selectivity issue.

DR. COLLIER: Because it is an operational assessment, it's going to be more like an update, and then it's going to have this one additional workshop to focus in on the selectivity issues, and so we're going to really -- Maybe, any other data issues that you guys identify, we need to talk about that, in order to make sure that they're addressed. The big issue that we were talking about was the selectivity issues, and so we would have a workshop to talk about that, and then the rest of it is going to primarily be done by the Science Center, and there's not going to be much review of it until it comes to you guys as a completed assessment, and, if I'm speaking wrong, Erik Williams, please let me know.

DR. WILLIAMS: No, that's exactly my understanding as well, Chip.

MS. HOWINGTON: Can I make a suggestion?

DR. SEDBERRY: Yes.

MS. HOWINGTON: Okay. Since this is obviously a little bit confusing, how about combining the two sentences? You have your generic TOR of convene a panel of SSC representatives to meet via webinar and at an in-person workshop to review the alternative recreational datasets that exist, the selectivity issues regarding the chevron trap and video indices, review model development, and provide guidance. Would that be better?

DR. SEDBERRY: I think that's better.

DR. SERCHUK: As Jeff pointed out, the first statement under 5, it's the generic sentence. If you look at black sea bass, if you look at Spanish mackerel, 5 is included in those terms of reference as well, and so I think we ought to have it in here just the way it is. If we're talking about the data workshop, I would prefer that it be a separate term of reference, because the -- It was pointed out that there are SSC representatives that participate in the assessment process for all assessments, but the data workshop, with respect to the selectivity issues, particularly, is something that is not included, and I think that that should get its own term of reference, particularly if we were thinking about there might be some expertise outside of the SSC that we wish to identify to be part of that. Thank you.

DR. SEDBERRY: Thanks, Fred.

MS. LANGE: I agree with Fred, and I was a little confused when I read these before as well, especially the red snapper one, and the difference, I think, is that we are now looking at operational assessments, and we're kind of used to looking at the regular SEDARs, where the panel is a fairly large panel and not just SSC representatives, but most of the review will be done by the SSC here, with a sub-group of SSC members, right? There's not going to be a -- With the operational assessments, the review -- Prior to coming to the full SSC, the review itself will be done just by a subset of SSC members, right, and the individuals that the assessment -- The stock assessment personnel, and it's not going to have a lot of other people from other groups, right?

MS. HOWINGTON: Anne, there still will be other people on the panel, and I'm going to be requesting from the Snapper Grouper AP for there to be some observers that are appointed, but it will be similar to what a panel would look like with a standard.

MS. LANGE: All right. I was just thinking that might have been the difference, because it was a bit confusing to me with that additional bullet, but I agree with Fred that a separate subset should meet, as has been discussed, but separate from that Number 5.

MS. HOWINGTON: The reason why there's an additional bullet is because, moving forward with operational, there typically will not be a workshop. That's the reason why it's being emphasized, is that there is going to be one for red snapper, to specifically address these issues. I can make it convene a panel of SSC representative members to review, and then just make it an additional TOR. I feel comfortable with that as well.

MS. LANGE: I think that's what Fred suggested, and, if that's the case, I agree with that.

DR. SEDBERRY: I think also what Fred was suggesting is a panel of SSC members and outside experts, and I believe that's what the SSC agreed to at a previous meeting.

MS. NEER: I just want to clarify things, and I think we're getting a little confused, and so, under the way we do operational right now, operational is just another word -- The new term we're using where we combine updates and standards into operational. This assessment, you guys already looked at this statement of work a while back, and it's coming back to you again, is an operational assessment, but it's similar to the way you guys were doing standards already, and there is no sub-panel that reviews the assessment before it comes to the SSC.

The SSC is the review body, and it is the full SSC that reviews the assessment when it comes to you, and so that's the first point. It's not a subset of this SSC that's going to review it and then it comes to the full SSC. It is reviewed by the entire SSC as a body, just like the ones you're going to review tomorrow. That's point one.

Point two is there are two components that are being looked at. As Kathleen had initially said, and, as Jeff Buckel also pointed out, there are sort of two components. There is the regular assessment panel that has been in place for every assessment that we've done in the past few years, and there's this group of people that has those experts and has the fishery observers and SSC members, and that is one panel. That is the main assessment panel, as we know it today.

Now, we're going to be changing that, most likely, after the Steering Committee meeting, but that's next time. For this one, there is an assessment panel that is made up of some SSC reps, some fishery reps, and any other outside experts that the council deems necessary and requests and appoints.

There is a separate component. There is this issue of selectivity, and that is probably going to be handled outside of the SEDAR process, because the hope is to look at these selectivity issues for both red snapper, as well as vermilion and perhaps other species. Therefore, it shouldn't be tied up under this particular terms of reference. I know it is confusing for you guys, because, when this got put together, the document you are looking at, that wasn't clear how that was going to be run, and that's something we're still figuring out the details of, working with the State of Florida.

We still need Number 5, and it should be just to convene a panel of individuals to participate in the assessment process, and we always have some SSC members as part of that process, and that is how we have always done it, and there is nothing different in that step. That second bullet is the fact that we want to do the special section on selectivity, which probably will not actually happen during the assessment process, but external to it, and so review by the entire SSC, only that one review step, and, two, this particular assessment, this one right here, still has a regular old assessment panel. I hope that clarifies it a bit.

DR. SEDBERRY: I think it does. Thank you for that input and that clarification. It makes me feel better about it, and I don't know how the rest of the SSC feels.

DR. LI: I agree with Jeff and Fred that separate those two things, the two items, and I just feel, in that way, we can keep the language consistent, because the TORs for other species, and just for red snapper it's a special item, because there is an issue rising for red snapper particularly. Thank you.

DR. SEDBERRY: Thank you, and I agree with that as well. I think Julie mentioned that this is an operational assessment, which combines a standard and an update, and we've done this a lot of different times, and this is no different than others, but it is a little different, because I believe it started out as being suggested as a research assessment, and, really to speed things up, it was changed to an operational assessment, and the SSC was concerned that, with the selectivity analysis and the new data available, that it needed perhaps some additional eyes on it as it was being done, SSC eyes and expert eyes, through this process, and so it is a little different than the other operational assessments, although this whole process is a little new, and maybe they're all different from each other, but I agree that this sub-bullet under 5 is important and perhaps should be a separate bullet with clarifying language.

DR. REICHERT: George, can I ask a clarifying question? I know there may be others in the queue, and so I'm happy to wait, but I just want to --

DR. SEDBERRY: Go ahead, and then I have Anne after you.

DR. REICHERT: Maybe Kathleen or Julie can answer that question, and so this operational assessment will be conducted in a series of webinars. If there are not a series of webinars, then I also assume there is no panel that can discuss and review.

MS. HOWINGTON: This operational assessment will be conducted via an in-person workshop and a series of webinars.

DR. REICHERT: Okay.

MS. NEER: This one is different than the next two that we're going to review.

MS. HOWINGTON: Right. The other two are just webinar.

DR. REICHERT: Okay, and the in-person data workshop is specifically to address alternative recreational datasets and the selectivity issue, correct, or are there other data discussed, also? I just am getting increasingly confused, in terms of, one, the function of an operational assessment and the -- Because, earlier, Julie said that the selectivity issue is not specific to red snapper, but also to other species, but now I understand that it is part of this red snapper assessment, and I just want to clarify where I am getting a little confused.

MS. NEER: Chip, would you like to clarify the council's approach, or would you like me to do so?

DR. COLLIER: I will do it. The approach that the council is thinking about taking for the selectivity issue is to look at multiple species at one time, looking at that FWC study that they did and how that could potentially be integrated into the stock assessment. We are going to do it -- The plan right now is to do it outside of the SEDAR process and convening a group and putting them together, and we're going to be working with FWC in order to set this up, and we would like to have it sometime later this year, but it is just in the planning phases, and we don't know if we have time to do it or anything, but we want to get this done, and that is our goal to get it done, in order so that the analysts can have it for the modeling of red snapper.

The Science Center has indicated that they would like to have the stock assessment to you guys in April of 2021, and so we need to get it done pretty quick and have that meeting together and pretty narrowed down, in order to focus just on the selectivity issues.

DR. REICHERT: Okay. That makes sense, and so, if this is a workshop that addresses multiple species, then maybe it should not be in the -- It's outside the SEDAR process, and maybe it shouldn't be in the SEDAR South Atlantic red snapper terms of reference.

MS. HOWINGTON: I think the reason why it's in the terms of reference is because there are two workshops. There is the one that Chip was just discussing that is outside of SEDAR that is going over multiple species and there is a data workshop that will occur on December 2 and 3 of 2020 that is going to be about red snapper.

Now, hopefully, that other workshop that's outside of SEDAR will have already occurred, and the selectivity issues will have already been addressed, and that will have already happened, at which point in time this in-person workshop will be a normal, standard, in-person data workshop. However, if that hasn't happened, then the term of reference specified this because it was such an important thing that the SSC had in mind, and so I think that's where the confusion is happening, is there are two different workshops, and the reason that this one is specific is to make certain that these get addressed.

DR. REICHERT: Thank you.

DR. SEDBERRY: Thanks, Kathleen. That's very clear, and that sounds good to me, but it's just that, in the wording of TOR Number 5, that's not obvious that that's what is going to happen, and so this needs to be wordsmithed a bit, and I don't know what the next step would be, if you could be working on that and we can come back to this later or how we can move on, incorporating all that we've said here and make sure that we get to see the wording of it, the final wording of it, before we break up today, and is that possible?

MS. HOWINGTON: Yes. I would like -- Does everyone understand what these terms of reference were trying to get at, because I would like to try and get more feedback on this, and then, while you all are discussing the next point, I can be working on this and hopefully come back to you.

DR. SEDBERRY: That's what I was thinking.

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

DR. SEDBERRY: Yes, go ahead, Fred.

DR. SERCHUK: Based on what Kathleen has said, it seems to me that the first sentence in 5 is fine, and would it also be correct to say that the panel should take note of a workshop to be held -
- The findings of a workshop to be held in December that will be examining recreational datasets and selectivity issues, and would that get at this difficulty that we're having, that that workshop is going to be held outside of the SEDAR process?

DR. SEDBERRY: I believe the December workshop is going to be part of the SEDAR process. It would take up the selectivity issue, if it's not settled prior to that time in a preceding workshop, and is that right, Kathleen?

MS. HOWINGTON: Yes.

DR. SEDBERRY: So I like that. That, to me, is a --

DR. SERCHUK: Then I'm confused. I thought I heard that -- (The remainder of Dr. Serchuk's comment is not audible on the recording.)

DR. SEDBERRY: While we're waiting for Fred to come back, which he probably will right in the middle of --

DR. SERCHUK: Can you hear me now?

DR. SEDBERRY: Yes.

DR. SERCHUK: Okay. My confusion, and I really would like to get this resolved, is I heard, in a previous comment, that there was going to be a workshop that was going to be held outside of the SEDAR process. If that's the case, then I think we have to have some statement that the process needs to take account of the findings of that workshop, but, if there's not going to be a panel

workshop outside of the SEDAR process, then the question is moot, and we don't need to have any language concerning that, but that's what I heard. Thank you.

DR. SEDBERRY: You heard right, Fred. There is going to be a workshop outside of the SEDAR process that's going to be held prior to the workshop that will be in the SEDAR process. The one that's outside the SEDAR process will look at selectivity in the video index and all that related to the Florida study on red snapper and vermilion snapper and some other species.

DR. SERCHUK: Would it be appropriate to mention that the findings from that workshop ought to be considered?

DR. COLLIER: Fred, can I just pull up a Word document real quick that you guys can look at, and maybe this is the wording that you want to see?

DR. SERCHUK: That's fine.

DR. COLLIER: It seems like we're talking about the wording quite a bit, and let me see if I can make myself a presenter. What I have typed up there is convene a panel of SSC members to meet via webinar and in-person, as needed, to review model development and provide guidance, and that's what Julie and Kathleen have been talking about. That is the normal SEDAR process for an operational assessment, currently, and then, italicized, I have: "Outside of SEDAR, hold a workshop to focus on selectivity issues regarding chevron trap and video indices."

DR. SEDBERRY: I think what Fred is trying to say is that he wants it to be made clear that the results of that workshop will be carried forward into the regular SEDAR workshop, which is the one scheduled for December.

DR. SERCHUK: Exactly. Thank you.

DR. SEDBERRY: Not to complicate it, but we had also said that, if that selectivity workshop outside of the SEDAR process does not occur for some reason, then the selectivity and other issues, regarding red snapper only, will be taken up at the December SEDAR workshop.

DR. ERRIGO: I actually think that's implicit here, and so you're going to take the results of this outside workshop and provide -- Carry that forward and provide it to the SEDAR data workshop. If there's nothing to provide, then the SEDAR data workshop will continue as planned, because they're going to have to make a decision on using that selectivity for red snapper, regardless of what the outside workshop found. They might say, okay, yes, we like that, and we're going to do that, but they make the final decision anyway, and so I think this is implicit in this statement.

DR. SEDBERRY: As long as Chip has this up on the board, let me call on Anne, to see what she has to say about this Agenda Item 5.

MS. LANGE: I apologize if I missed this in meetings past, but, because of the discussion we had initially on this Term of Reference 5, shouldn't it be convene a panel including several SSC representatives, because this, to me, sounds like we're just SSC members, which is why I questioned whether or not it was the standard SEDAR review.

DR. SEDBERRY: This was Marcel's original issue.

DR. REICHERT: Exactly. That was the first --

MS. LANGE: Yes, Marcel, and that's what confused me as well, because it sounded like something -- It was not the way we do a normal SEDAR.

DR. REICHERT: Exactly. We do not have a separate panel.

MS. LANGE: Correct, and so it would be including SSC representatives.

DR. REICHERT: Thank you.

DR. SEDBERRY: Yes, I agree with that. That needs to be clarified.

MS. LANGE: Okay, because that was what I was trying to say earlier on, too. Sorry.

DR. SEDBERRY: That's good. Thanks, Anne.

DR. REICHERT: Thanks, Anne. That was exactly the point that I was going to make.

DR. SEDBERRY: Now, can the SSC as a whole agree that this is the perfect substitute Term of Reference for the existing Number 5?

DR. LANEY: Yes, if you get rid of the "of" after "panel".

DR. SEDBERRY: Thank you, Wilson. Wilson, as long as you're unmuted, your hand has been raised for a long time now.

DR. LANEY: Yes, and, originally, it had to do with something else, but, after all the discussion on this one, I just wanted to make sure that I understand what the heck we're saying here, and so am I correct in that there is going to be this panel outside of the SEDAR process that will focus on selectivity issues regarding the chevron trap and video indices, but not just for red snapper, and it will include other species as well, and then the results, decisions, whatever from that panel will then be considered by the red snapper SEDAR workshop, which will be held in December of 2020, and am I understanding that correctly?

DR. SEDBERRY: That is the way that I am understanding that. I think that is correct.

DR. LANEY: Okay. Well, then I think I understand everything. My question had to do with the second bullet under TOR Number 2, which is suppose is somewhat related to this whole discussion, because it involves alternative recreational indices being possibly included in the red snapper SEDAR. It just says to consider including, as an estimate of recreational catch, the alternative estimates of catch, and my only question was is that something that we have done with previous assessments, and, if not, how is this done? Are they just treated as an additional index or value that goes into the model? Is that the way that works? I guess that's a question for Kathleen or Erik or somebody out there.

DR. SEDBERRY: Can anyone answer Wilson's question?

DR. COLLIER: Yes, there were alternative catch estimates used during the open season for red snapper. The main one was the estimate derived by the FWRI FWC study that's been used for the past few years, and so we're just making sure that they're continuing the use of those alternative estimates and not necessarily trying to develop new ones, but continuing with those alternative methods.

DR. LANEY: Okay, Chip, and so that's just as an additional estimate along with the MRIP recreational estimates, or does it replace the MRIP recreational estimates?

DR. COLLIER: So they go through -- When they did it during the last SEDAR, they went through and evaluated, state-by-state, which one would be considered the best estimate, and so MRIP might be considered for some states, and then other states, particularly Florida, is not going to be an MRIP value for the catch. The releases will be based on MRIP.

DR. LANEY: Okay. Gotcha, and that's because we presume that the state estimate, in that case, is more precise or accurate, whichever is the correct term, than the MRIP recreational estimates?

DR. COLLIER: Well, that's going to be up for the panel to decide, and so they will review those numbers to make the recommendation on what should be used, but we just want to make sure that, because this is different than the normal terms of reference and it's different than the normal way estimates are derived for recreational catches, that it was included in the terms of reference.

DR. LANEY: Okay. That's fine, and the other part of my question was have we done this with any other species, or is this unique to red snapper?

DR. COLLIER: Can you say that again, Wilson?

DR. LANEY: Have we done this with any other species, or is it unique to red snapper?

DR. COLLIER: On the South Atlantic side, it is unique to red snapper, because we have such short seasons.

DR. LANEY: Okay. Thank you. That's all I had, Mr. Chairman.

DR. SEDBERRY: Thanks, Wilson. Meanwhile, back at Term of Reference Number 6, which is I think what is next --

DR. COLLIER: George, Fred has his hand up.

DR. SEDBERRY: Go ahead, Fred.

DR. SERCHUK: Thank you, Chair. Sorry, but I'm in the slow group, and so I'm still on the bullet, on 5, and I wonder -- Hold an in-person data workshop, that's fine, but, really, are we asking the panel that includes several SSC representatives to review the results of the data workshop that will be evaluating the alternative recreational datasets and selectivity issues? Isn't that what we're asking the panel to do?

DR. SEDBERRY: No, I think we're --

DR. SERCHUK: Isn't the panel going to integrate the results of that data workshop into their model development?

DR. SEDBERRY: Yes, as a part of it, and so that's not all that we're asking them to do.

DR. SERCHUK: Okay, but we're asking them to -- How are the findings of the data workshop going to be used?

DR. SEDBERRY: Okay. We've re-written Number 5, and so maybe we can bring up Chip's new version of it and make sure we're talking about the same thing here. Chip, can you bring up that Word document again?

MS. NEER: George, if I may, while Chip is bringing that up, there is only -- There are not separate stages in an operational assessment. There is not a data stage and then an assessment stage and then a review stage. There is one panel that does all the data discussions and all the assessment discussions, and the SSC provides the review, and perhaps that's where you're getting a little hung up. This workshop that's under this bullet that Chip is working on, that is an external project of SEDAR, and they are going to bring that bit of information, and that's going to be one of the many pieces that will be reviewed, along with the recreational questions that you guys were discussing.

DR. SERCHUK: All I'm asking is for the word "review" -- To review the data workshop outside of SEDAR that is focusing on selectivity issues, and I don't see an operational verb here. It says, outside of SEDAR, hold a workshop, but it doesn't tell you how the workshop results are going to be included.

MS. HOWINGTON: I think what you're saying, Fred, is that maybe we should add something underneath Term of Reference Number 2, under the bullet that says to address SSC selectivity concerns, and maybe there is an additional point that we can add, if you go up right now, Chip, and it says -- Maybe you want to add a word or a sentence underneath here, Number 4, saying review the outside workshop, and I don't really know what we're going to call it, but review the workshop that focuses on selectivity issues, and would that be helpful?

MS. NEER: Perhaps review the report that comes out of the selectivity working group meeting, something along those words. Let's call that a selectivity working group.

MS. HOWINGTON: Or workshop.

MS. NEER: As opposed to an assessment panel, where we're getting confused.

DR. REICHERT: Again, can I ask a quick clarifying question? I just want to make sure that the alternative recreational dataset is no longer part of that external workshop, correct?

DR. SEDBERRY: I'm sorry, but could you say that again?

MS. NEER: The external workshop is just dealing with selectivity in the South Atlantic.

DR. REICHERT: Thank you.

DR. SEDBERRY: Thank you.

MS. NEER: I think there's two ways to potentially deal with this. We could add a bullet, like Kathleen has mentioned, under 2, or we could tack it on to the bullet on the text that Chip was working on, and just make clear that a report will be provided that will be reviewed by the panel, the assessment panel. We have: "Outside of SEDAR, hold a workshop to focus on the selectivity issues regarding the chevron trap and video indices. A report will be produced and provided to the SEDAR workshop being held in December of 2020 for their consideration."

DR. SEDBERRY: That sounds good to me. Let me see if there are any hands raised. I do not see any. Any objections to this? I don't see any hands raised.

MS. NEER: I would take out the word "regular", because that gets confusing, what you mean by regular. I would say just reviewed at the SEDAR workshop in December of 2020.

DR. SEDBERRY: To me, this addresses Marcel's original concern, and it addresses Fred's concerns and the issues brought up in previous SSC meetings. This looks good to me. Any final comments on TOR Number 5 for red snapper? I don't see any, and I believe we have already covered TOR Number 6, and so I believe we are ready to move on. No, we're not ready to move on. Our action items are to approve the terms of reference and schedule, and so I would say that the SSC approves the terms of reference, as modified, and then we also need to request participants for this series of workshops.

MS. HOWINGTON: Yes, but, first, I would like to announce the schedule, just so, that way, participants know what dates they are going to be available. Chip or Mike E., would either of you mind me being the presenter? I already have the draft schedule pulled up.

DR. ERRIGO: No problem. I will pass it to you right now.

MS. HOWINGTON: Thank you. All right. For SEDAR 73, South Atlantic red snapper, once again, I would like to remind everyone that the scheduling call was postponed, and so these dates have not been approved, but they have been reviewed by Erik Williams, and they are hopefully going to be very, very close to the schedule that will be presented to the council in June, but the game plan is, this July, July 6, 2020, will be the first data scoping webinar, and so it's going to get started that quickly. The next dates that you need to know are October 16 is the data deadline for final datasets, and then there will be an in-person --

DR. REICHERT: Kathleen, can I ask a quick question?

MS. HOWINGTON: Yes, sir.

DR. REICHERT: The terminal year is 2019, correct?

MS. HOWINGTON: Yes, sir.

DR. REICHERT: Thank you.

MS. HOWINGTON: Then there will be an in-person workshop in Beaufort, North Carolina on December 2 and 3 of this year, and that will then be followed by a series of assessment webinars in January and February of next year, 2021, hopefully completing the assessment report and submitting it to the SSC by your April meeting of next year, 2021. Does anybody have any questions about this schedule or comments? Do we have any hands raised?

DR. SEDBERRY: I do not see any hands raised, and so I don't believe there are any questions regarding the schedule.

MS. HOWINGTON: All right, and so then the next question is who would like to volunteer to participate in the SEDAR 73 South Atlantic red snapper operational assessment?

DR. SEDBERRY: Can you clarify what webinars and meetings this would involve? This would not involve the workshop that's outside of SEDAR?

MS. HOWINGTON: It would not involve the workshop that is outside of SEDAR. This would be as a normal SEDAR assessment and the schedule that you see in front of you, and so the only in-person workshop that is currently on the schedule is December 2 through 3 of this year, and then a series of webinars in January and February of next year, and so those are the big ones. I have a question for you, Chairman. How would you like to take participants? Would you like them to raise their hand, or would you like to just say that I volunteer?

DR. SEDBERRY: I would like to do this like we normally do. Usually, we ask for volunteers at the meeting, and we usually get them, but, if we don't, we follow up with email, and so I will just ask right now if anyone would like to volunteer for this, and we need two or three?

MS. HOWINGTON: Typically, it's three is the number that we get. Sometimes it's three or four, and sometimes it's two or three, and so, because this is red snapper, I would say three or four.

DR. ERRIGO: More would be better for red snapper.

DR. SEDBERRY: Four is probably better, you said?

DR. ERRIGO: I said more, but yes.

DR. SEDBERRY: So the more the merrier, and so let's see those hands raised, for anyone would like to volunteer as the SSC representative.

DR. REICHERT: George, you can put me down, since I expect that we'll be involved one way or the other anyway, because of our survey.

DR. SEDBERRY: Okay. Thank you, Marcel.

DR. REICHERT: However, if there are three other SSC members, then I may be involved just as a data provider.

DR. SEDBERRY: Let me see if anybody else has their hand raised. Actually, you don't need to raise your hand. Just speak up, if you would. I know some of you just need a day or two to think about this, and then you will be happy to volunteer, but, if you can do it now, that helps us to schedule things.

MS. LANGE: If you don't have enough people, I should be available.

DR. SEDBERRY: Thank you, Anne. I see Mike is putting you on the list. Anybody else? I have not really looked at the schedule on this, relative to my calendar, but it's possible that I could be available, but I will have to get back to myself on that.

DR. BUCKEL: Since the workshop is in my backyard, I will volunteer for this one.

DR. SEDBERRY: Thank you very much.

MS. HOWINGTON: All right, and so I have Marcel Reichert, Anne Lange, Jeff Buckel, and then George Sedberry, potentially.

DR. SEDBERRY: Yes. That's what I have.

DR. NESSLAGE: I'm not sure that I can make all of these, but I would probably like to listen in on them and participate as much as I can, if that helps.

DR. SEDBERRY: That does, and, again, I haven't looked at my calendar, but maybe, between you and me, we can pick one of us as the fourth person, for sure, in the next couple of days.

DR. NESSLAGE: Great.

MS. HOWINGTON: All right. That all sounds fantastic. That gives me my three to four, and so, with your permission, Mr. Chairman, can I move on to black sea bass?

DR. SEDBERRY: That sounds good to me, and I see no hands raised, and so I think we are done with red snapper. Well, you're never done with red snapper, are you, but we're done with it for now.

DR. COLLIER: George, did you want to take public comment on red snapper, before we got to the other ones?

DR. SEDBERRY: Well, when I looked at my little script that I wrote for myself, I realized that we were supposed to take public comment before we started this, but I think it makes more sense now, seeing the amount of time it takes for each one, and that we take public comment on them as we go, and so let's do that now, and so do we have any public comment on the terms of reference for red snapper, for the red snapper SEDAR? If you would like to make a comment, please raise your hand. I see Rusty Hudson. Rusty, go ahead.

MR. HUDSON: I heard fisheries appointments would be made, and they cancelled our Snapper Grouper AP spring meeting, and so I was wondering how Myra or Kathleen would be doing the

outreach for those appointments, and that's the main thing, and I liked the discussion on the red snapper otherwise.

MS. HOWINGTON: Those appointments are going to be done via email, actually probably either tomorrow or the next day, and I wanted to get the terms of reference to the SSC, and then she's going to be sending out an email asking for volunteers, which I'm guessing, since you perked up, you might be on that list, but that's going to be how that gets done. That way, we can get the participants appointed by the June council meeting.

MR. HUDSON: Okay. That's what I was trying to get at, and so thank you very much.

DR. SEDBERRY: Thank you, Rusty, and thanks, Kathleen. Any other public comments?

DR. COLLIER: C.J. Schlick had his hand raised earlier, and I don't know if he has a comment. You are self-muted, C.J., if you want to speak.

MS. SCHLICK: Hi. I actually wasn't intending to raise my hand, and I'm sorry.

DR. SEDBERRY: That's okay, C.J. Those things happen. Any other public comments? Seeing none, we are ready to move on to black sea bass.

MS. HOWINGTON: This is the black sea bass operational assessment terms of reference. As we've stated before, I will go over some general highlights, and this is going to be all webinar, and there will not be a workshop involved. There were no major changes from the statement of work to this term of reference from where you saw last October, I believe is when we met, and so do we have any questions or comments about these?

DR. REICHERT: It's the same as in the previous one, and I would re-type, or re-formulate, Number 5. It's the same issues.

DR. SEDBERRY: Right, and it's not a panel of SSC representatives, and it's a panel of SSC representatives to be included on the panel, or SSC representatives to be included on the panel.

DR. REICHERT: Exactly, and, also, since the same selectivity issue is raised here, I assume that that second part of what we formulated in the previous terms of reference is also applicable here.

DR. SEDBERRY: I am not sure that there were enough black sea bass in that Florida study to be included in that selectivity workshop.

DR. REICHERT: I agree, George, and that's the point that I brought up earlier, but it is listed under Number 2.

DR. SEDBERRY: Okay. Well, I guess that can be decided at the workshop, and so, if there is enough data in that study, it would be great to have black sea bass included, and that's a decision that will be made at that workshop, and so, yes, it needs to be included in the workshop then.

MS. HOWINGTON: Marcel, do you think that there needs to be additional wording here for this point or just, for the workshop, make certain that black sea bass is looked into as well?

DR. REICHERT: Well, I think we can capture that in the consider sensitivity analysis resulting from the fourth bullet point, consider the sensitivity -- Now I can't see anything. Here we go. Consider sensitivity analyses to address concerns addressed at the selectivity workshop, and then that -- Or move that to the last part of that, but at least that means that, if there's anything that comes out of that selectivity workshop that is relevant for black sea bass, then that could be addressed. Then I think that captures it. Thank you.

DR. SEDBERRY: Any other suggestions or corrections for the terms of reference for black sea bass? I do not see any hands raised, and so I guess we can move on to the schedule, or did you have something else to add to this, Kathleen?

MS. HOWINGTON: The schedule, like I said, the scheduling call was postponed, and so the schedule is very, very easy to go over. We will start at some point in time mid-year next year, 2021, and so that's March or April, maybe, potentially May, but that will be in 2021, and it will end in April of 2022. I can write that up here if you want to visualize that, but I have a start, I have a beginning, and I have an end, and it's going to be only webinars, and so there's no in-person meetings at all. Any questions or comments about that schedule? Of course, I will be presenting a more complete schedule the next time the SSC meets.

DR. SEDBERRY: Okay. I do not see any hands raised.

MS. HOWINGTON: All right, and I recognize that this is pretty far out, but do we have any volunteers to participate in the black sea bass operational assessment starting in mid-2021 and ending in April of 2022?

DR. SERCHUK: I would be glad to participate.

DR. SEDBERRY: Thanks, Fred.

DR. DUMAS: I would be glad to participate.

DR. SEDBERRY: Thank you, Chris.

MS. HOWINGTON: Do we have one more?

DR. SHAROV: I will participate.

DR. SEDBERRY: Thanks, Alexei.

MS. HOWINGTON: I have, for the black sea bass operational assessment, Fred Serchuk, Chris Dumas, and Alexei Sharov.

DR. SEDBERRY: Mike has gotten that down, and so I think we are done with black sea bass.

MS. HOWINGTON: Okay. Do we want to take public comment now?

DR. SEDBERRY: We do. Thank you. Are there any public comments regarding the terms of reference for the black sea bass operational assessment?

DR. COLLIER: Rusty Hudson has a question.

DR. SEDBERRY: Go ahead, Rusty.

MR. HUDSON: I had always understood that June of each year would be when we would get the annual -- Like black sea bass is January to the end of the year, and we would get those numbers, usually by June, and I heard March come up in the discussion, but are we going to be okay with the terminal year of 2020 and having the data available for the analysts?

MS. HOWINGTON: The beginning would be the data scoping webinar, and then there's typically a very long gap in time between the data scoping webinar and when the first assessment webinar would begin, and there should be enough time for the 2020 data to be incorporated.

MR. HUDSON: Okay. I don't have a problem with the commercial, and I just was wondering with the new MRIP approaches and other type of stuff like that, if that would throw kind of a delay in things, but I look forward to seeing this take place, too. Thank you.

DR. SEDBERRY: Thank you, Rusty. Any additional public comment on black sea bass? Now I believe we're ready to move on.

MS. HOWINGTON: All right. The next SEDAR assessment is Spanish mackerel, and it is also going to be an operational assessment, and it will also be conducted only via webinar, and there will not be a workshop for this, and it has the same schedule as black sea bass. Again, it will be starting in mid-2021, and it will be ending in April of 2022. Again, there were not very many changes from the approved statements of work that occurred last October, and so, if you're ready, I would love to take questions or comments.

DR. REICHERT: I'm sure you guys can guess where I'm going with this, but it's the same issue with Number 5.

DR. SEDBERRY: Okay, and we can use the similar wording that we used on black sea bass for this.

DR. REICHERT: Yes, but I don't think there's a selectivity issue here, and so that's not a problem, but it's just clarifying Number 5. Thank you.

DR. SEDBERRY: Thank you, Marcel.

DR. SERCHUK: Can you remind us, Kathleen, what year the SEDAR 28 assessment took place and what year the terminal year was in that assessment?

MS. HOWINGTON: Yes. Give me one moment.

DR. SERCHUK: In the black sea bass, you talked about from a terminal year of 2015, and what is the terminal year that was included in SEDAR 28 for Spanish mackerel?

MS. HOWINGTON: Give me two seconds. I am having to pull it up right now. I am looking at the SEDAR 28 South Atlantic Spanish mackerel, and the terminal year is --

MS. NEER: It's 2011.

MS. HOWINGTON: Thank you.

DR. SERCHUK: The only reason I raise that is -- I know we've had this discussion before, but it will be about ten years that have transpired since that model was used, and, typically, in that span of time, there are a lot of developments that occur in modeling, and so I'm not saying the model that was done back then should be updated, but it seems to be that there may be other models that might be more appropriate that have been developed in the past decade, and I know we have consistent with previous assessments, but I'm just wondering -- To have that much time go by and not re-look at models -- That seems to be a very long time, indeed. Thank you.

DR. SEDBERRY: Thanks for that comment, Fred. I don't know what I can add to that, other than I think, if we consider new models, it becomes something other than an operational assessment, but I'm not sure about that.

DR. SHAROV: I had exactly the same question as Fred. I understand that this is sort of a Center description under Item Number 1, or TOR Number 1, and so, initially, when we say update the existing model, or I guess that's for the consistency, but then we're asking to provide alternative models as needed, and then we say, in the end, apply the current BAM configuration, and so apparently we started from you can use the previous model, and then use whatever you think is most appropriate, and then we say use actually the BAM configuration, and so we already pre-determined which model will be used in this assessment, and it's a little bit confusing, although I understand that it's probably more just a question about wording, but we need to maybe give some more -- Get some clear ideas, and so what are we asking actually here, in terms of the modeling exercise? What do we want this team to do?

DR. SEDBERRY: I think that's an interesting question. Chip or Kathleen, what do you think?

DR. COLLIER: If the model changes, that is definitely not an operational assessment. Essentially, that would have to be a research track assessment, and what they are trying to do is just update the previous BAM model with modifications that you guys have observed through time since 2012, and I think that's when that assessment was completed, and so there's been slight modifications to BAM, based on recommendations either from the CIE or from the SSC, and those configurations are within the BAM model now, and they would be using -- I believe they will be using those configurations as an update SEDAR 28.

MS. HOWINGTON: Yes, that's how I read it as well.

DR. CROSSON: Marcel and I were on both of the last two Spanish mackerel SEDARs, and I know there was some -- The one before that, there was some problems determining overfished status in the time series, and so I think there was some shift between -- I mean, the last one basically resolved some of the uncertainty that we had in the previous SEDAR that was done on Spanish, and so I don't remember anything being controversial about this one that we did in 2012.

DR. REICHERT: That's right, and maybe -- I don't want to ambush Erik, but, Erik, do you expect -- I'm not sure if you have the model kind of in the back of your head, but do you expect any major changes to what was used in SEDAR 28 relative to now that may dramatically change the outcome, because I think the dilemma here is either we agree with these terms of reference or we, as an SSC, should make a different recommendation, which means, since we only have an operational or a research track, it means that we need to go the research track direction, which I'm not sure if that's justified just yet, but, Erik, can you possibly address that?

DR. WILLIAMS: Sure, Marcel. I would be glad to chime in. Thanks. I've been in this business long enough to know that anything can cause these models to change, or at least the results to change, quite a bit, but, that being said, this particular case I don't expect major changes. The one that I know that we do want to address that was brought up in the last review is we had run this as a split-sex model, and we didn't really have much data that was actually divided by sex, and so, in the end, the reviewers recognized that it didn't really add much to the model, and so, that being said, if we collapse it to just a single-sex model, I suspect it's not going to change the outcome, because of exactly what the reviewers picked up on, that we just sort of overcomplicated the model and didn't really gain any information from it by going to the two-sex model.

That would be what the only what I would consider semi-substantial change. Otherwise, the basic data is not going to change. You've still got age information, and we have index information and all of that, and we would apply the current BAM configuration, which includes incremental changes to things like adding different likelihood functions in, and I am trying to think back that far what we might not have had that we do have in there now, maybe another selectivity option, and that's about it that's coming to mind. Yes, I would -- With all that said, I don't think I would consider this a big enough change to kick it up to sort of a research track.

DR. REICHERT: Thank you, Erik. That helps me. I think I would be -- Although I realize the SEDAR 28 is definitely a long time back, and we -- Of course, no one would like to leave that much time between assessments, but that's the reality, and I think I would be comfortable with leaving this, or as an SSC leaving this, as an operational assessment, but Fred or others may have other ideas. Thanks for that explanation, Erik.

DR. SEDBERRY: Thanks, Erik. That helps me to understand it too, and I agree that it should remain an operational assessment, and maybe that TOR 1 needs some wordsmithing to avoid the confusion about the different models.

DR. SHAROV: The way it's written, it looks to me like, initially, the team is expected to just simply update the model with data through 2020, but you will, or they will, run the previously-developed model, right? It says update the approved SEDAR 28 model with data through 2020, which means you enter the data through 2020 and then you run it, and then we say that it needs to be modified, and so, at least for me, the confusion could be avoided if you just skip the middle sentence and say update it the approved SEDAR 28 Spanish mackerel model with data through 2020 and then apply the current BAM configuration, incorporating improvements developed since SEDAR 28, and that will make complete sense.

DR. SEDBERRY: Thank you for that suggestion, Alexei. That clarifies it to me too, but I just want to make sure that we're not leaving out some kind of important detail there in the middle,

and I don't think we are, but I agree with what you just said about leaving that middle sentence out.

DR. SERCHUK: I can agree with Erik's comment, and I'm not trying to create any problems here, but it does concern me that such a long period of nearly ten years goes by without really looking at whether the model is still appropriate, and I recognize that we don't expect very many changes, and we do have some major changes in recreational landings that have happened since then, but I'm willing to accept the language as it is. I just wonder whether we can give some thought to how long of a period to go, and I realize, without getting into the specifics of particular species, before we start thinking about, well, maybe we need to have a research track, because so much time has gone by, and I know we can't discuss it now, but maybe that's food for thought for our next in-person meeting. Thank you.

DR. SEDBERRY: Yes, Fred, and maybe that's something that the SEDAR Committee could take up and come up with a white paper or a plan or some kind of protocol that the SSC could review at our next in-person meeting.

DR. SHAROV: I agree with Fred in general, but I think we should defer to the expert opinion, and I think Erik is one of the individuals that knows better where the stock is and where the model is, and I think we should rely on his evaluation of how much of a change is expected there, and so I think we should just leave it there.

As for the time that runs between the assessments, that is obviously a concern, and the methods do evolve, but, as Fred knows for sure, there is still VPA models used in Woods Hole, I guess for a period of thirty years or so, and they have not changed yet, and so we are all guilty of that, but, in this case, I think we should realize an expert evaluation and just accept what the team says that they will do.

DR. SEDBERRY: Thank you, Alexei.

DR. REICHERT: I agree with Alexei and Fred, and I think it's probably good for the SSC, in our report, to at least mention that we agree with these terms of reference as modified, but that there was some concern within the SSC with the long time between the previous assessment and this operational assessment, and maybe we can add some language that it would be good to develop some guidelines as to how to approach that, and so I think -- Because we have raised that in the past, and so I think it's important for us to have that reflected in our report.

DR. SEDBERRY: I agree, and so that's a recommendation that needs to be recorded in the notes, so that we can make sure it makes it into the report as a recommendation.

DR. ERRIGO: I'm sorry. I missed that. What is it that needs to be recorded in the notes?

DR. REICHERT: That the SSC discussed the long time period between the last assessment and the --

DR. ERRIGO: Yes, of course. I will put that in the notes.

DR. REICHERT: And some concerns about changes in the model that may occur.

DR. SEDBERRY: Given the long time, yes. Any other comments or questions on the terms of reference?

MS. HOWINGTON: Just to clarify, am I deleting that sentence, or am I keeping it in?

DR. SEDBERRY: I think the consensus was, or at least nobody objected, that that sentence should be deleted.

MS. LANGE: That was my question as well, whether that sentence was staying or not. Thank you.

MS. HOWINGTON: Would you like me to also remove that in black sea bass and red snapper?

DR. SEDBERRY: I think there was some concern about the time period for this particular one, but I'm not sure. It didn't come up.

DR. REICHERT: I think the time period in Spanish mackerel was much longer between assessments than the other ones, I believe.

DR. SERCHUK: I think we want to be careful here, and it really relates back to the definitions of what an operational assessment is and what a research track assessment is, and I think, in normal terminology, an operational assessment -- Let's say it was conducted four or five years after the previous benchmark assessment, or previous research track assessment, and the second sentence would be completely appropriate, and we didn't expect to have large changes in the modeling structure if the last assessment was only done four or five years ago, as it was done for black sea bass.

The second sentence I think was appropriate, given that, in most cases, operational assessments would be conducted on a more timely basis than every ten years, and so I think, from my perspective, that's why I thought the language for an operational assessment, in terms of Term of Reference 1, was appropriate, but, because this assessment has been done quite some time ago, maybe it isn't appropriate in this particular case. Thank you.

DR. SEDBERRY: Thanks, Fred. I think that clarifies the question that Kathleen was asking in regard to the black sea bass and red snapper.

MS. HOWINGTON: Yes, it does. Thank you, Fred.

DR. SEDBERRY: Any other comments or questions on the terms of reference before we move on to the schedule? Okay, Kathleen. I think we can talk about the schedule.

MS. HOWINGTON: All right. Like I said, it is the same schedule as black sea bass, starting in mid-2021 and ending in April of 2022, and it is all webinars, and there is no workshop, and so, with that in mind -- Well, are there any questions or comments about that schedule? Okay. Do I have any volunteers to participate in the SEDAR operational assessment for Spanish mackerel, starting in mid-2021 and ending in April of 2022?

MR. ADDIS: I will participate.

DR. SEDBERRY: Thank you, Dustin.

DR. LANEY: I am willing to volunteer for this one, also.

DR. SCHARF: I can help with this one, too.

DR. SEDBERRY: Great. Thank you, Fred. That's three, and I believe that's enough for an operational assessment like this.

MS. HOWINGTON: Yes, I think that would be great, and so I have Dustin Addis, Wilson Laney, and Fred Scharf. Then I believe, with that, unless we have anything else that anyone wants to discuss about these terms of reference, then it would be public comment, and then I think my section is finished.

DR. SEDBERRY: I believe you're correct, Kathleen, and thanks for your input and guidance on this and for leading the discussion. At this point, I will take public comment on the terms of reference for Spanish mackerel. Do we have any public that would like to comment? You can raise your hand. I see Rusty has his hand raised. Go ahead, Rusty.

MR. HUDSON: Thank you. This Spanish mackerel SEDAR 28, I was a participant in that in 2012, and we had a terminal year of 2007, and so we're already thirteen years out. Back then, the BAM model wound up being more appropriate than the ASPIC model that was used, and my question has to do with what do we call the 2020 season? Is that what ended on 29 February 2020, or will it end on 28 February 2021? My concern is the MRIP estimates that have been revised and the PSEs associated with that, being final versus whatever, in motion during that assessment, versus commercial census, which is a lot more reliable, the landings in a census.

SEAMAP, I kind of wondered how they operated, in order for their sampling and stuff, because recruitment, with Tracy's recent presentation, wasn't as great on Spanish mackerel, like it was with king mackerel, and so are we talking March 1, 2019 to 29 February 2020, and is that the 2020 year, or is that a 2019 year? I guess that's my ultimate question. Thank you.

DR. SEDBERRY: Chip or Kathleen, can you answer that question?

DR. COLLIER: I cannot remember if this species was set up on a calendar year or on a fishing year. I would have to look into that.

MR. HUDSON: It's monitored at the SERO website as a fishing year and not a calendar, like the previous two. I understood that Kathleen said it would start at the same time as the black sea bass, and I was kind of concerned about what you did and would have finalized, versus sort of still in flux about the data.

DR. SEDBERRY: So is your concern about what constitutes the terminal year?

MR. HUDSON: Yes. What's 2020? Is 2020 what ended on February 29, 2020, or does it end 28 February 2021? That's my question.

DR. SEDBERRY: I don't know, off the top of my head, how we handle things that the fishing year is different from the calendar year, what we would call the terminal year in the assessment, because my brain is not working right now, but somebody must know the answer to that.

MS. NEER: I don't know what it is specifically for Spanish, and I would have to look it up in the South Atlantic, but we just will determine whatever fishing year we're using, and that's what it would be, but the assessment is still run on a calendar year, but the data comes in at the terminal year, a fishing year, I believe. Erik can answer that question though, and we would have to look up what the fishing year is and then make adjustments to have the right data.

MR. HUDSON: That's why I brought it up, because I did both cobia and Spanish, and Spanish is pretty important to a lot of my people that I deal with here in the South Atlantic Council region, and this is only a South Atlantic Council one, like it was before, because we did cobia, and that was sort of a combo and the other genetic issue and all kinds of other stuff going on, but, no, the Spanish, it looks like people keep catching, and then we're actually starting to close earlier here lately, and so I'm just not sure what's going on with the stock, and it should be pretty strong, based on what everybody is saying and the levels year in and year out.

DR. SEDBERRY: Thank you for that comment and question, Rusty. I hope we answered your question.

MR. HUDSON: It will be, George, whenever we get to the assessment, but I just didn't know if there might be a little timing issue in trying to start the same time as the black sea bass, but, having participated in all of these -- Well, not all of them, but being on the periphery anyway on some of them, thanks.

DR. SEDBERRY: Thank you.

DR. COLLIER: George, before we go on to the next topic, I was wondering if I could get volunteers for that selectivity study that we're going to be doing outside of the SEDAR process.

DR. SEDBERRY: Sure. Yes, we can do that next, and then we have one more SEDAR agenda item, and then we'll take a break. You need volunteers for an in-person selectivity workshop that's going to deal with the Florida study on trap and video selectivity for red snapper, which also collected data on vermilion snapper, and maybe red porgy and black sea bass and some other species as well, and this workshop is going to look at that data and see how it might be useful for those assessments, and is that correct?

DR. COLLIER: That is correct.

DR. SEDBERRY: That workshop was planned to be an in-person meeting sometime this summer, or early fall.

DR. COLLIER: Early fall might be more likely.

DR. SEDBERRY: Okay. Any SSC members that would like to volunteer to participate in that?

DR. REICHERT: Since I may have some knowledge of the topic, I volunteer, although I expect that I may have been asked as an outside SSC participant also, but I'm happy to participate in that.

DR. SEDBERRY: Thank you, Marcel. We need a couple more.

DR. REICHERT: I am not going to be the only one on this panel, am I?

DR. SEDBERRY: No. Chip, where is this in-person workshop likely to take place?

DR. COLLIER: I am not certain yet. We're still in the planning phases.

DR. REICHERT: I think it all depends on if we are actually able to meet in-person, I guess.

DR. SEDBERRY: Yes.

MS. LANGE: If you need another person, I can likely be available for that, this fall especially, early fall.

DR. FLOWERS: I could also help out, too.

DR. SEDBERRY: Thank you, Jared. Thank you, Anne. Chip, was it your feeling that three would be enough?

DR. COLLIER: Yes, that would be good.

DR. SEDBERRY: Okay. Then I think we are there. Thank you, volunteers. The last agenda item we have under the SEDAR agenda item is a replacement for Rob Ahrens on the scamp assessment. As you probably know, Rob has resigned from the SSC, and so he was on the scamp assessment and another workgroup, and so we need to find a replacement for him on the scamp assessment, which is a research track assessment, and I think it's the first research track assessment, and so it was done kind of differently.

Marcel can fill you in on the details, and I just volunteered you, Marcel, if you have any questions about how it's being done, but we need someone to -- We need a volunteer to replace Rob on that panel, which will be conducting the assessment and the review, and is that right, Marcel?

DR. REICHERT: I think the -- Julie may be able to chime in, but the results will be going to the SSC, right, Julie?

MS. NEER: Yes, and so we're looking for a replacement for Rob. He was serving on the Assessment Development Team, and that's that core group of SSC and scientists that are going to be involved in all stages of the process, and so data, assessment, and even potentially webinarizing into the review workshop, and there was a CIE review for this, and it's like a benchmark, and it's called a research track, and it's the new method that we're using, and so it is a commitment in time, and it is going to be -- We have already started the data process a bit, and we had our in-person data workshop that got cancelled, and that was supposed to be in March, and so we're doing the process via webinar right now for the data portion, and there will be assessment webinars over the summer and into the fall, on the current timeline, and a review workshop, on the current

timeline, being held in March, though most ADT members will not have to come to the review workshop.

Then, ultimately, it comes to the SSC at the end of that whole process, and so it is a commitment of probably, I would guess, at least the upcoming year as an ADT member, and so consider that when you're deciding whether you want to volunteer for this, and that is sort of the bad side. The good side is this is the first research track we're ever doing, and these ADT members are going to be critical in helping us redefine and narrow in on how this process should go and what works well and what doesn't, and so you get to help get in on the ground floor with this new process, and so that's kind of the carrot to sign up for this long haul. Thanks.

DR. SEDBERRY: How many webinars or in-person meetings are you looking at?

MS. NEER: We should probably have no in-person meetings, unless we need ADT members to attend the review, but we don't know that that's going to happen, but I would guess lots of webinars, and I don't know, because we haven't -- We don't build that far out. We build up to a certain point in the schedule, and, given it's a research track, then we assess where we are and see if we can move on to the next stage, and so, as you said, it's a little different.

Probably I would say four or five assessment webinars, would be my guess, but we're not even sure when those are going to start, because the data is a bit behind, because we had to cancel that in-person workshop, and so it would be late summer or fall, is when we were planning on having our assessment webinars, of this year.

DR. SEDBERRY: Okay. Thanks. Any volunteers? Churchill, maybe?

MS. NEER: Just one note that the ADT is serving -- It has members from both the South Atlantic and the Gulf of Mexico SSC's on there, as well as Science Center representatives, because remember initially that scamp was going forward as one big assessment, until stock ID split it, but the ADT is still serving on both assessment panels.

DR. SEDBERRY: Okay.

DR. GRIMES: George, I'm already signed up for the tilefish, along with you.

DR. SEDBERRY: That's right.

DR. REICHERT: It's a very fun group, just so everyone knows. The ADT is a blast.

DR. SEDBERRY: I just thought of Churchill because of his vast experience in the region and the long history and perspective he would bring to this, but I understand. I can say the same thing about tilefish too, and you've got a lot of experience there, too.

DR. ERRIGO: Church, I think he just called you old.

DR. GRIMES: He's right, but he has to look in the mirror and say the same thing for himself.

DR. SEDBERRY: I don't see anybody raising their hand, and so, in this case, maybe we'll give it a day or two, and, if you are interested, you can send me an email, to let me know, or, if you have any questions about it, I'm sure that Julie or --

MS. NEER: They can contact me if they have any questions.

DR. SEDBERRY: Julie would be able to answer your questions, and Marcel has firsthand experience too, and so --

DR. REICHERT: Alexei is also an ADT member. Julie, it may be good to let people know when the next plenary is scheduled, because I think it would be really good if we have someone available for that plenary.

MS. NEER: Currently, the next plenary is scheduled for May 26, I believe. It's the last week of May.

DR. REICHERT: Okay. Thank you, and so it would be really good to have someone, and, just as an aside, I am chairing the plenary, and so, for me, it's kind of difficult to have two hats on, and so it's really helpful if there is an additional South Atlantic SSC member on that panel, and so thanks.

DR. SEDBERRY: This panelist is subject to approval by the council, right, and so they wouldn't be approved by that May meeting, but they could still participate unofficially, and is that how that would work?

MS. NEER: It's possible that they could be approved via email, and I'm not sure, but that's fine. They could absolutely participate though, listen in and everything. The SEDARs are all public meetings.

DR. SEDBERRY: Right. Again, if you're interested in volunteering, and, as Marcel says, it's a very exciting panel to be on, and so drop me or Julie a line and let us know. Hopefully we can come up with a name before the end of our meeting this week. Is there anything else that we need to talk about, in terms of SEDAR, in terms of this agenda item, which is Number 3? Okay. I am going to propose that we take a short break here, fifteen minutes, and so let's be back at five minutes to four. We are recessed until 3:55.

(Whereupon, a recess was taken.)

NEW SCIENCE CENTER RECREATIONAL WEIGHT ESTIMATION PROCEDURE AND UPDATED ABC'S FOR UNASSESSED SPECIES

DR. SEDBERRY: We're back. I hope you had a great break, and our next agenda item is the new Science Center recreational weight estimation procedure and updated ABCs for unassessed species. Our notetakers for this agenda item are Chris Dumas and Jared Flowers, and Mike is going to run us through the spreadsheet that you should be sort of familiar with from our MRIP workshop last summer, and we've seen it a couple of other times, but it's Attachment 5 in the

briefing book, and there is two other attachments associated with this agenda item, Attachment 6 and Attachment 7.

As we heard at the MRIP workshop last summer, the Fisheries Science Center has modified the methodology, mainly the sample size, for the hierarchical application of average weights, the way they estimate weight of the landings, and so the ABC recommendations we made in October last year for unassessed stocks, that we did at our October meeting, were based on the old method, and the new method that Mike has applied has resulted in slightly different landings estimates, and so, therefore, the ABC that we previously recommended might be affected by this, and so we've been asked to look over the ABCs for unassessed stocks and decide about updating the original ABC with the new landings data or if additional consideration is needed.

We're not asked to review the methodology, but just to review the effects of that methodology on the landings estimates and ABCs, and so the council has also asked that we review dolphin wahoo ABCs, given that our third-highest landings value we used was also the second lowest, because of a very short time series of four years for that method, and that we maybe consider a longer or a different time series or to use ORCS instead of the third-highest to set ABC for dolphin and wahoo.

I think the way we're going to do this is we have several action items, but Mike is going to bring up the spreadsheet, and we'll talk about the snapper grouper aspects of that first, and then the dolphin wahoo aspects, and so we'll look at the spreadsheet, and Mike is going to point out the significant changes that these new weight estimates have done in the landings, and then he will go through that for the snapper grouper, and then we'll take public comment, and then we'll have discussion on snapper grouper, and then he will present the same kind of thing for the dolphin wahoo, and we'll take public comment and then discuss dolphin wahoo. We have several action items for this agenda item, and we'll talk about those after Mike runs through that spreadsheet to highlight the differences.

DR. ERRIGO: I just wanted to start out, real quick, by going over what the methodology was and how it changed, and I will do that real quick, and we did receive a presentation on this at the MRIP workshop back in August, and what it is, as most people know, is the Science Center uses a different methodology from converting recreationally-caught fish, landings in numbers, to weight than the MRIP does.

What they do is they have this minimum sample size of fish that they use to convert from numbers to weight, and they collapse across strata in a hierarchical fashion until they get their minimum sample size of fish, and, once they have that, they apply that average weight across all the fish within that strata, or collapsed strata. Originally, the sample size was thirty weighed fish, and that is what the landings were based on when we originally looked at all the landings data and made the ABC recommendations in October.

Since then, the Science Center has changed that methodology and is now using a fifteen-fish minimum sample size cutoff, after doing a study to look at the cost-benefit analysis of reducing that sample size and that they lose versus what they gain for reducing the sample size, and it turns out that, on the curve, fifteen fish wasn't -- It turned out to be that they were gaining a lot in -- They weren't losing a lot in terms of precision, but they didn't have to collapse as much across strata, and so they went to a fifteen-fish minimum sample size, and that changed the landings in weight that we saw last time, and so what we need to do is we need to just make sure that you guys

are okay with the new landings trends, and we can re-calculate the ABCs based on the new landings, rather than the landings that you saw in October.

Like George said, we'll go over the snapper grouper species first, and, for the most part, what I've been looking at, what I've been seeing is that there is not a huge difference between the two. In all of these graphs that I will show you, the red is the old weight estimation of thirty fish that we saw in October, and the blue is the new weight estimation procedure using the fifteen-fish minimum, and that's the same throughout.

This is spadefish, and it's one of the ones that has -- You will see, from year to year, there's not a huge difference, but there are some years that have a big difference, like this one here in 2014. Originally, there was this humungous spike that was caused by the weight estimation procedure, which now has gone away, and so there's a big difference in that year, and there's a big difference in this particular year, 2002, and, in fact, this year right here, this difference here, is what's causing the difference in the ABCs, this large difference in ABC.

What I have noticed is that -- This is a graph of the sector landings, and so the green is commercial, but the orange is the old recreational, and the blue is the new recreational, and the gray is MRIP. The blue line, the new, the fifteen-fish minimum cutoff, is closer to the MRIP weights than the old weight estimation procedure was, and that mostly holds throughout. That's spadefish, and you see there's a big difference there, but, for most other species, the differences aren't that huge. Here is bar jack, and you will see the differences are pretty small in most years, and here is black grouper, and you're looking at the red and blue lines, and you can see they almost overlap perfectly, and you can't even see the difference in the ABC estimate.

Gray triggerfish is the same way, and it overlaps almost perfectly. Hogfish does overlap perfectly, and here is scamp, and here are the species in the deepwater complex, and there is almost no difference in most of them. This is jacks, and there is very, very little difference. Snappers, there is very little difference, and you can see some difference in the ABCs here, and that's due to these higher values here.

Grunts, again, there's really no difference, and there's a little bit of difference in margate in this portion of the time series, but, overall, it's pretty small. Then shallow-water, and you can see that they overlap very well. Sometimes there's a couple of years where they are different, and this is porgies. It's very, very close to the original. That's it for snapper grouper. Do you guys want to just discuss that, or do you want to take public comment and then discuss those species, and then we could move on to dolphin and wahoo?

DR. SEDBERRY: Thanks, Mike. I think the way we'll proceed with this is to take public comment now and then have the SSC discuss and make any recommendations or consensus statements we can come up with for the action items and then move on to dolphin wahoo, and so, right now, I will call for public comment on the weight estimations and ABC recommendations for unassessed snapper grouper species, and so is there any public comment for this agenda item regarding snapper grouper species? I don't see any hands raised. Going once, twice. Now we'll take clarifying questions or comments, et cetera, recommendations from the committee. If you have anything, please raise your hand.

DR. SERCHUK: I wonder whether we could walk through one of these presentations in a little bit more detail, just to see what was done, and can we start with the spadefish? Is that the first one?

DR. ERRIGO: Yes.

DR. SEDBERRY: Another thing you might look at from your briefing book, and I can't remember which attachment it is, but it's the presentation from the Center on the change from a sample size of thirty to fifteen, and you look at the curve there that shows the precision or accuracy, and it's kind of a rarefaction kind of curve that really reaches an asymptote around fifteen fish, and there's not much difference between fifteen and thirty, and so it really is not adding a lot of additional information for that additional sample size, and, in the meanwhile, Mike can run us through one species as an example, to look at the details of what was done, and so go ahead, Mike.

DR. ERRIGO: These are just landings here with the ABC in place, and the blue is the new fifteen-fish methodology, and the red is the old thirty-fish sample size methodology. The difference is how they're converting from numbers of fish to weight, and what it is is they have a series of different hierarchical strata, and so it's like species, year, wave, mode, state, area fished, and they look at the number of samples in that lowest category.

DR. SEDBERRY: Mike, if I could just interrupt. Because the sample size is lower, they have to -- They don't have to go as far in the hierarchy as they did before, and so the sample size may be lower, but it's actually more precise, or more relevant, to the fishery at hand, if you know what I mean, because they are really focusing in on the species size and location, et cetera, without having to add additional samples from other areas that may not be as relevant.

DR. ERRIGO: Yes, right, and, being that the sample size is lower, you would have to condense less across strata, which means that the weights that you're getting are more relevant to the fish that you're applying them to, and they match up better with the MRIP survey, which is why the fifteen-fish -- This light blue line, the fifteen-fish methodology, matches better with the MRIP waves, which is the gray line, whereas, before, we needed thirty fish, and they were collapsing across more strata more often, and, a lot of times, you could get things like this 2014 spike, which didn't appear in the MRIP data, because you're applying weights of fish that may be way outside of the strata that the fish were actually sampled in.

They determined that you're not losing a ton of precision by going from a thirty-fish minimum sample size down to a fifteen-fish minimum sample size. You are matching up better with the MRIP survey of how the fish are surveyed, and you're not losing very much precision when you do that. Does that help?

DR. SERCHUK: Could you also explain the comparisons under the ORCS table?

DR. ERRIGO: The old value is the -- This is using the data from thirty-fish minimum size methodology, and the new value is using the data from the fifteen-fish minimum size, and these are the different like scalars and year ranges. For ORCS, you take the maximum landings within a certain year's range, to get your catch statistic, and then you multiply it by the associated scalar and then the council risk factor to get your ABC.

DR. SERCHUK: Okay, but, in this case, Mike, in both cases, it looks like the ABCs, either because of the scalars, are generally above almost all the data points in their respective time series, and would that be correct?

DR. ERRIGO: The new ABC is above --

DR. SERCHUK: In both cases, the old value was close to two-million, and there was no values in the old dataset except for that very high value in 2013 or 2014, and now, with the highest values that we have with the blue series, which is the new series, we have three values that are 1.5, but most of the values are lower than that, and so we're just saying that we believe the stock is underexploited in both cases?

DR. ERRIGO: Yes.

DR. SERCHUK: Okay. I'm just trying to get a handle on this, because, typically, when we don't see -- Okay. That's just a function of the ORCS method, correct?

DR. ERRIGO: Yes, it is a function of the ORCS method, and that's right.

DR. SERCHUK: Thank you.

DR. ERRIGO: I am just noticing now that these are mislabeled. This is the new value, and this is the old one. I am very sorry about that. They are labeled correctly here.

DR. SERCHUK: Now I'm even more concerned that we have a value for the ABC close to two-million, and we've never seen a point in the blue series that high.

DR. ERRIGO: Right. That's how the ORCS method was developed. If you have no concern over a stock, you want to set your ABC value in such a manner that you don't -- If you have regular variability, it won't accidentally close your fishery down.

DR. SERCHUK: I understand that, but most of the high variability is three datapoints, and they're all about 1.5, and I'm not trying to go back and change the methodology, but I'm just trying to understand it from the man-from-Mars point of view that comes in and looks at the history of the fishery and then looks at a value of an ABC that's quite a bit higher than almost all the data points.

DR. ERRIGO: It is, and that's why it is. That was the impetus behind the ORCS approach, was to set a value, in a fishery you have no concern over, that wouldn't accidentally shut down your fishery based on the variability in the landings.

DR. SERCHUK: Okay. Thank you.

DR. SEDBERRY: Any other discussion? Our action items here are to review the new landings time series data for all unassessed stocks, due to the change in weight estimation, which we just did, and consider updating the previous ABC recommendations based on these new landings. This is just for snapper grouper species. The action items for dolphin wahoo are slightly different, and so do we want to update previous ABC recommendations based on these new landings? Does anybody have any thoughts in regard to that?

DR. REICHERT: Since the updated -- Correct me if I'm wrong, but, since the updated data are considered best scientific information available, I think it would be good for us to update those ABC recommendations based on those new numbers, given the fact that we are not changing the way we came to the ABC recommendations earlier, and we're just correcting the numbers. Is that right, Mike?

DR. ERRIGO: That's correct. The question was whether you agree with using this methodology and simply updating the ABCs based on the new weight estimation methodology.

DR. REICHERT: The new weight estimation methodology, when I looked at it, did not make me reconsider the original methods that we used, and so I am okay with updating these new numbers.

DR. SEDBERRY: Very good, Marcel. That was nicely put, and I agree with you. I don't see that this changes our best scientific information available determination, but I see that Shep has his hand raised.

MR. GRIMES: I was just going to say that it seems that, in some respects, what constitutes the best available information has changed. The Southeast Science Center has changed its methodology, and here are the new weight estimates, and, in looking at this, I agree with what Marcel was saying, like go back, and it seems to me, on a case-by-case, or stock-by-stock, basis, and saying, okay, well, in this instance, we have this new information, and, yes, the ABC should have been higher, based on these new numbers, and we don't want to change our approach, but here's the new number. In other instances, maybe you would look at them and say, well, the new information is indistinguishable from the old, and it may not be worth changing your ABC recommendations, but I think we do need to build a record for looking at the new weight estimation methods and the numbers that come out. Thank you.

DR. SEDBERRY: Thank you, Shep.

DR. SCHARF: Just apologies if I missed it, and I had to step out for just a second, and, Mike, it seems like the colors on the graph got switched a little bit in the first plots that include the ABC estimates. It seems like the black line says that's the new one, and the purple is the original, but it doesn't match the table down below.

DR. ERRIGO: The table below was mislabeled. I corrected it on the screen, and the graph is correctly labeled. Purple goes with red, and black goes with blue.

DR. SCHARF: Okay. Thank you.

MS. LANGE: I just wanted to agree with Marcel. I think that, if these are the new data, then the methodology shouldn't change, but the estimate should change. Shep indicated that, if it wasn't a big change, you may not want to actually change the ABC, but I'm wondering if we should be consistent. If these are the new data that we're using, shouldn't all of the ABCs be adjusted to new numbers, based on the original methodology?

DR. ERRIGO: George, I can speak to that, actually. That's a good question, Anne, and, in regard to what Shep said, I think that would be an interesting, good, consideration if these were actually

in place yet, but these were never implemented by the council. You made recommendations, but the council still is struggling with allocations, and so they need to figure out the allocations issue before they can implement these ABCs, and so they haven't been implemented yet, and so they're still just your recommendations, and so updating all of them, even if they're not very different, I think is not a huge issue, but, if they were already in place, then it would take an amendment and a lot of work. If the difference was 100 pounds, or like 0.1 percent, then, yes, you might want to consider saying it's not enough a big enough of a difference, but, in this case, they haven't been implemented yet, and so just changing the numbers is easy.

DR. SEDBERRY: I agree with that as well, and consistency is important, too. I don't think any of us would disagree with that.

DR. SERCHUK: I have one more technical question. In terms of the range of years that were selected, are the same years used in both comparisons, or were they selected because they were representative of maybe normal fluctuations in the fisheries at that time? I am just wondering if the new series showed quite a bit of difference in terms of the stability or some other factor that was used to select the range of years -- Would the range of years comparison be appropriate in both cases?

DR. ERRIGO: I did not use the dataset or trends to choose the years. I used the same years that were originally chosen in the original ORCS methodology. Those years were used for all species.

DR. SERCHUK: Okay. Thank you.

DR. SEDBERRY: Any additional questions or comments? I think the consensus of the committee is that we accept the new weight estimates and their adjustment to the landings and the resulting change in the ABC, and is there any objection to that consensus statement? Okay. I don't see any hands raised. I think, Mike, we're ready to move on to dolphin wahoo, and maybe you can go into some detail about why we're addressing this separately and how the issues are a little bit different from snapper grouper.

DR. ERRIGO: Yes, and so, for dolphin and wahoo, the council had a few concerns here. For dolphin in particular, they were concerned about the range of years and the methodology used to together, and so, for dolphin, the range of years was 1994 to 1997, which is four years, and then, in order to get the ABC, we took the third-highest, which happens to be the second lowest, this point here, which, as the council was discussing, they said, if it's a stock that has no concerns, why would you use the second-lowest in the time series, rather than a higher value, like we have been using in most other stocks, and one of the issues is that, if you take the third-highest in this time series, it comes out to be three-quarters, or 75 percent, of the average landings of this time period, and so you're reducing from the average.

For almost every other stock, the third-highest increases from the average, and, if you guys look over here, on the very left-hand-side, that's what this table shows. It's all the unassessed snapper grouper species, and it looks like the third-highest divided by the average of the time period that we took the third-highest from, and you will see that all of them either are at one, or most of them are greater than one, and the only two, besides dolphin, that are less than one are sailor's choice and yellowmouth grouper, and I have them here, and the reason is because they are rare encounters, and they have these big spikes, and then the rest of the landings in that time series are down here,

and here's yellowmouth. Again, it has this huge spike, and then the rest of the time series is down here, and so it pulls the average way up from where the third-highest is.

This one, the third-highest is down here, but the average is actually up here, because of these two high points, and the same with yellowmouth grouper. It has this one random humungous spike, and so it pulls the average way up here, whereas the third-highest is this point, and so that's what caused it in those, but that's not what caused it in dolphin. It's not a random spike that is pulling the average up, and so that was their concern. That was the council's concern with dolphin, and, for wahoo, they were worried about, again, a stock that there isn't any real issue with, yet, based on the time period that was used and the third-highest, it's really clamping down on landings. After this peak here, this actually comes way down again, and this only goes through 2017, because those are the only years that I had for comparison purposes. I had 2018 and 2019 that comes way back down here.

They were, again, worried about why clamp down on a stock that there isn't any real issue with, and so they were really hoping that, since you guys already -- You have deemed ORCS to be the best science, and if you could please reconsider the use of the ORCS approach in setting the ABC for these species, which is a possibility, as long as you justify why you're using ORCS. Even though it's not in the Dolphin Wahoo FMP, you can still use it, and so that's the story with dolphin and wahoo.

MS. LANGE: I am just wondering. Wahoo is different, but, for dolphin, there's only four years, and do you remember or have notes on why it was that we took just such a short time period, because, no matter what you do, the lowest-third or the highest-third is going to be -- The lowest-third is going to be the same as the second-highest, or whatever you want to call it, and so is there a way of expanding the number of years, possibly, to get a better --

DR. ERRIGO: You guys can definitely expand the number of years, and you can use a different part of the time series, and you can use the 1999 to 2007. You can do all kinds of things, and you're not married to that decision. That decision was made actually quite a long time ago, when this time series looked very different. The landings time series has actually changed in how it looked over the years.

MS. LANGE: I can't remember what the criteria were for selecting the time period. As you said, it's been a while.

DR. ERRIGO: There were issues with the landings way back when, and this was a time when the landings were more reliable, and they were a lot flatter than this, and they weren't as spiky, and so that's why it was chosen, but, over time, this time series looks significantly different from what it looked like when that decision was made.

MS. LANGE: Okay, but, again, was there a specific statement of how we selected the years? I thought there was some sort of criteria for what period of time we would use, and I just can't remember what that was.

DR. ERRIGO: These years were chosen for dolphin way before the discussions happened for ORCS and stuff, and even the third-highest with snapper grouper species, and so we didn't have the same criteria.

DR. SEDBERRY: For some species, we chose the time period to exclude the worst effects of the great recession, and also to look at a time period that had relatively stable landings, but, like Mike said, I believe, when these were chosen, it's been so long ago that those considerations weren't in place, and I just don't remember exactly why it was so short.

DR. ERRIGO: It was a reliability issue, and somebody had put together the landings from this time period, 1994 to 1997, and they felt that those were a very reliable, strong source and that the rest of the landings there were questions about, and we no longer have that -- That doesn't hold anymore.

DR. REICHERT: Mike, if I remember correctly, the reason we chose these years was, first, because, at the time, they were, as you said, more reliable and more stable, and, also, there was a regulation put in place that the SSC thought may affect the landings, correct?

DR. ERRIGO: There was a regulation that went into place, yes.

DR. REICHERT: So that's why we -- I think those were kind of three reasons, and, of course, we did not have the whole ORCS discussion and some of the other discussions. We didn't have the advantage of that discussion, and that's just digging into my recollection.

DR. SEDBERRY: I guess our choice here is to pick a longer and/or different time series or to pick the ORCS approach, which is not part of the FMP, but, if we can justify, we can use, and so I think that's the decision we need to make.

DR. ERRIGO: I do have some additional information that might help you with getting out of this 1994 to 1997 time period, or even just using the same 1999 to 2007 that was used for other species or choosing ORCS and then using those years, and I am working on, and this is a work in progress, but I'm working on a CPUE index for dolphin and wahoo. I didn't send it around, because I don't have documentation for it yet, but this is what it looks like for dolphin.

There were issues in the early part of the time series with the data, and so I wouldn't trust these early years. In fact, up through 1990, there are some issues with the data that I am trying to gap fill, but, from 1991 on, the data is good, and the red is the standardized DLM model, and the blue is the nominal for dolphin, and they pretty much both show that it's flat. There is no trend, and this is the one -- They are standardized to their long-term averages, and so one is the long-term average. That's this line here.

DR. REICHERT: Mike, can you remind us, and especially those not involved in the development of ORCS, what our standard time period was that we used for the ORCS methodology?

DR. ERRIGO: It was 1999 to 2007.

DR. REICHERT: Thank you. If we would apply the ORCS methodology, that would be the time series that we would look at and perhaps use as a reference, correct?

DR. ERRIGO: Yes, and so that's this year, 1999, to 2007, right here. Even though the landings look like they trend, this CPUE index doesn't show this same trend.

DR. REICHERT: Remind me. For the species that we apply the ORCS to, we assume that the landings trends was telling us something about the population, and so I think it's important for us to consider whether or not that is true for dolphin also, and so there weren't regulations put in place such that that may affect that signal, which I don't think there were, were there?

DR. ERRIGO: I don't think so, no. I think regulations were put in place earlier.

DR. REICHERT: The dolphin fishery has not shut down, correct?

DR. ERRIGO: Let me just say this is an MRIP index.

DR. REICHERT: Yes, I understand, but the previous graph --

DR. ERRIGO: This is total landings, but it's almost all recreational, and the recreational sector has never shut down.

DR. REICHERT: Okay. Thank you.

DR. BUCKEL: Mike, how did you determine what efforts were for dolphin, or did you use a combined MRIP effort?

DR. ERRIGO: I used the MRIP data, and there is no headboat data in there, although this goes through the north, and so into the New England area, and they do have headboat data in with the MRIP data up there, but the effort is angler hours, but it's expanded angler hours, and so it's expanded by the trip effort. There's a lot that went into that index that would need explaining, which is why I didn't send it around for everyone to see, because I don't know, but I have it, if you wanted to look at it, if you wanted it for ancillary information.

DR. BUCKEL: I appreciate you doing it, and I trust your judgment in making it, and I think it tells us that we can definitely either go with the longer -- Consider something even longer than ORCS, given how flat that CPUE looks, or just go with the ORCS, and I would be happy either way, but definitely it argues from moving away from just the four years that we used before.

DR. SEDBERRY: Yes.

DR. CROSSON: I think of dolphinfish, and the reason it's -- It's not really an ORCS, and the reason it's just never been assessed is that it has such a short life period, and it's a prolific reproducer, and it's highly pelagic, and it moves through our area, but it's certainly not limited -- The stock is just not limited to our area. They are roaming all over the Atlantic Ocean, and so I don't feel like we need to go in to apply the ORCS rule. I am just comfortable with taking a much longer time series than what we've been using for these three years, whether it's 1994 to 2007 or 1999 to 2007, but just taking the average landings and just applying that, and I don't think we need to go into the ORCS rule.

DR. ERRIGO: So you're saying that it's more than ORCS, but you're comfortable using the third-highest?

DR. CROSSON: No, I don't want to use the third-highest. Just take a big chunk of time and use the longest -- Take an average over the time period, like we've done with some of these other species, and that just seems like a practical -- I don't care -- I went and looked over the Excel sheet many times and the different ABCs you have up there and the average of 1994 to 1997, and it could be the average of 1999 to 2007, and it could be the average of 1994 to 2007. I think we should just pick a long time series and just use that. I mean, this is not a species that I think it's ever going to be assessed. I would be surprised if it was, but I will leave that up to the rest of the committee what they think, but I don't see the need to go into ORCS here.

DR. SEDBERRY: 1997 to 2007 is a time period we've used in a lot of other species, setting the ABC, but you're saying that it's not necessary to apply ORCS or use the third-highest out of the 1997 to 2007?

DR. ERRIGO: Just so you're aware, the reason why we haven't used averages in the past is because, if you use the average, you're going to cut off everything above the average. This is the ABC. This is the limit.

DR. CROSSON: Good point.

DR. REICHERT: Yes, that's what I was going to bring up.

DR. ERRIGO: The third highest, and, for ORCS, it's the highest value in the time series.

DR. SEDBERRY: Then, Scott, you would be arguing for the longer time series, 1999 to 2007, using the third-highest?

DR. CROSSON: Yes, and you reminded me about the effect of using the average value, and I would agree with that.

DR. SERCHUK: I am trying to reconcile some of our discussion now with some of our recommendations that we made in October, and, in October, for dolphin, and, again, the time series don't seem to have changed very much, it says the SSC recommends including Monroe County landings into the ABC calculation for tracking the ACL. The SSC originally recommended the years 1994 to 1997 as the reference period for dolphin, but the snapper grouper reference period years were used instead to calculate the ABC. This is corrected here. Then, in bold letters, our recommendation is the SSC also recommends adding the ORCS method to the Dolphin Wahoo FMP and revisiting this ABC at that time.

Now we're discussing not using the ORCS method, and I want to make sure that, if we're changing our recommendation from six months ago, that we provide substantial support for why our previous recommendation is no longer correct, and this is just a matter of parsimony and consistency. That's the point that I have on dolphin, but I have another point in terms of the previous stocks that I can make when you feel it's appropriate after we've left this topic, Chairman. Thank you.

DR. SEDBERRY: Thank you, Fred.

DR. BUCKEL: I agree with the things that Scott was saying before, and my recollection is the reason we picked the four years was we were -- We said all the things that Scott said about the life history of dolphin, and we picked four years where we were going to ensure that the landings weren't going to hit the ABC, and we wanted to ensure that that didn't happen, and so, if we go with 1999 to 2007 and pick the third-highest, then there's going to be a potential, and like the spike that happened recently would have hit that ABC, and so we may want to -- If we're all comfortable that this is a species that would be really hard to overfish, then we may want to set that ABC at something that is not going to be hit very often, based on the recent time series, and the 1999 to 2007, the third-highest there, might not do that.

DR. CROSSON: Mike, what is the punishment for the fishery if it does go over the ACL? Is this one that they have to -- I guess, for the recreational fishery, do they have to do it for several years in a row before there is any payback?

DR. ERRIGO: There's never a payback unless the species is overfished. For the recreational, if you go over, you monitor in-season next year, and, if you go over again, you close. I believe that's -- I mean, that's how it is for snapper grouper. If someone knows that that's different for dolphin and wahoo, please speak up, but I believe that's how it is for dolphin wahoo, also.

DR. COLLIER: Mike, I've unmuted John Hadley, and he's the lead for the FMP, if he wants to speak about it.

MR. HADLEY: For dolphin, the commercial ACL includes an in-season closure once the commercial allocation is met. For the recreational sector, there is -- I would have to go back and look at the exact mechanics, but it's essentially, as Mike Errigo mentioned, monitor for persistence, and then, essentially, if the landings are above the ACL, then there can be an in-season closure, but only if the species is deemed overfished.

The council, right now, is looking -- That's not really -- Since there's not likely to be a stock assessment on dolphin, the council is looking at revising that recreational accountability measure in Dolphin Wahoo 10, and I think how that accountability measure is revised will kind of be dependent on where this ABC is set, and so that's -- It will be changing in the near future.

DR. COLLIER: George, Anna Beckwith, one of the council members, has a question.

DR. SEDBERRY: When I saw her hand raised, it occurred to me also that I neglected to take public comment on the dolphin wahoo portion of this before we started our committee discussion, and so let's take the question from Anna and then see if there is any public comment, and so go ahead, Anna.

MS. BECKWITH: I appreciate the discussion. I think Mike Errigo made some great points and discussed sort of what the committee, Dolphin Wahoo Committee's, perspective was. I did just want to clarify that, at least from my understand, from your previous discussion, it seems like, during the October meeting, the SSC was comfortable with the ORCS approach for dolphin, and one of the main reasons that you guys did not pursue it was because it was not in our FMP, and so, as Mike had previously mentioned, as a committee and a council, we did discuss that, and we were comfortable with you guys having thought that ORCS was appropriate, and we wanted to make sure that you guys knew that you can move forward with ORCS.

If you do decide to move forward with something besides ORCS, I would like to have a very clear reasoning why, for whatever final decision you guys make, because certainly the council was quite comfortable with that previous discussion to move forward with ORCS, except that it wasn't part of the FMP, but that's my only comment, but I appreciate the discussion. Thank you so much.

DR. SEDBERRY: Thank you, Anna. Do we have any other public comment on the dolphin wahoo? I do not see any hands raised. Now where are we on this?

DR. ERRIGO: Just so everyone can see, I changed the purple line, and that's where the ORCS method would put the ABC. Right now, if we simply update the ABC based on what you guys had in October, it puts it way down here. If you use ORCS, it's way up here. The lower part of this graph, that's the third-highest, 1999 to 2007.

DR. SEDBERRY: That's essentially the same method we have now, and just, instead of the four years, it's using the 1999 to 2007?

DR. ERRIGO: Yes. If you want to use ORCS, it's that higher one.

DR. SEDBERRY: Okay.

DR. NESSLAGE: Earlier, someone had mentioned, council staff had mentioned, that they anticipate that accountability measures would change with the next I guess amendment, and does that mean that they'll be changing to be more strict or not? I think we're all kind of looking at this and going, okay, the ORCS-based ABC is higher than the highest reported landings, which might be fine, and we don't know, and we don't have a stock assessment, but we would want to see, if there was a sudden ramp-up that was trending, some sort of way to, I think, stop and take a look at the stock before it got out of control and things got overfished, and so I guess I don't know if other people would feel comfortable knowing -- Like in what direction do you expect the accountability measures to go? Will it be stricter, I guess, or more loose? What changes are being considered? Thanks.

DR. COLLIER: Luckily, John Hadley already had his hand up, and so I think he wanted to answer that question.

DR. SEDBERRY: Oh, good. Go ahead, John.

MR. HADLEY: The accountability measure, I mean, I guess you could say, in some aspect, it's going to become more strict, the reason being that, right now, the way it's designed, it will not likely be triggered at all, and, since there is a caveat in there that the recreational accountability measure does not get triggered unless the stock is deemed overfished, since it's not going to be an assessed stock, then that's not likely to ever occur.

That is why the council is looking at revising it, because it's not really an adequate accountability measure at the moment, since it's never going to be triggered, and so there is various measures that the council has considered, looking at potentially -- It's not clear, at the moment, because this is still kind of an action that's in development, but the council is looking at different ways as far as potentially allowing a year of persistence and then, if the landings are in fact elevated, then the

accountability measure is triggered, or it could be triggered right away if the landings hit the ACL, and so it's really up in the air at the moment exactly what that accountability measure will be, but, really, the point being taken out of there is that overfished language, since it's not going to be an assessed stock.

DR. SEDBERRY: Thanks, John. Just a time check here. We're very close to our scheduled quitting time, but as I mentioned when we started, we're probably going to run long today to finish up this agenda item, so that we can start in on the SEDAR reviews tomorrow. What we have left to do is finish this up, take final public comment on today's webinar, and then recap where we are with any consensus statements or recommendations that we've come up with today, look at Mike's notes and make sure we're comfortable with that.

How can we come to a resolution on this? I think where we're standing now is it seems to me that the committee is recommending that we reconsider the ABC using third-highest, but with a timeframe of 1997 to 2007. Is that what the committee is thinking?

MS. LANGE: That's what I was thinking. Again, four years is -- To do the third-highest with four years of data, I'm not sure, and I was going to try to look at this overnight, where that year decision was made, where that four-year decision was made based on, if I can figure that out, and I think it would be good if we could use the same method, just different years.

DR. SEDBERRY: Okay. I agree with you, too. Again, I'm thinking 1999 to 2007. Do any other committee members have any thoughts on this?

DR. LANEY: George, I'm with Anne and Genny on this. That makes sense to me, to broaden the years to 1999 to 2007 and use the third-highest, and, like Genny, it doesn't make a whole lot of sense to me to set a value that is higher than any value in the whole time series.

DR. SEDBERRY: Thank you, Wilson.

DR. SCHUELLER: I am just trying to see how this aligns with what we wrote in the October 2019 report, and so there's a table in there, and I'm looking at page 13, which is dolphin wahoo, part of the table, and it says the SSC recommends including Monroe County, and we've established that, and that's been done. Then it says the SSC originally recommended the years 1994 to 1997 as the reference period for dolphin, but the snapper grouper reference period years were used instead to calculate the ABC that is corrected here. I am not -- The wording is confusing on that, and so the SSC recommendation from October used 1994 to 1997, but that's not what is written in the notes.

DR. ERRIGO: That, I think, is not -- It's confusing.

DR. SCHUELLER: Yes, the whole thing is confusing.

DR. ERRIGO: What was corrected was the original ABC recommendation -- Not recommendation, but, originally, 1999 to 2007, to 2008, was used, when it should have been 1994 to 1997, which was recommended, and so that was the correction.

DR. SCHUELLER: Maybe it's just late in the day, but, when we first talked about this -- What you're saying is the 1999 to 2008 was used when we first looked at it, and we changed it to 1994 to 1997 at the meeting?

DR. ERRIGO: No, 1994 to 1997 was originally decided for dolphin, and then, after the original meeting to do ABCs in 2011, there was some confusion, and the years 1999 to 2008 were used instead, because that's what was used for all the snapper grouper species, and so then, this time around, that was corrected to use, again, the original 1994 to 1997 years.

DR. SCHUELLER: Okay, and so the notes are just confusing, but we also made the recommendation of adding the ORCS method to the FMP, and so, basically, what's happening here is the council is saying you made this recommendation, and can you just use that to give us the ABC right now, instead of them moving forward with putting it into the FMP.

DR. ERRIGO: They still plan on putting it into the FMP. The problem is, with everything that's going on, that amendment, that plan amendment, is going to take quite some time to get through.

DR. SCHUELLER: Right, and so they just want us to go ahead and use the method, even though it's not in the FMP?

DR. ERRIGO: That's right, until they can get it into the FMP, yes.

DR. SCHUELLER: The question here really is, is the group willing to move to the ORCS method, as requested by the council, and does the science support that?

DR. SEDBERRY: Well put. I do not know enough to answer that question, and we have several hands raised.

DR. REICHERT: I think Amy made a lot of the points that I made, and I was looking at that report of ours, and so, if we recommended using -- In October using the ORCS, and we are now moving away from that, I think -- Someone else mentioned it also, but I think it's important that we justify why we are revisiting that decision, and we also recommended, in that October report, to revisit the ORCS method, and then in the background of all of this is the development of the new ABC control rule, but that's new, and we can't use that until that process is all done.

My concern with, if we use the ORCS, is it is higher than any of the previous landings, and I am very comfortable with the fact that I don't think this stock is in trouble, but I am a little uncomfortable setting that at -- Using the straight ORCS and setting it at that level. Anyway, some of the other arguments Amy made already, and so thanks.

DR. SEDBERRY: Thank you, Marcel.

DR. CROSSON: In terms of the rationale, I think we don't have to make this too complex. I think we can go along with what Wilson and Marcel said, which is that we're uncomfortable with -- Applying the ABC portion of the control rule -- I'm sorry. Not the ABC. It's late in the day. Applying the ORCS method from the control rule is producing a number that is so far outside the normal historical landings series that we're not truly comfortable with that, and, also, given what we know about the biological nature of this species -- I mean, looking at the CPUE index that Mike

Errigo generated, what that indicates to me is, again, what I already know, which is that this stock ranges all over the place, and it's not really what we -- Based off the landings we have here, historically, it really hasn't affected the biological viability of the stock in any one way.

If the landings were far below MSY, and the stock was actually enclosed in this area, then I would expect the CPUE to go up, and it has, and it's just -- It's all over the place, and so I guess, if we're wanting a number that's not going to be too constraining, but still somewhat realistic, given the time series, then maybe go from -- Expand it so that we're using the third-highest from the 1994 to 2007, and that's a really long time series, and it's still before the great recession, and that comes up to -- In my calculations, the third-highest would come up with a number that's twenty-four-and-a-half million, and I think that's something that I could live with, and it still seems that it's not particularly constraining.

I don't want to punish any portion of the fishery, either the recreational or the commercial, but I just want to make sure that we're getting a number that feels realistic, given what we know about the species, given what we know about the stock, and given what we know about the historical landings.

DR. CROSSON: Thanks, Scott.

MR. GRIMES: I was just going to comment that I think -- I don't see anything incongruous about your prior recommendation to add ORCS to the FMP, to the ABC control rule and the FMP, and then deciding not to use it in any particular instance. I mean, you're adding it as an option there, and it doesn't mean that you have to use it every time. I guess, as I understand it, it's in the snapper grouper control rule, and you want to have the full suite of options, but clearly it's not in your dolphin wahoo control rule now, yet you're talking about applying it, and so just putting it in there doesn't mean that you're going to be bound to it, and I interpreted your suggestion before just to be that you are adding ORCS -- You recommended adding the ORCS as an alternative under the dolphin wahoo control rule, but I think you would still have the other approaches, presumably, that we have in snapper grouper. Thank you.

DR. SEDBERRY: Thank you.

DR. SERCHUK: This is a point that I raised before on -- I understand the logic here that the value is too high, but we don't want to constrain the fishery, but I made the same point on spadefish, that that value coming out of the ORCS was considerably higher than any of the landings we've seen before, and we didn't even discuss that, and so I'm thinking that let's make sure that we're consistent in our approach to these sort of things, and that's all I'm saying, because we're now changing numbers that we provided in October, and I am concerned now that we're sort of changing the basis for our advice sometimes without having a firm basis for doing it.

Also, while I have the mic, I'm finding difficulties in comparing the ABC values that we provided as revised in October, and, in Table 1 on page 9, for example, for spadefish, it says the revised ABC for spadefish is 1,052,663, and then, when I look at the ORCS stats that are given, I don't find that number in there at all. I find a very different number in the tables that we presented here, and so I'm not trying to be critical, but I can't cross-walk some of the information that was presented in Table 1 as the ABC value for the MRIP data at that time with the old values that are provided in the tables that we have now, and so I'm a little bit concerned. Thank you.

DR. SEDBERRY: Thank you, Fred.

DR. BUCKEL: I'm glad that Fred mentioned that. I wanted to bring up the spadefish and also the jacks, and so just this consistency, and we do have ABCs that are over the highest landings ever recorded for other groups, and so, just because ORCS did that here, it doesn't mean that we should throw it out, unless we have some other argument, because, as Fred mentioned, we just want to stay consistent across species and groups.

DR. SEDBERRY: Thanks, Jeff. From what I'm hearing, it seems like we're leaning towards recommending ORCS for either 1994 through 2007 or 1999 through 2007, and is this what the committee is thinking?

DR. ERRIGO: I just wanted to say, real quick, the same thing I said about spadefish -- The job of ORCS, the way it was developed, what it was developed to do, was set a limit above all the variability in the landings for species that you're not worried about overfishing occurring or it becoming overfished, and so it's actually doing its job here, but that's all I wanted to really say, and it's also doing its job for spadefish and the jacks, and that's what it was meant to do. That's all.

DR. SEDBERRY: Thanks, Mike.

DR. SERCHUK: Can I make a comment, Chairman, please?

DR. SEDBERRY: Yes, and then I have Wilson.

DR. SERCHUK: When I look at the ORCS table that you've given for spadefish, it says that the ABC, based on either one of the numbers that are presented there, and I don't know whether they are switched, is 1.9 million or 1.3 million. When I look at the bar jack table -- Excuse me. When I look at the table for the bar jack. There are numbers that are given -- Can we go to the spadefish table? I am sorry to confuse you. We have numbers that say --

DR. ERRIGO: Do you want spadefish or bar jack?

DR. SERCHUK: Bar jack. I am going to show you my source of confusion, even though it looks like I am confused. If we go to the ABC value at the bottom of that ORCS table, what was the old value, Mike?

DR. ERRIGO: I think these are reversed. Let me just see. Yes.

DR. SERCHUK: Okay, and so let's say the old value is 84,871. When I look at Table 1 on page 9, it says the revised value that we gave in October was 42,919, and I can't follow it.

DR. ERRIGO: 42,9109 was the third-highest and not the ORCS value.

DR. SERCHUK: It says ORCS in the table. Table 1 says ORCS.

DR. ERRIGO: That was a mistake then, and I don't know why, because I have 42,919 right here.

DR. SERCHUK: I understand that, but that's why I'm saying it's very difficult for me to walk through some of these things when the ABC basis as ORCS -- That's what the information is that we provided, and now we discover that it wasn't the ORCS, and I'm not blaming anyone, but you can see that it's leading to some inconsistencies for people that are trying to compare our report in October to what we're doing now, and I'm not castigating anyone. Thank you.

DR. COLLIER: Fred, I was looking at that same table in the report that was given to the council at their December meeting, and, on page 9 in Table 1, it's saying 84,871 is the revised ABC value for bar jack.

DR. SCHUELLER: Can you say that again, Chip?

DR. COLLIER: The report that was given to the council at December 2019, your SSC report, Table 1 on page 9, it indicates the ABC value for bar jack is 84,871, and that matches the old that Mike has listed on -- It's Cell 041 that is displayed on the page right now.

DR. SERCHUK: Okay, that's fine, Chip, but our final report from November 13 of the SSC meeting shows different numbers, and that's all.

DR. ERRIGO: I think there was a revised report that was put out. That might be why.

DR. SCHUELLER: Spadefish has the same exact issue, and so the report from our October meeting last year shows 1,052,663 as the ABC that we put forward, and it's in that purple column that is in the spreadsheet we have this time, but it's not in the table, and I don't know.

DR. ERRIGO: I don't know what happened.

DR. SCHUELLER: Why was a revised report put out, I guess, and why didn't -- Did we know about it?

DR. ERRIGO: I don't remember if there was a revised report. I am just saying there might have been a revised report put out if it was different in one than another.

DR. COLLIER: There is a revised -- It is saying revised report, and the date on it is November 19.

DR. SEDBERRY: Okay. That explains something, yes.

DR. LANEY: Mr. Chairman, I was just going to ask a question and say, relative to the fact that we appear to have been inconsistent with respect to spadefish and bar jacks and dolphin wahoo, in that we were recommending ABCs that were above the highest time series values for those other two species, then, as opposed to your statement that consensus appears to be to recommend ORCS for dolphin wahoo, I was just going to ask the question of as to whether or not the SSC might want to also rethink the spadefish and bar jack recommendation as well, if those are above all the values in the time series, fully understanding what Mike is saying about the intent of ORCS, but wondering how realistic it is for us to be setting values that are above all the time series values, and that's the question.

DR. BUCKEL: Just to Wilson's point, it was the jacks complex and not the bar jack. The jacks complex is where we have the ABC above any of the highest values.

DR. LANEY: Okay. Thanks, Jeff, for that correction.

DR. CROSSON: I would like the committee to stick to dolphinfish, if possible, for this right now, and, in terms of moving forward with ORCS, that was not my understanding, and I've kind of lost track of what I think the committee is favoring right now, and, if I don't have any support for the idea of using this third-highest from the 1994 to 2007 time series, given the fact that the ORCS method is spitting out a number that is so extremely high, and given the different things about the report, and is there nobody that thinks that this is a reasonable approach to making sure that we're being precautionary? We don't have the ORCS, I guess, right now in the dolphin FMP to begin with, and so, if this is something that the committee doesn't favor, then I will back down, but I just thought that this was a reasonable compromise.

DR. SEDBERRY: I thought the committee was tending that way too, until I said that, and then it started tending towards ORCS, and so I don't know -- I do not know what the pleasure of the committee is at this point.

DR. CROSSON: This is where it's hard not having anybody in the same room.

DR. SEDBERRY: Wilson, is your hand up again, or did I forget to clear it?

DR. LANEY: No, sir. My hand is up again, in response to Scott's query, and I would say that I support his recommendation as a reasonable compromise, and I thought that Anne Lange did as well, and so I think at least two other members were in concurrence with your suggestion, Scott.

DR. SEDBERRY: I was too, but then I had no sooner said that than it started going the other way, and so I'm not sure.

DR. CROSSON: We don't usually make formal motions when we're meeting, and we kind of read the room, but is that something we should be doing here, given this webinar?

DR. SEDBERRY: We have stayed away from formal motions, for a variety of reasons, and it doesn't make any difference whether we're in person or not, and I think those reasons still apply, and so I would rather not do that. I would rather come to consensus.

DR. CROSSON: Okay. Then does anybody disagree with my proposal?

DR. REICHERT: This may be a question for Chip or Mike. I am not sure if we can. Isn't that part of the council approved way of how the SSC operates? What we can do is make a decision, and, if members of the SSC disagree, they can write a minority report, and I think that's the procedure, and so I don't think we can make motions, but I would like to hear from Mike or Chip about that.

DR. ERRIGO: I think that is correct. Your standard operating procedures are such that you don't make motions. I don't think you can just suddenly throw in a motion like that. It needs to be discussed and decided upon.

DR. SEDBERRY: Scott has the question up there. Does anybody object to using the third-highest, with I believe the timeframe of 1999 to 2007?

DR. CROSSON: No, 1994 to 2007.

DR. SEDBERRY: Okay. 1994 to 2007.

DR. SCHUELLER: That was going to be my question, and so, in the notes that were taken, it said 1999, and I thought I heard 1994, and so it seems like the proposal on the table is 1994 to 2007, and I don't have a problem with that. I will leave it at that.

DR. SEDBERRY: Thank you.

DR. BUCKEL: Mike, could you scroll over to your catch per unit effort index? I am fine with going with a longer time series than the 1999 to 2007, but I was just curious how you picked 2004. I know 2007 is before the recession, and so that's a good endpoint, but what about the first year? Would 1989 or 1994 -- I am just curious what your justification for 1994 was.

DR. CROSSON: Nothing, other than that's what we had originally started with, 1994 to 1997, and so I was just trying to pick a very long time series. I mean, I guess, theoretically, you could go back that far. I don't have any strong objection. I just thought there was a big enough time series that -- I just thought 1994 to 2007 is a pretty good chunk of time to be using, and I don't have anything quantitatively that I can tell you the reason that we would only start with 1994.

DR. BUCKEL: Mike, I think you said before you felt the data before 1989 were a little suspect, and so --

DR. ERRIGO: Before 1991.

DR. BUCKEL: Before 1991, okay.

DR. ERRIGO: There were some issues that I had to gap fill and be different kinds of things, but I gap filled for the CPUE, but I don't know what effect it has on the landings, and so I might avoid those years, if you're going to calculate the ABC.

DR. BUCKEL: Okay. So then maybe starting with 1991, and I'm okay with either one, 1991 or 1994, but I'm just trying to think of one justification for 1991, so we could say, okay, for the reasons you mentioned, the data are consistent from 1991 onward, or there's no issues, and then you see that the catch per unit effort is flat during that time period, and so we feel like we can deviate from what we used for the snapper grouper, the 1999 to 2007, and not use the ORCS, because it just gave a very high value. If it was a motion that Scott put out, I guess an amendment might be to consider 1991 as the beginning year.

DR. CROSSON: Let me just add something really quickly, and that's, if you go back too far, we don't have logbooks on the commercial side, and so I'm not sure you can go back as far as you can with the recreational data series on the commercial side.

DR. ERRIGO: You can go back to 1991. You would be fine going back to 1991.

DR. BUCKEL: It might not change the value, looking at that.

MS. LANGE: I was looking at the 1999 to 2007, because those are the years that were used for the other stocks. This was originally from 1994 to 1997, which was too short, and so I can see either doing the 1997 to 2007, which has consistency with the other stocks, or, as Scott had originally suggesting, starting at 1994, because that was already included in this stock, in going to 2007 to a reasonable number of years in.

I am not sure, and I think that's a straightforward explanation, but I'm not sure how we start going back further and further, when we didn't do that for the other stocks. To me, that's a little more than just realizing we had too short of a time series, unless it can be fully documented on exactly why a particular year was chosen over the ones that have been used in the past.

DR. SEDBERRY: That's a good point, Anne. Consistency is always good, although dolphinfish is quite different than snapper grouper, but, still, I think, unless there is some really compelling reason to go back to 1994, I don't know why we would.

MS. LANGE: Well, again, 1994 was included in the dolphin numbers originally, and so that's -- To me, 1999 to 2007 is consistent with the other stocks. 1994 to 2007 incorporates the years that were already used, but, going earlier than that, I'm not sure there is a justification for it.

DR. SEDBERRY: That's what I meant to say. Thanks.

DR. BUCKEL: The only justification is that Mike has put together that time series of catch per unit effort, but he mentioned that he wasn't comfortable necessarily moving forward with that, and so that's a good argument for sticking with the 1994, and I'm fine with that.

DR. SEDBERRY: Okay, and so are we homing in on a consensus for using the third-highest for the years 1994 to 2007? Is there any objection to that?

DR. REICHERT: I have no objection, but I think we need to make sure that we add some language to the report to justify why we did not feel comfortable with using ORCS, since that was one of our earlier decisions, or recommendations, and I think just saying that we felt it was too high may not be sufficient, and so we need to come up with some language that is distilled from what we discussed to justify that decision, and we don't need to do that now, but I just would like to put a placeholder in there that we make sure that we add that to the report, so we justify the decision.

DR. SEDBERRY: I think you're right, since we previously --

DR. REICHERT: I think that various people have mentioned the reasons why we as an SSC were uncomfortable with that, but I just want to make sure that that's reflected in the report.

DR. SEDBERRY: Okay, and we can -- When we recap for today, maybe we can look at that part of Mike's notes and at least put a placeholder in there or some specific wording, if we can come up with it easily.

DR. REICHERT: I'm fine with that, or later, when we review the report, to make sure that we're comfortable with the language.

DR. SEDBERRY: The statement now reads: SSC Consensus: The SSC concurs with using the third-highest landings from the years 1994 to 2007 as the ABC for dolphin. I haven't heard any objection to that.

DR. SCHUELLER: Not to just drag this out forever, and I don't necessarily have an objection to it, but I do not have a clear understanding of why people are against the ORCS, other than the sort of bland statement of the value is higher than anything we've seen in the time series. I would be very concerned if the value was higher than anything we've seen in the time series if it was a species that we thought that the life history or something suggested that we would be concerned if we fished at that level, but what we've said is this is a fast-growing, quick-reproducing species that is basically a giant open population along much of the coast, even outside of our management boundaries, and, if that's the case, that doesn't align with that sort of misgiving about the value for ORCS, and so I'm happy to do either one of them, but I don't think that there's a clear outline for why we're choosing either of them. It seems like gut feeling, based on I don't know. I really don't understand, I guess.

DR. SEDBERRY: I'm kind of in the same boat, and I am just imagining the Chair, who will not be me, going to the June council meeting and telling them what we decided and why we backed down on ORCS and having only the reason being that we thought the ABC was too high relative to the landings, and I don't know what too high means.

DR. ERRIGO: George, relative to this, the ORCS ABC that I calculated uses a council risk factor that wasn't in the snapper grouper FMP, because there were no species that were moderately low, and so I extrapolated it based on the low and the moderate categories, but the council can decide to not use the 0.85 as the risk factor and instead use something else, or they can set the ACL lower, and so this is just what it would be if they used this risk tolerance factor. I just wanted to make sure that everyone is clear on that.

DR. SEDBERRY: So then there's even a less-compelling reason to not use ORCS, to me.

DR. SERCHUK: Just a small point, Chair, in our provisional statement about what the SSC is advising. I don't like the word "concur", because we're not concurring with anything. We are recommending, and so, whatever we decide, I think we should use the word "recommend", Chairman. Thank you.

DR. SEDBERRY: Thank you. Yes, we will wordsmith all of this, but thank you for that recommendation. That makes sense.

DR. NESSLAGE: I just wanted to clarify that, when I was talking earlier about ORCS, I was just noting that it's higher than the highest landings observed, and I wasn't necessarily arguing that was a bad point. Honestly, I would be fine with the ORCS approach. Also, if folks feel that's not

conservative enough, I'm fine with what's on the table as well, but I guess I'm trying to -- I am frantically going back through our minutes, because I vaguely remember, at the October meeting, that we had a long chat about how ORCS doesn't perform well in the MSEs that have been done and that we didn't like the way that our ORCS were set up, and we were having some angst about applying ORCS, period, and that we really wanted to take a look at revising it, and am I crazy in remembering those discussions?

Like I said, I am frantically going through the minutes, but, if that's the case, I think -- I'm looking at some of the discussions that it comes up, this uncertainty in applying the ORCS methodology, and I think folks were backing away from ORCS in general when it came up on the table for this earlier for these species, and so I think that's factored into our thinking then, and I know it factors into my thinking now, although, in my opinion, as long as there is an actual accountability measure that is useful, and I applaud the council for moving in a direction where they would be changing it to something that would be not reliant on an assessment, then I would be fine with ORCS, personally, by that's my two-cents.

DR. SEDBERRY: Thanks, Genny. I thought Shepherd had his hand raised, but maybe it went down.

MR. GRIMES: I did have my hand raised, but Genny covered everything. I was just going to point out that if you look -- I think it starts on about page 23 of your minutes that are in your briefing materials, and that's when this management strategy evaluation and the poor performance of ORCS was raised, but she covered it well. Thank you.

DR. NESSLAGE: George, then I guess that raises the question of, if we have issues with ORCS for this species, we probably have an issue with ORCS for all of our species, and so, if we do go with the current recommendation that's on the board here, that's fine with me, but I think that part of the justification and discussion needs to be that we would really like to revisit the use of ORCS, period, as part of our recommendation back to the council, if we are allowed to do so.

DR. SEDBERRY: I think one of our recommendations from our last meeting was revisit ORCS, and that's kind of a loosey-goosey word, or phrase, but I think that's what we've said, is let's go with the third-highest, but maybe revisit ORCS at some point during the ABC control rule revision, and I think that's what we said.

DR. ERRIGO: Yes, George, you're correct. You were going to look at the ORCS methodology as the ABC control rule was developed. It's just that, with everything that's been going on and timing, that could not happen at this meeting.

DR. SEDBERRY: Right.

DR. SCHUELLER: Thank you for jogging my memory, Genny. I think that the consensus that's on the table is fine. I do think -- I agree with Genny that we need to put in the information about the ORCS and reexamining that and the issues related to its performance through the MSE, and so I just wanted to state that and get that on the table, that I'm comfortable with that, and I think that that gives us a better footing than maybe some of the things that were being said a little earlier in the day, or maybe I was just not as focused as I am right now. Thanks.

DR. SEDBERRY: Thank you. That makes sense.

DR. SERCHUK: Chairman, the exact wording that we used in our recommendation was, and we can still use the same wording, if you feel it's appropriate, was the SSC recommends review of the ORCS method and its applicability to many of these stocks whose ABC is much higher than historical weight-based catches, for example lane snapper. That was our recommendation, and I don't see anything in our discussion to say anything different.

DR. SEDBERRY: Okay. Thanks, Fred. If everyone can just look at what Mike is typing right now in the Google Doc, and, of course, it's going to need a little wordsmithing and looking up the wording from the October meeting report, but, of those two bullets under the consider the use of ORCS for dolphin and wahoo, it makes sense, and, of course, we've really only been talking about dolphin.

DR. ERRIGO: Yes, and we do also need to address wahoo separately.

DR. SEDBERRY: Yes, but I think what we have there has captured the consensus of what we have said about dolphin. Now do we want to say something different about wahoo? Mike, can you remind us if there was anything strange or different about dolphin that could make us come up with different recommendations for wahoo versus dolphin?

DR. ERRIGO: With wahoo, the issue here was that the ABC is overly restrictive for a stock that doesn't have any issues.

DR. REICHERT: We know that it doesn't have any issues based on --

DR. ERRIGO: Well, based on the fishery itself, but also the CPUE index is flat. It has a lot of variability, but there's no trend. Again, this has the same issues with data in the early part of the time series, and so, from 1991 on, it's pretty flat. It bounces around a lot over here.

DR. REICHERT: So, if we are using the arguments, we should be using the same methodology.

DR. SEDBERRY: I would think so.

DR. REICHERT: Then my only concern with this stock is what's happening in the last couple of years, where you've got this, except for the very last datapoint, an upward trend, and so I would say that we should at least mention that caution, that we should continue to monitor the trends, and, if they continue, maybe revising our recommendations.

DR. ERRIGO: This data only goes through 2017. 2018 and 2019 are back down here. This trend does not continue.

DR. REICHERT: Okay. Well, that alleviates all of my concern. Thanks.

DR. SEDBERRY: So can we go back to our consensus statement and just say dolphin and wahoo? It would be recommending the third-highest landings from the years 1994 to 2007 as the ABC for dolphin and wahoo. I don't see any hands raised, and so I'm assuming there is no objections. Go ahead, Anne.

MS. LANGE: Sorry, but what years are used here for wahoo? It looks like it says 1994 to 2003.

DR. ERRIGO: Yes, 1994 to 2003 were used.

MS. LANGE: Okay, and so that's different than what is in here.

DR. ERRIGO: Yes, but the issues are similar with dolphin. Dolphin use 1994 to 2007, because it's an issue with restrictive catch, and so extending the time series helps alleviate that somewhat.

MS. LANGE: Well, I'm just saying that, in the report, if one is using 1994 to 2007, they can't use the same years for wahoo, if you're using 1994 to 2003.

DR. ERRIGO: I'm sorry, but I didn't understand. Why can't you use the same years for dolphin and wahoo?

MS. LANGE: What I'm saying is the table that you gave, the figure that you have for wahoo, has a different time period, and so, if you're going to use the same time period, then you need to redo the figure. I guess it doesn't really matter right now, but it's just a recommendation.

DR. ERRIGO: For dolphin also, I have to recalculate the ABCs, because I didn't have one from 1994 to 2007, and so I can do that.

MS. LANGE: Right. That's my question, just making sure that whatever is in our report matches the numbers that you're actually generating.

DR. ERRIGO: Yes, I can generate those, and I can even show it to you guys, if we have some time, like tomorrow or Thursday.

DR. SERCHUK: The proposed language that you had including wahoo with dolphin would be completely consistent with the notes that we had from last year that say all the same recommendations and caveats for dolphin apply to wahoo as well, and so that would be consistent with the advice we gave last year, or in October. Thank you, Chairman.

DR. SEDBERRY: Thank you. Okay. Are we good with these ABC recommendations for dolphin wahoo? Okay. I think what I would like to do now -- We still have a few minutes left before 6:00, which I was hoping not to go any longer than 6:00, is to go through the notes that Mike took today in the Google Doc, just kind of scroll through them and look at any consensus statements and recommendations, research recommendations, other kinds of things that we all agreed on, and make sure we like what those things say. Hopefully this won't take too long. We started with the SEDAR, and then we reviewed terms of reference for three species, red snapper, which is shown on your screen right now, and --

DR. ERRIGO: George, for these, I'm going to get that language, and I can even put it in here, or I can put in the modified language.

DR. SEDBERRY: Okay, and then, that Term of Reference Number 5, we had that same kind of change for all three of them, and so that can be added in later, and we'll do like we normally do.

Tonight, the notetakers for this session can -- If they could please send their notes to Mike and copy me, and then Mike will incorporate those, as needed, to clarify and send out a version of this sometime tonight or tomorrow. Does anybody see anything right now that needs to be fixed under the terms of reference for red snapper? How about black sea bass, black sea bass and Spanish mackerel? If you see something, just speak up. It's hard for me to read this and look for hands at the same time, and so just speak up if you see something that needs to be changed, and, again, you'll have additional opportunities to do this, but I just want to make sure, while all this is still fresh in our somewhat fresh heads, that we have captured everything, all the important things, that were said today.

DR. REICHERT: Just a quick note, George. I think it would be easiest if we could just add the names to Table 1 for the report, so we have it all in one place, and I always like to refer to that, and so just as a suggestion.

DR. SEDBERRY: For the SEDAR participants?

DR. REICHERT: Yes, exactly.

DR. SEDBERRY: Yes, we'll make up a summary table for that.

DR. ERRIGO: Sure. No problem.

DR. SEDBERRY: Scrolling on down, did we miss the -- Never mind. I'm confused.

DR. REICHERT: We all are at this time of the day.

DR. SEDBERRY: So we have action items dealing snapper grouper and with dolphin wahoo, again incorporating the new weight estimates into ABCs and adapting those as the best scientific information available. We have a couple of consensus statements there, which is always good, and then a recommendation. Again, that needs some wordsmithing, taking some wording from previous reports, but Mike will be working on that.

DR. SERCHUK: Just in the SSC consensus, we don't recommend updating our ABC. We update our ABC recommendations based on the new weight estimation procedure, and that's just a small wording change there. That's all. Thank you.

DR. SEDBERRY: It's under the snapper grouper part, and the SSC --

DR. SERCHUK: Has updated their ABC recommendations.

DR. SEDBERRY: Yes, that's right. Then I guess we need to make sure that those weight and ABC recommendations apply to both the snapper grouper and dolphin wahoo, because, for the dolphin wahoo, we talked about the approach, or not only the approach, but also using the new recommendation on the approach using the new and approved weight estimates, and I guess the existing wording covers that. Never mind.

DR. REICHERT: No, but I think that's a good point, to make sure that we all understand that, for dolphin and wahoo, we basically made two recommendations, one to update it and then the second one to change our methodology to determine ABC.

DR. SEDBERRY: That's what I was trying to say, or to make sure that it's clear, yes.

DR. ERRIGO: Here's what the original wording looks like. It's review new landings time series for all unassessed stocks. Consider updating the previous ABC recommendations based on these new landings.

DR. REICHERT: So that refers to that --

DR. ERRIGO: That's snapper grouper and dolphin and wahoo. Then, down here, dolphin and wahoo, in particular, we change the time series.

DR. REICHERT: So it is in there.

DR. ERRIGO: Yes.

DR. SEDBERRY: Okay.

DR. REICHERT: I just want to make sure that, in the final report, we do add considerable language as to our justification for the consensus, but we've already talked about that.

DR. SEDBERRY: Yes, and I'm sure the notetakers have captured that in their notes, and we will add that in. Is there anything else we need to cover today? I think we are on schedule, according to the agenda, and we don't need to take public comment at the end of the day. We'll take it at the end of the meeting, which will be towards the end of the last, final webinar, in addition to at the beginning of each agenda item tomorrow and Thursday.

I forgot to mention, when we started earlier this afternoon, is we are doing this by webinar, and we're doing this by webinar because of the pandemic that we're all facing, and I meant to say something to all of you about facing that and dealing with this at the same time and how much I appreciate you all being able to join us for this meeting, in spite of the total disruption of your lives otherwise, and it was very great of you to be able to join us to have this SSC meeting, so that scientifically-based fisheries management can go on, in spite of the rest of the world, and so I really appreciate you all being able to do this today and for the next two days, and so thank you very much. Does anybody have anything else to say before we recess for the day?

DR. REICHERT: Thanks, George.

DR. SEDBERRY: Thank you, Marcel, and thanks to all of you. Everybody stay healthy, and we will talk to you tomorrow. Tomorrow, we are starting at 8:30. I don't see any reason to try and start earlier, since we are on schedule, and so we will see you all tomorrow, bright and early in your pajamas, at 8:30 to talk about SEDAR 38, king mackerel.

(Whereupon, the meeting recessed on April 28, 2020.)

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APRIL 29, 2020

WEDNESDAY MORNING SESSION

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The Scientific and Statistical Committee of the South Atlantic Fishery Management Council reconvened via webinar on April 28, 2019 and was called to order by Chairman George Sedberry.

DR. SEDBERRY: Good morning, everybody. Welcome back to day two of the spring SSC meeting. again, I want to thank everybody for participating yesterday, and I thought it went particularly well, considering the circumstances, and I was a little more nervous than I usually am about the webinar, but I think everybody used it well, with raising their hands and not interrupting, and I thought the discussions went really well, and it was almost like we were in the room together, and so thanks, everybody, for your participation and cooperation.

Today, we are going to take up -- Starting today, we're going to take up several stock assessments. The first one on the agenda for today, for this morning, is SEDAR 38, the king mackerel assessment update review, and, for this agenda item, I have asked Jeff Buckel, Fred Scharf, and Alexei Sharov to take notes. There are two attachments associated with this agenda item, Attachment 8, which is the assessment report, and then Attachment 9 is the presentation that you're going to hear shortly.

The way we're going to do this is the way -- We're going to try and stick to the procedure that we normally follow in our in-person meetings, and we'll have a presentation on SEDAR 38 from Dr. Matt Lauretta from the Southeast Fisheries Science Center, and we'll allow the committee members to ask clarifying questions during and following the presentation, but just clarifying questions, and let's not get into the discussion until after public comment, and so the presentation, clarifying questions, and then public comment and then discussion and then proceeding with the action items for this agenda item.

We're asked to review the king mackerel update assessment and provide fishing level recommendations. King mackerel was last assessed in 2014, during SEDAR 38, where the stock was found to have not been overfished and not undergoing overfishing, which is still true, and so we will hear the presentation from Matt, and then, again, like I said, we'll have the opportunity for questions, clarifying questions, public comment, and then additional discussion, and so, Matt and Mike, if you all are ready, I think we can proceed with the presentation.

SEDAR 38 KING MACKEREL ASSESSMENT UPDATE REVIEW

DR. LAURETTA: Good morning, and thank you all, again, for joining. I guess I would like to start off by thanking all the data providers and the assessment team and everyone that has worked on this through the challenging times, and particularly the data providers at both state and federal levels and everyone that really rallied to make this thing come together, and I'm proud to present

the results to this group and stakeholders, and, as you will see, it's sort of a positive trend for the stock, and let's go ahead and review the weight of evidence for that, and so without further ado.

Let's just start off with the broad overview executive summary here, and so, as Mike mentioned, we last did the SEDAR 38 in 2014, with data going up to the 2012 fishing year, and we determined that the stock was not overfished and not overfishing, and so, in this update, we have added five years of data to the stock synthesis model, and that would be up to the fishing year 2017, which runs to February of 2018, and the model was peer reviewed during SEDAR 38, and we kept everything as consistent as possible, because of how much effort went in from the group, including stakeholders and all the state personnel, and everybody worked really hard, and so I tried to keep as much consistent as possible.

When we updated that, we still determined the stock to be in a good state, and that is it's not overfished, and it's not undergoing overfishing, and, essentially, that's where we left it, and, since then, we have seen that all fishery indicators, including landings, fleet CPUEs, and the scientific survey, have shown increasing trends since SEDAR 38, and so it wasn't overfished then, landings are up, relative abundance is up, and so obviously then it stays in a positive state.

Really, the estimated biomass has been trending up, beginning in 2013. When we were in SEDAR 38, we sort of saw this downturn and a period of low recruitment was estimated towards the terminal part of the time series, and I will show you sort of what has happened since then, but, essentially, the exploitation rate has remained steady since 2010, and it's been about 0.04 to 0.05 exploitation per year, and that's in numbers removed, and so about 4 to 5 percent per year since 2010, steady.

Our average unfished recruitment estimate would be 9.8 million age-zero fish, slightly higher than the SEDAR 38, and I will explain that shortly, and the equilibrium landings at the target exploitation, and the target is about 0.14 per year, and so you can see we're well below that, based on our current exploitation estimates. 18.3 million pounds is the equilibrium yield at the 9.8 million fish per year.

Then this is really what I want to emphasize to this group, because so much relies on this recruitment cycle that, when we left -- As I mentioned, SEDAR 38 saw this five-year low period recruitment, potentially some of the lowest on record, and what we've seen since then is a cycle back into a period of high recruitment occurring in 2013 to 2016. This leads to a large increase in the projected overfishing limit than what we saw in SEDAR 38, with an overfishing limit of -- These are four cohorts that were expected to be above average, based on the model estimates, just entering the fisheries within the last year or two and leading to an overfishing limit projection of thirty-four million pounds in 2021, decreasing to about twenty million pounds by 2025, as the cohort moves through the fishery.

Let's review the weight of evidence, and so now to dive into the details. I have broken this presentation down into five sections, and I don't know if it's appropriate to do a pause for clarification after each section, and they're sort of brief up until the stock status update, and so that would be the longest section, but we can do it either way. We can wait until the final presentation or break the clarifications into sections.

DR. SEDBERRY: Why don't we stop after each section for clarifying questions, while the presentation is still fresh, and I think that will work well, and it won't take too much time.

DR. LAURETTA: Okay. That sounds like it will work well, because some of this upfront stuff will be a bit of a review, and then, when we get to the data in the models, we'll probably want to focus more, and so here is some of the general life history overviews that go into the SS3 model. One, we know the -- One of the main things of why we wanted to move to SS3 in SEDAR 38 is to account for this divergent growth patterns in males versus females.

We know that females grow faster and to larger sizes than the males. A fully-grown female averages around four feet, and a fully-grown male is about thirty-eight inches, is what we're estimating in the data. They mature early in life, and they start maturing around age-two, and so fast-growing and high maturity. There is multiple stocks known, at least two, the Gulf of Mexico spawning and the South Atlantic.

They migrate southward towards the mixing zone in the fall, and they overwinter in south Florida and migrate north, and the mixing zone removals has since been reduced from where they were assessed in the past to this sort of purple zone in the future, and we split those 50/50 between the two stocks, and, again, that occurs over winter, from November through March.

The commercial fisheries accounted for about 33 percent of the total landings by biomass over the last couple of years, handlines being the predominant one, about 97 percent of the landings, mostly caught by trolling. Gillnets were historically higher, but, since more recently, about 3 percent commercial landings, and they are primarily operated by smaller vessels, and then a juvenile bycatch fishery with the shrimp trawls is estimated to be about less than 1 percent of recent biomass removal, but, again, they're the juveniles, and so even though by biomass -- Numerically, they're more considerable, especially historically.

The recreational fishery has accounted for about 67 percent of the landings in recent years, and 94 percent of that has been the charter and private fleet combined, about 1 percent headboats. 5 percent is a default assumption in the model of recreational landings, and this is tournaments, and those tournaments target the oldest, largest king mackerels, and we've used that as the one asymptotic fleet in the model, and I will go into a little more detail later. It's mostly rod-and-reel fishing, and approximately 1 percent of the recent biomass removals are dead discards, and that's, again, by biomass. Numerically, they're more considerable, because they're thought to be below the minimum size limit or related to bag limit regulations.

Now on to the modeling size of things. This is really the only slide of methods, and so I will spend maybe a little more time than we would like to on model configuration slides, but the data is configured by fishing year, and that runs March 1 to the end of February. Again, one of the major advantages of SSC 3 is how we can account for these gender-specific growth patterns and how that plays into fisheries selectivities and mortality estimates.

We had a fixed natural mortality based on the size of individuals as they grow, and that's shown in the lower right panel there as the mortality rate. It's really high at age-one and dropping fast and sort of tapering off to older ages.

DR. SEDBERRY: Matt, can I interrupt you for a second? We have a hand raised, and Churchill Grimes has a question.

DR. GRIMES: Is it okay to ask a question about stock structure now, or should I wait?

DR. LAURETTA: Go ahead and ask before I get too far into the model.

DR. GRIMES: I guess, in the mixing zone in Monroe County, you apportion the harvest to Atlantic and Gulf stocks equally, and is that right?

DR. LAURETTA: Yes, that's correct.

DR. GRIMES: There's a couple of publications, and I think even a SEDAR document from a previous assessment that says that the contribution is spatially and temporally variable in that mixing zone, and so I guess my question is maybe the harvest down there is small enough that, when it's apportioned into two stocks, it doesn't make much difference in the assessment, and so I guess I'm asking the question. Is it not trying to incorporate that temporal and spatial variability in the presence of the fishing in the winter mixing zone, and is that an issue?

DR. LAURETTA: That's a very good question. Certainly stock structure and mixing remain a major uncertainty for the assessment, and our estimates of uncertainty will not account for what you just mentioned, and that is definitely something to keep note of through this process. The SEDAR 38 team, which I was a part of, of updating the previous VPAs and then helping with the Stock Synthesis setup and building, but the data providers, or the data team, and especially at the assessment workshop, we spent a lot of time trying to understand the dynamics of the two stocks and the data at hand, to provide stock structure and mixing.

The group came up with a pretty slick way of tracking the catch rates of the fleets as they moved across sort of down the Gulf bend and down Florida and along the Atlantic coast of Florida, until they sort of converged in their catch rates, and so that was the main driver to change the mixing zone, is we saw this fleet dynamic that was tracking them from the North Atlantic down into the coast of Florida and on the other side, in the Gulf, too.

I think that's all pretty well documented, but, at the end of the day, you're absolutely right that the previous iterations of the assessments, going back to 16 and before, made various assumptions about the size of the mixing zone, based on the conventional tagging data, et cetera, and it was a huge player, and most of the landings sort of fell into that mixing period zone, and, when you allocated them, you got sort of -- If you split it 50/50, you got stocks that looked equal in productivity, but all that changed when we sort of did the synthesis during SEDAR 38 of mixing.

We reduced it down to that period, where it appeared the fleets were converging on the landings, and that really reduced the amount of mixing zone landings uncertainty, where now it became a smaller fraction, and I want to say around 10 percent of the Atlantic removals, where as it was well over 50 to 60 percent before when it was allocated, and so it's certainly not -- Given our current assumptions, it's not the big issue it once was, but we still need work on validating stock structure and mixing, and there's no doubt about it, that that uncertainty is not integrated into the assessment. Does that answer your question?

DR. GRIMES: Yes. Thank you, Matt.

DR. LAURETTA: You're welcome. Okay. The recruitment assumption was determined at the review panel, and we had a really great team of expert reviewers, and, looking at the relationship between recruitment and the stock, there was no clear pattern, and so the advice was to fix steepness at 0.99, and that assumption is retained in this model, and you can see that, and I don't think it's an unreasonable assumption at all, given what we're seeing in the recent time period, and I will explain that later.

Anyway, we started in 1900 in an unfished state, with the assumption, and it ended again in fishing year 2017, which runs up to February of 2018, and so it's about two years past from where we are today. The fleet structures were two commercial fleets, the handline and the gillnet, and the shrimp bycatch, juvenile bycatch, fleet, assumed to be all age-zero fish, recreational headboats, the charter and private fishery combined, assumed to have similar selectivities, and then a recreational tournament fleet.

All the fleets except the tournaments were assumed to have domed selectivity, and that's pretty obvious in the length compositions coming in from the fleet monitoring. The tournament was the exception, and it's asymptotic, as it catches the largest, biggest fish, and we think that those fish are fully vulnerable, was the decision of the SEDAR 38 team.

The indices of relative abundance used in the model are the commercial handline, trolling only logbooks, the recreational headboat index, and then a SEAMAP trawl juvenile survey. The shrimp bycatch is an effort-driven model, and so it sort of just uses the estimates of effort to determine how many age-zeroes are being caught each year, and the regulation-based size limits were used on retention functions within the model.

The stock benchmark was defined as a spawning potential ratio of 30 percent of unfished production, and the equivalent mortality rate was exploitation rate by numerical abundance that achieves that SPR 30 equilibrium, and then, over here, I will just mention the upper-right are sort of the timeframes of the data inputs, and so you can see we have catches from the commercial handline going all the way back to the 1900s, and then you can see the various fleets entering in the catches, up to the recent tournament entry, and the indices of abundance span, et cetera, moving forward. Okay. Is there any further questions on sort of that general overview of model assumptions and stock?

DR. LANEY: My question is with respect to the shrimp bycatch, and does that include offshore and inshore bycatch, and I ask that from the perspective of thinking -- My perception is that it would be mostly offshore, because my perception also is that king mackerel is not particularly considered an estuarine-dependent species, and so is it mostly offshore shrimp effort that is capturing those age-zero king mackerel?

DR. LAURETTA: That is an excellent question. I am trying to think back to the 2014 here, and I don't have a clear answer for that. I believe that -- The team did a lot of work on that, and overlapping -- I would have to actually go back to the shrimp bycatch report to get you a straight answer on that, and I'm sorry that I don't have a clear one. I know that the team that does the shrimp estimation has been doing it for a while, and I think they have a good grasp on it, but I don't actually know the spatial distribution of the shrimp effort, to tell you the truth.

DR. LANEY: Thanks, Matt, and Marcel may have a sense for how estuarine dependent it is, but I guess SEAMAP samples offshore as well, and so I may look to Steve Poland, and I may ask Kevin Brown if they caught any in their bycatch studies in Pamlico Sound, and maybe that would be a good place to look.

DR. LAURETTA: Actually, now that you bring it up, I have some updated shrimp bycatch reports from John Walter and Jeff Isely that I will post to the -- I will post those to the SEDAR 38 site, so that we have that information. I didn't take note of spatial distribution, in all honesty, as I was looking through the shrimp estimates, but maybe it's in the report, and I will post the updated report, so that we can review that information.

DR. LANEY: Okay. Thanks, Matt. That would be very helpful. Thank you, Mr. Chairman.

DR. SEDBERRY: Thanks for the question, Wilson, and thanks, Matt, for that discussion. I was just thinking that there might be some information in the SEAMAP data as well. At least in some years, they had two depth strata closer to the beach and farther offshore, and there may be some information in there that indicates some depth distribution of young-of-the-year king mackerel.

DR. LAURETTA: When we get into this data section here, we'll pause for a minute on the discard slide, and I will show you that it was more of an issue historically, and where the shrimp effort has been lately is not such a -- It's not really having much impact on the model at all, but we'll stop and pause there, and I will highlight that when we get to the discard slide, and how's that?

DR. SEDBERRY: That sounds good, and, before we move on, Yan Li has a question.

DR. LI: Thank you, Matt. I have two clarifying questions. One is, in the model, you treated the Gulf of Mexico and the South Atlantic, those two stocks, as one stock, one single stock, in the model, right?

DR. LAURETTA: No, I'm sorry, and we have two stock assessments. Michael Schirripa did the Gulf of Mexico, and the only overlap is that purple mixing zone that I showed a few slides back during the November through March.

DR. LI: I see. I was wondering if -- Thank you for clarifying.

DR. LAURETTA: You're welcome. I know, in some of the historic SEDARs, they did mix stock models, and we just didn't think that we had the information to do any better than splitting it 50/50 in SEDAR 38. A lot of these decisions were a lot of discussions amongst really knowledgeable people, both scientists and stakeholders, about the fishery, and so I tried to uphold as many of those as I could up until -- There were some things I had to change, but I was part of the team, and I know how much effort went into defining especially things like stakeholder input on historic landings, where we didn't have good information and we had to rely on expert opinion, and so I left as much as possible from that, but the real documentation will come in that main data workshop report from SEDAR 38, which is also on the same link here. Any other questions then on sort of the general overview?

DR. LI: Matt, I had one more question. Can you scroll back one slide and show the data? I noticed that, for the catch data, the longest time series can go back to 1900, and then, for the abundance data, the longest time series of abundance indices goes back to around 1930 something, and then most of the abundance index data is kind of short, kind of only like one-third of the whole time series in the model, and how do you think this will affect the model outcomes, especially the indices of abundance, total abundance, biomass?

DR. LAURETTA: The long series back is actually shrimp effort, the light green you see there in the indices, and so it's an effort series. You're absolutely right that we don't have a long time series of abundance here. Essentially, it starts with the headboat around 1980. What that means is, prior to 1980, we don't have length comps or abundance indices. Really, you see all the data starts up when fisheries started being monitored.

Those are the periods that we can start estimating recruitment deviations. Everything sort of before that is like an equilibrium removal type model, and you will see that clearly in the recruitment estimates and the historic time series, and so it's really more about giving us a starting point reference than it is do we believe the smooth trajectory of biomass decline that has occurred up until the data starts in and then we get our recruitment signal for what the stock has -- Sort of what we think has been more likely its reproduction signal over the last forty years, but, yes, before that, it's really an equilibrium removal, is all we're trying to do, is just say, okay, given these removals, where do we think the starting point was for this unfished state, and that will be clear in the time series estimates when I show you -- I will try and highlight it, and you can really see where the data come in, and, before that, it's not like we think it was this steady, slow reproduction, and it's just an assumption we have to make to hit our relative equilibrium benchmarks to base our advice off.

That is an issue with every stock assessment, and I don't know anybody who has got a time series of abundance going back before 1980 for these stocks, but it's just when NMFS started the data collection programs, but I can say this is the only SEDAR that I work on as part of the HMS team, and I do consider this stock to be data rich compared to a lot of the other things, but, number one, it's because we have these standardized sampling programs that we don't have in the international fisheries that at least we have consistent data streams, for the most part, given regulations, but that's where we're at, and you'll see it clearly in the estimates, that, really, where we get the data signal is where we start to estimate deviations, annual deviations, in recruitment and get more of a realistic signal on the stock.

DR. LI: Thank you, Matt, and, given this situation, would you consider like moving the start year to the later time period, when most data can cover it, and then, given this, would you say the abundance of biomass estimate from the model for the earlier years was less reliable or more reliable?

DR. LAURETTA: Are less reliable in the current year?

DR. LI: When we make decisions, we should focus on the abundance estimates in recent years instead of the whole time series, or instead of considering the early years, because of the issue you just mentioned?

DR. LAURETTA: Well, I mean, certainly, when it comes to catch advice, the most recent abundances are critical, yes, absolutely, and particularly what's happened in recent recruitment, as we think the -- We don't have a real strong juvenile targeted fishery, and so the fish enter starting around age-two, depending on the fishery, up to the tournaments around age-five or so, and so there is that lag, but, yes, certainly observing the recent recruitment pattern, to understand what the cohorts entering the fishery are going to be like, versus the ones that are sort of moving through, but I don't -- You know, I wouldn't recommend removing the early time series, because it does give us that reference point of unfished state, and we don't think there was much fishing prior to 1900 and that the stock was in a fairly unfished state, and that's where we want to base our biomass target on, is 30 percent of the unfished egg production as our lower biomass threshold.

It was definitely out of the scope for me to start messing with the timeframe of the model for this operational, but these are the kind of things that we can make recommendations for a research assessment, and I would be curious to see what happens if we started it at the period of data.

From all the other HMS stocks, we would miss the decline in biomass that we think occurred, and we sort of started out at this state, and then it might not matter for this one, with the steepness at 0.99, because we're sort of looking at an average across the time series, which the recent period falls within that range, but I will show you that there's some evidence for a lower steepness in the data, and we don't see it in the stock-recruit pattern, but those are the kind of things that I would recommend for a research track assessment, to understand better how our advice could change if we eliminate the unfished starting state assumption, but I think, as I will sort of build the argument for as we move along here towards the end, is I think we might be dealing with a potentially really useful tool for stock management, and let's go ahead, and I will make that argument to you, and we can have the discussion on it, and I will tell you why I think that. Any other questions? Did I answer your question?

DR. LI: Yes, and thank you so much, and I am just thinking that, to me, the catch data informs the fishing mortality, and the abundance index data is critical to inform the population size information, and then, because we don't have the data in the early time series, and then I am just wondering how this can inform the status, the population status, for the early fish status in the early time series, but that's just my comment. Thank you so much.

DR. LAURETTA: I will leave you with a little food for thought, and the catches themselves, the removals from the population, are really important in defining the scale, and it's actually the abundance series in the recent time that is defining the trend, and they are relative, and they don't give us any sense of scale, and none of them are an absolute abundance time series, and so they're really defining what the current trend is, and it's those catches that are helping us define what the total scale had to have been to allow for those removals over time, if that makes sense, and what the recruitment -- Essentially, what average recruitment would have had to have been to allow for the removals at that size and age, and that's really why we do it.

Then, again, the abundance indices are providing the trends for the recent time, and we're fortunate, in theory, to have a fisheries-independent scientific survey of age-zero fish in the Atlantic for mackerel, and that winds up being a really important data series, as far as giving us a first look at recruitment to the population years before they enter the fishery.

Just to sum up, for your question, absolutely the current abundance estimates will be the most important for management advice in the near future, and that is what happened in the recent past is critical for what to do with the fisheries in the near future. The rest is just sort of giving us a relative equilibrium point to scale our current status to, and obviously we don't base our -- You wouldn't base the advice on what happened forty years ago.

DR. SEDBERRY: Thanks, Matt. I just wanted to -- We still have a few more questions, or hands raised, and I just wanted to mention to the committee that there is still quite a bit of this presentation to go that might answer some of your questions, but, if you have clarifying questions on what's been presented so far, we can take those now.

DR. REICHERT: Just quick clarifying information relative to Wilson's question. For those of you unfamiliar with the trawl survey, that operates in ten to fifteen meters of water, and so relatively close to shore, and the vast majority of the king mackerel that we catch is age-zero and age-one, and so it's a very -- Offshore is, obviously, a relative term, and so that's just to understand where the SEAMAP trawl survey mostly operates.

DR. LAURETTA: Yes, very good, and I remember that we spent a lot of time looking at those effort distribution maps, and your comment just range a bell with me, and I know we discussed it, and I would look to those SEDAR 38 documents, and I'm sorry that I can't direct you to an exact one right now, and I've sort of been focused on getting this update, but we did spend a lot of time looking at the overlap between the SEAMAP scientific survey with the shrimp trawl effort. Thank you for your comment on that. We'll have to review that, and, again, I will post the updated shrimp bycatch reports, which, seeing as how they were nice enough to put together an updated report for us, and so we have a little more information that I can provide.

DR. REICHERT: Thanks.

DR. SEDBERRY: Thanks, Marcel, for that question.

DR. BUCKEL: Matt, thanks for your presentation so far, and I had a question on the natural mortality, the choice of Lorenzen, and so recent assessments in the Southeast have switched from the Lorenzen to the Charnov et al. 2013 model that relates body size to natural mortality, because the Charnov has the data that was used in the Lorenzen, but then updated data as well, and that returns different natural mortality rates than the Lorenzen, and that's been determined, I guess, to be better estimates of natural mortality, and I was just curious why the decision to go with Lorenzen versus the more recent Charnov model.

DR. LAURETTA: Again, the more recent -- Maybe, within the last five years, it's been adopted in other stock assessments, I guess, and, again, these sort of parameterization decisions occurred in the 2013 data series and seminars and a lot of mackerel experts in the group, and I'm holding the assumption, but, you know, Lorenzen was a very biologically plausible scenario, much more than sort of a constant rate that I think had been used prior, and so we wanted to account for the fact that these fish grow fast, to large sizes, and become predatory, whereas a constant mortality rate at-age didn't really seem biologically plausible, and so, at the time, it was the better alternative, and certainly this would be a great research recommendation to look at, if there's a weight of evidence for other stocks in the Atlantic, to move towards a better alternative model, and that should be put on the table for a research assessment, for sure, and we can look at the effects it has,

but, in this case -- Again, my goal was to keep as much of the decisions from SEDAR 38, where a lot of thought and discussion went into them, as to hold them as constant for the update and really focus on replication of the data methods, to where I could, and I will show you.

There were some cases where I couldn't, where it was clearly this method is not supported anymore, and we have to revise this series, but, where possible, where there was justification for maintaining it, I wanted to keep things consistent, to really give us a message of where the stock has gone, and that really was the objective. The objective was an operational assessment, and so, in order to avoid the mission creep, I didn't look at alternative mortality schedules, fecundity schedules, any of that. Really, the goal was to let's update the data series and see what has happened to the population trends.

DR. BUCKEL: Great. Thank you.

DR. LAURETTA: Any other questions on Section 1?

DR. SEDBERRY: Thanks, Matt. I don't see any additional questions, and you can move on, and I really appreciate those research recommendations, and I want to make sure that the notetakers are capturing those.

DR. LAURETTA: Excellent. That was my comment, if the notetakers could jot down some of these comments, where they would be very appropriate to look at in a research recommendation, and let's make sure they get written down, if that's possible. Thank you.

DR. SEDBERRY: Thank you. We can move forward now.

DR. LAURETTA: All right. Data. I love data, and hopefully -- That's what sort of drives all of it, and so we'll take some time looking at it. As we run through some of these sort of chart-intensive figures, I first want to go over the charts a little bit, just to make sure that everybody is able to interpret them, and then I'm going to go through like a series of small bullet points or notes that I had from my observations, and so I will try and keep that framework as we move through the talk, because there is a lot to absorb here.

What we're looking at on the right two graphs are the commercial landings on top, and they are in yield in millions of pounds, and the recreational landings below, and they're in catch in millions of fish, and we can see the breakdown by fleets in each one, and it's gillnet and handline, and you can see it's mostly handline in the very -- Over the last several decades, and, on the recreational side, it's mostly the charter and private, and, to a much lesser extent, headboat and tournaments.

I have highlighted the two assessment time periods from SEDAR on here, so we can really track where we were, especially on our last assessment on SEDAR 38, and you can see that, between SEDAR 16 and SEDAR 38, we saw a very steep decline in the landings, and that drew concern from I think a lot of stakeholders and managers, and so a lot of our focus at the time then was on what has occurred to cause this decline in landings, and we see that, since that time, since the SEDAR 38, we've seen a pretty steady rebound, and so, when I say landings increased since SEDAR 38, it's true for both of the major fleets. In the charter, it's clear. During that SEDAR steep decline, it was actually the lowest catches since the fishery development period in the 1950s, and then we see the increase. That's the basic catch inputs.

Now this is the discards, and this is where I wanted to highlight a little bit, and so let's start with the bottom left figure here. We see these are all discards in thousands of fish, and so up to 400,000 fish are being discarded each year, and so the numbers, in numerical abundance, are not insignificant at all. When it comes to biomass, it's about 1 percent of the removals, compared to actually targeted landings, because, obviously, the fish are bigger when they are landed.

Here, we see that the early period of landings was mostly juveniles discarded by the shrimp fishery in the early period, and that, more recently, it's gone to the recreational, charter, and private discards, and we think this is directly related to the size and bag limit regulations.

Then, if we look at those two figures on the right, these are sort of the SS outputs, which does its internal calculations of size structure and numbers or weight, and it gives you sort of a scale on metric tons, and so, when we compare them, what we see is the bottom one is what's been discarded dead, and, again, this is an assumption of about 20 to 22 percent discard mortality across the fleets, and what's discarded dead is about 300 metric tons, compared to the total landed is about 600, and so about twenty-times more retained yield than is being discarded dead into the system, but, again, numerically, it may be more important, because it is age-zero fish, but they're also the most abundant.

The indices of abundance, and so here we're looking at four indices, and three of them are used in the assessment, but I went ahead and updated the charter and private so that we could get a look at it for inference, even though it's not being modeled in SS3. The upper-left is the commercial logbook, and the red line presents the index from the previous SEDAR 38 assessment, and the black line presents the updated data, up through fishing year 2017.

As we kind of go across the fleet, we see commercial logbook was sort of trending down during the last assessment, and it since has bounced back up to about its domed average there, and the SEAMAP trawl was at that lull period on the upper right there, and we see that we came into it with the lowest three observations on record, I think, and, since then, we've seen a pretty steady bounce, and then this spike in 2016, and that becomes really important as we try and interpret the model outputs later, and so definitely note that.

The recreational headboat was also at this sort of all-time low index when we ran SEDAR 38, and, since then, it's seen a really sharp rebound, drastically sharp, especially in the most recent year or two, and then, here on the chart, are private again, and they're not modeled, because of issues with regulations on retention and data of discards, and it was decided not to use that fleet, because it's undergone so much of regulations that it's hard to know when the signal shifts due to a regulation versus abundance. What we can see is it was also at its all-time low during its terminal series of SEDAR 38 there, and it has since even shown a rebound similar to the recreational headboat.

Overall, we had excellent replication of the SEDAR 38 standardization methods, and I thank everyone -- I thank Tracy and everyone that worked on the SEAMAP trawl, and it's really important, and that whole team, and I appreciate them. They did an excellent job.

All indices showed a positive trend since the last assessment, and the recruitment peak I mentioned, the shark positive turn in the headboats, and then I already mentioned that we didn't model the charter private, for the reasons that I mentioned.

Fisheries length composition, here we're looking at the six main sectors, and so, again, here I went ahead and split out charter versus private, because it was in the data anyway, and so you can see they track each other pretty well, and so that's at least validating the assumption of putting them together, and they don't seem to have drastically different length selectivities. Anyway, we're looking at the handline is the first column, and it runs down by year since SEDAR 38, and so we can sort of track the frequency distributions over time, as you move down the rows.

One thing you will notice is that the handlines are the predominant source of measured fish, and there is no doubt about it. We've got hundreds, up to thousands, of fish per year being measured by the handlines, and that becomes really important when we look at the fits to the length composition later down the line, is that, really, we've got a lot of information from the handlines, quite a bit from the charter, a couple hundred samples every year, and then the other fleets don't have as good coverage.

Anyway, one of the most notable trends -- The important thing to note is sort of this drop-off of information on the tournament coverage and that, when we go past 2013, we have no data in 2014, and we have a couple hundred measurements in 2015, and then we lose the signal in the tournaments, and so, luckily, SS3 has this integrated approach, where it sort of accounts for sample sizes in its objective function, but it is curious that there was this drop-off of the tournament fishery, and I don't know if it was a sampling issue or a fishing issue, but it would be something that I would really appreciate some feedback, especially as we go through this and into the ages and stuff, from stakeholders and participants that might know.

I have heard talk about hurricane impacts and 2016 and Matthew, I think, which would have been right in the prime North Carolina fishing season, I do believe, and so those are the kind of things that I'm trying to get -- As we interpret the data, the anecdotes of what happened on the water become really important, but, anyway, overall, my points are thousands of fish measured annually, and so we have some pretty good idea of the size composition being caught, particularly by the commercial fleet, the main commercial fleet, and all fleets are observed for most years, and so we have decent fishery coverage, for sure.

The ages appear to be a little bit of a different story. Here, we only have a couple hundred fish, and so the columns are the same. They are by fishery, and, here, I just lumped charter and private, because there aren't that many samples to begin with, and, again, we see that it's primarily the commercial handline that is being sampled for biological age and growth. The other fleets almost have no information, all together, and we see the same effect on the tournament sampling. We just don't have any age structure. I'm sorry. It's not really age structure, but we don't think that the ageing data is representative of the catch. It's actually used to inform age class by five-centimeter bins, and so conditional length at-age, because we don't have very strong coverage.

There is this definitely notable truncation in the handline ages in recent years, and so we see, if we look down the handline fishery, and we go from 2013, 2014, and 2015, catching big fish, and then, by 2017, it seems to be missing the big fish, and so I instantly looked at the distribution of sampling on this, and I noticed that, in 2016 and 2017, where there is no sampling in North Carolina that occurred, whereas, before that, we had hundreds, 500 or 600, samples of fish from the northern range, and then I think it led to this very sharp drop in mean age, and so, again, we're not using this data as age composition, because we don't think it's being sampled representative to the

catches, but it is used to inform cohort strength within a five-centimeter bin, and so it's one way we were able to deal with this, and it's not a new issue for the update, and it was something that was dealt with in SEDAR 38, but I did put out a research recommendation to maybe explore the influence the age data has.

Because I saw this sharp drop in the ages in the sampling coverage, I actually toggled off the handline ages for 2016 and 2017, because they were giving a very strong signal, and so that was a decision that just -- The truncation was so sharp that we had to account for that in the model.

Then that's another -- So this would go into that discussion on the biological sampling, and I know, when we were doing some genetic sampling, and if there's anybody out there that contributed samples to the genetic stock structure stuff, I appreciate that. We had -- I actually got reports that there was trouble sampling the North Carolina fishery, because of hurricane effects, and so I would be curious to see if that was observed by the watermen as well.

This is a graph that I think is a really neat graph to show us how mackerel grow through the system over time. I put everything in inches, so that we can interpret it properly, but, again, here on the top, we're looking at the density distribution per age class, and females are on top and males are on the bottom, and so, for example, if we follow this red dome, the size distribution of -- That's age-one, and you can see it in the upper-right key there, is about -- It's somewhere between twenty and thirty inches is an age-one fish, and so it could be anywhere from its first month at age-one to its last one, somewhere in that range, and then we see sort of the mode shift as the fish grow.

It's clear that we have this growth difference between the two genders, and we can see that females get much bigger, and not only do they grow the larger size classes faster, but they actually get much bigger overall. There isn't that much separation in the juvenile size classes. An age-one fish, male or female, is somewhere around twenty-four inches. An age-two fish is around twenty-eight inches, on average, and then they start to diverge very fast at age-three and older. Your average fully-grown female is about forty-six inches, and so just under four feet, and your average fully-grown male is only about thirty inches, and, if we look at that four-foot mark on the X-axis there, we see that actually, if you catch a fish that's greater than four feet, there is almost certainly an older female in the population, and it's really rare to actually find a male that is over forty-eight inches.

Now, another important thing to note here is that I've overlaid the asymptotic mean size for each gender that's estimated with an SS3, and this estimate didn't change in the update. It actually was spot-on, and it's estimated internally to account for fleet selectivity issues, and here we see that, overall, there is really good agreement with what SS estimates should be the average size of a fish in that plus-group, and that that's that purple distribution to the far right, about forty-six inches that appears, and we see a little bit of a skew, which could be due to a plus group effect, or potentially a tournament selectivity that's giving those, and I'm not quite sure, but, overall, the model seems to have appropriately captured the mean size of a fully-grown fish.

This becomes really important, because, for example, if that vertical line was shifted to the far right, SS would want to interpret that as having a lot of cryptic biomass in the system, and so, as long as we think we're sampling the largest fish, or the oldest fish, and getting their representative sample, SS seems to be getting it appropriately modeled, which is, again, a huge advantage of why

we wanted to move to this framework, is to get that internal estimation and growth back in SEDAR 38. Those are the data. Do we have questions or comments?

DR. SEDBERRY: We do. Steve Poland, who is the liaison between the council and the SSC, has his hand raised.

MR. POLAND: I just had a quick question about the tournament sampling. To my knowledge, we have not had any interruptions in our sampling of king mackerel tournaments, as far as providing lengths and ages, and so I just wanted to ask that -- That tournament data, is that coming from the state, or is that federal port agents, or where is that coming from, because, if there's been some miscommunication between our state and I think we send all our samples down to Pensacola, and I want to get to the bottom of that.

DR. LAURETTA: That's a really good comment, and thank you for that. You know, I would have to refer to my data providers on that one. I know that the SEAMAP program provided a lot of information going into SEDAR 38, or sorry. The FishSmart program provided a lot of information on tournaments, including the retention function, but, as far as is it a sample processing issue then, I would have to go back to our data providers on that.

I'm assuming it's provided by the state, and I don't think we have a mackerel tournament coverage as part of our routine sampling, and so it would have been data provided. I mean, at the end of the day, the predominant source of length comps is the handline fleet, with several thousand per year, and the other one is the tournaments are maybe a couple hundred fish, but, yes, I don't have a solid answer for you. I could check back and get back with this committee on that, if I could send a query to the data providers.

MR. POLAND: Okay. I appreciate that, and I will follow-up on my end with my staff, just to make sure that there's not a box sitting in a warehouse somewhere with samples and data.

DR. LAURETTA: Yes, absolutely, and, for all I know, it could have been backlog of ageing effort, and so much goes on from the field to the assessment that I try and keep track of as much as I can, but that's certainly a good question, and I did -- I was curious about was it the FishSmart program, is it hurricane, and these are exactly the questions that I have for the group, and people that maybe are a little more familiar with the conditions on the water or the fleets -- We should make a note of that. Is it a sampling effect, is it a processing effect, or is it a communication effect, and I don't have a good answer. Sorry.

DR. COLLIER: George and Matt, John Walter said he could answer that question about the fishing tournaments.

DR. SEDBERRY: Okay.

DR. WALTER: Good morning, everyone. I just wanted to comment on two things, and I can answer the tournament question, and it wasn't a drop or a decline or a lack of any sampling, but the tournament data that went in there was specifically data that was collected as part of the Mike Wilberg and Tom Miller at the University of Maryland research program on stakeholder engagement in the tournament fishery, and so they had collected specific information and data from the tournaments that was outside of the standard sampling protocols that we currently have,

and so there was no issue with our actual routine sampling, and that was just a special research project. The research project was completed and finished and was successful, but it's not an ongoing thing, and so that's why -- It was fortuitous that we had the information, but, when it's not there, it's not a drop-off in any of our routine process.

The other thing is there was a lot of questions about why certain things weren't opened up for discussion, like natural mortality, the starting time period of the model, and remember this is an update assessment, and the terms of reference were pretty strict on what should and shouldn't be opened, and I think we can get the terms of reference posted in the briefing book, and that might clarify what the assessment staff and team were really able to address in an update. Thanks, everybody.

DR. ERRIGO: I just sent the terms of reference to the SSC.

DR. SEDBERRY: Thanks, John and Mike, and I think they're also actually reviewed in the report, and so you might see them there, too. Matt, I don't see any other hands raised, and so I think you can go on to the next section.

DR. LAURETTA: Okay. Great. John, thanks for those clarifications. That's really informative. That was sort of the hunch I had, from some discussions, but that's really important, and, again, I think it highlights why SS3 was a preferred platform. The VPA could never handle something like that, which was used up until SEDAR 38, and so, again, the objective function within SS3 accounts for a lack of sampling, whereas it would just look like a complete truncation of a catch-at-age in the VPA, and so, again, this argues to support the decision made by the assessment team during the last round, where we had the door open to all of this.

Just to reiterate that I really stuck to the script on this one, everywhere I could, because, given our current times, complicating the issue was not on the table, and so what you will see is, wherever I could keep it consistent, I kept it consistent, and it was a necessity, because I wanted to deliver this product, and we all understand, at the Southeast Center, how important this stock is for the region.

Anyway, let's go ahead and dive into Section 3. I know this is a really important one, and, where I said I tried to keep everything consistent, this is where I could not. This is where the methods have been updated and the previous method is estimating recreational effort and catches, and it's not supported that the new best practices is to use this recreational fishing effort survey, which has had significant impacts on the landing estimates for a lot of the stocks fished recreationally.

I dedicated a whole section to understanding the effects that this change has on the stock assessment, and the most efficient way to do that was, rather than creating two updated time series, I asked the data providers to only give me the correct estimates, or the best-supported scientific estimates, and I was going to put the series truncated at the end of the last assessment into the old stock assessment, to understand its effect, and that would be just as informative as creating multiple time series in an update assessment, and so simplify the problem, give me the current estimates, and I'm going to put them -- All things being equal in the SEDAR 38 model, I'm going to put these estimates in and see what happens, and so that's how I was able to measure the effect of this change on the assessment itself without creating multiple iterations of an update.

Let's first look at the effect on the overall landings estimates, and so this would be retained fish plus dead discards, and we see no effect on commercial estimates, and that's good. Again, let's run through the graphs. There's handline commercial fleets on the left, recreational fleets in the panels on the right, and red would be the SEDAR 38 time series in the final base model used for advice, and then the black would be the updated time series, given all the changes, and so we see no effects on the commercial fleets, as we would expect, and then we see this increased recreational private landings plus dead discards, as well as live discard estimates, and I will show you that in the next slide.

About a 38 percent, on average, increase per year, and you can see that as the black line sort of diverges upward from where it was in SEDAR 38, and quite significantly in some years, for example the mid-2000s or whatever that is, and you can see it jumps up to about 1.2 million fish, whereas, before, it was about 800,000, and so a 50 percent increase for that year.

The headboats actually saw a decrease in dead discards, which led to a lower estimate, and then the tournament was approximated as a direct proportion of charter/private, and so its change follows that of the charter/private fleet, by default.

Let's look at the effects on live discards here, and there was no effect on the commercial or bycatch, and the differences you're seeing are actually just updated standardization model estimates, and then, on the shrimp trawl, down on the bottom there, you can see that the most current estimates from SEDAR actually had more information, and so they diverged somewhat in the last couple, two, terminal years, but, again, it's pretty small compared to their historical discards, which was hundreds of thousands of juvenile fish, now to about 100,000 or less.

The recreational charter and private, again, we saw that jump in increased efforts lead to an increased overall estimate, and it's about 500,000 fish at the peak there of the landings, and then, more recently, it's down to about 100,000, and then it's jumping back up in the recent period, and so discards have tracked a similar pattern to what we saw in that index, the MRFSS index, in that they are trending upward pretty sharply, which, being juveniles, is indicative of recruitment events, potentially, and then the recreational headboat sort of had this sort of long-term effect, but they're so small that they're really negligible in the model, actually, and you could just turn them off altogether, and it would have no bearing on the model whatsoever, because we're looking at maybe 2,000 fish discarded live, and that's really small compared to the 500,000 or so discarded live and then the millions of fish being removed each year.

Actually, the recreational headboat is sort of just a drop in the bucket, and I don't know if that's a data issue or the reality, but I guess the thought on that is there's the high boat bag limit, and so it's less likely to be discarded fish.

Now the overall effects on the stock assessment time series, and so, on the upper panel, we're looking at the recruitment estimates, going back to the start of the model, and this is the effect that I was sort of talking about, is that it's this equilibrium assumption up until we have data, and then we try and estimate recruitment deviations, which gives us a more realistic biological signal. Before that, it's all about getting a baseline assumption at equilibrium.

The bottom panel is the spawning biomass estimates, and the SEDAR 38 is shown as a black line, and you actually can't see it here, because the gray line is that decreased headboat effect, which,

as I mentioned, is pretty negligible overall. Not only are the landings small in comparison, but the discards are negligible, and then the red line is really the effect of the increased charter/private landings on the assessment, and so what we essentially see is that that increased landings leads to increased recruitment estimates, particularly in the years where they went up considerably and the recruitment estimates prior to that also showed a jump, and you can see that in the red line in the upper panel.

That actually leads to a scaling of the spawning stock biomass upward, and that is a higher -- Recent time period recruitments leads to a higher overall unfished average, and then that sort of scales everything with these two parallel biomass lines, and so mean unfished recruitment was approximately 5 percent higher with the new charter/private landings and about half a percent lower with the headboat changes, and then the benchmarks also scaled at those same proportions, and so that's really important.

While we're seeing absolute changes on the relative scale, they're actually not as significant, and everything is sort of scaled to the simple principle that, if more fish were removed from the system, more fish had to have been created, and that's what is leading to the higher level of recruitment and a subsequent increase in spawning biomass, but, notably, very importantly, the target SPR 30, the target fishing rate that is at SPR 30, did not change substantially with this effect.

Here is the tabular summary of the overall effects, and so, if we just run through the columns here, the first column there of values is what the SEDAR 38 base model outcome was. Then here we look at the two effects of the fishing effort survey changes in landings on the model, and so we see the unfished spawning biomass benchmark went up about 5 percent, I think, under the increased charter/private landings. Total biomass followed recruitment, and, of course, our target was unaffected, because that's a defined target.

The spawning biomass at the target also scaled up a bit, but then our F -- I mean, it did change a little bit, but not noticeably at the two decimals, and it's all around 0.15, somewhere between 0.145 and 0.15, and our total yields scaled up considerably under those assumptions, and so that's the equilibrium yield at 30 percent spawning potential ratio.

DR. SEDBERRY: Matt, if I may interrupt, and I don't want to interrupt your flow, but, if this is a good time, we have a couple of hands raised.

DR. LAURETTA: I think it's a great time. That's sort of the wrap of that section, and so it's a great time. Let me just close with you can see a sort of trending-down effect if you drop the headboat landings a bit, and everything sort of scales down about a half a percent or so, and that's really the main point, and so let me just say, overall, we saw an increased landings estimate from the fishing effort survey, and that led to higher recruitment estimates for years leading up to those removals and an overall increase in the equilibrium unfished level and sort of things scaled accordingly. Okay. Questions, please.

DR. SEDBERRY: Thanks, Matt.

DR. SERCHUK: It's just a question of labeling. In the last two slides, this one and the one before that, it's unclear to me what the units are. Is that millions of tons, or is it millions of pounds, or is

it millions of fish for recruitment? I'm not sure, and it would be helpful if the labels for the units were on these figures. That's all. Thank you.

DR. LAURETTA: Yes, absolutely. That's a very valid comment. Recruitment is in thousands of fish, and so, in this case, it's around ten million is the equilibrium, and you can see that on the next slide there, and it's just under ten million in SEDAR, and you're absolutely right. These are -- They do appear unit-less, and I'm sorry, except for the bottom most important one, and that at least has its units, but that's fair.

We're talking about ten million fish here, and so these are in thousands, and this is actually a metric of eggs produced, millions of eggs produced, and it's sort of an abundance scaled by spawning output scaler, and so it's not a numerical abundance of fish, but it's really a metric of spawned production, and so it's sort of a spawner abundance times their maturity fecundity schedules that produces this relative production rate, and so this would be the -- The most simple interpretation is eight billion eggs, and so these are millions of eggs produced in the population, and so, in its unfished condition, we think that the population spawning output would be about eight billion, and we think that, at its lowest point, we think that it was knocked down to about a third, just over a third, of its production, and it has since rebounded, and, again, these are SEDAR 38 terminal effects, and so these aren't the current time block.

The interpretation is egg production, and so we're looking at -- That fits with the SPR 30 spawning potential ratio of 30 percent of this level is the target, and so we want to talk about reproductive potential of the population, and the minimum threshold is 30 percent from this assessment here. Does that answer your question?

Then we can look at it here, and let me run through the units again, and this would be -- Biomass is in millions of eggs produced. Total biomass is in metric tons, and recruitment is thousands of fish, and so 9.7 million, going to 10.2 under the increased charter/private, or slightly lower at 9.6 or 9.7. Then this F is exploitation rate by numerical abundance, and so the simplest interpretation is 15 percent of the individuals in the population is the target removed each year, and that includes age-zeros, and that's an important note, because they are highly abundant, and they do calculate into this, and so -- Did that answer your question, or can I clarify it further? I'm really sorry about that.

DR. SERCHUK: That's helpful. Thank you very much.

DR. LAURETTA: I think you'll find that I did capture the units on the figures, for the most part, and we're probably going to find a couple here where I may have dropped them, but, for the most part, I tried to be really diligent about making sure that I put not only the units, but that I put them in fishing units, and that is millions of pounds and inches.

DR. SERCHUK: Can I ask one more question? In many mixed fish stocks, fecundity of the largest females is very important, and, because there is differential growth rates and survivorship, one would hope that a large portion, or a significant portion, of the spawning stock biomass would be produced by animals that are these three to four-foot females, and would that be correct?

DR. LAURETTA: Certainly you're right. The large female fecundity effect comes into play in that calculation very much so, and that is the fecundity assumption is based on weight, and so,

given your growth patterns, the largest ones sort of have the biggest mass, and they do account for the most egg production, and that's why the spawning biomass is scaled not in numerical abundance, but trying to exactly account for that effect that you said that you want spawning potential, spawning output, and, therefore, the biggest fish in the stock definitely get higher spawning output, and so, just as a very simple example, if you had a ton of old females in the population, that might be an equivalent spawning potential -- Let's say a few old females in the population could be the equivalent to several hundred small, just-maturing fish, because of their egg production, and so that is absolutely in that calculation, and that's why we think the spawning egg output is a very good metric for this stock, because of exactly that. They mature young and small, but their egg production isn't nearly what it is when they are old and fat.

DR. SERCHUK: Thank you.

DR. SCHUELLER: I had a question about the headboat landings, and why would they have changed with the FES change, and the headboat landings should be coming from the headboat logbook survey that's done out of the Beaufort Lab.

DR. LAURETTA: Absolutely, and you are right. That's a great question, and it's not actually the landings themselves. It's the estimate of dead discards that changed, and we spoke a little bit about this, and it's based on a ratio of landings to charter and private, and so, while the landings themselves didn't change, the estimated dead discards did, and does that make sense?

DR. SCHUELLER: Yes, and why did the estimate of dead discards change? Did the discard mortality rate change?

DR. LAURETTA: No, but the number of discarded fish, as I understand it, is based on -- It's estimated from how many were discarded from the charter/private fleet, and it's calculated off the ratio of landings, and so, when charter and private landings went up, headboat landings -- Actually, retained landed fish did not, and then the number of discarded fish that got allocated to the fleet went down, and that accounted for the change in overall removals. That's my understanding.

DR. SCHUELLER: Okay. Thank you.

DR. LAURETTA: It's a very valid question, and I had to dive into it too, because I spent -- You wouldn't believe how much time I spent in replicating validation of data in replicating methods from SEDAR 38, and that stood out, and I had to really dig into it, because you're absolutely right that the landings themselves, the retained fish landings, did not change, and it really was when I added in the dead discards, just for model efficiency, put them in, the dead discards, and then treat the live discards as these are the fish live, and some proportion of them died, but, anyway, it's one of those things that changed, but, at the end of the day, it has -- As I've shown you in the last slide, it has very little bearing on the assessment, and then the live discards themselves is completely negligible, but even the dead discard change didn't have a large effect, and it was really the charter or private change that affected everything, about 5 percent. It only changed the landings by about 30 percent, on average, but it changed the overall time series and benchmarks by about 5 percent upward. Does that answer your question?

DR. SCHUELLER: Yes, and I can look at the document a little bit more, if I need to.

DR. LAURETTA: Okay. I don't have any clearer explanation. That was my understanding, going into the data inputs, but are there any other questions on the Section 3, the effects of the fishing effort survey?

DR. SEDBERRY: Matt, there are no hands raised, and so I think you can move on to the stock status updates.

DR. LAURETTA: Okay. This will be the longest section, and so, if there is a pressing issue, please go ahead and interrupt, and I really don't mind, because this one is going to sort of go through the weight of evidence that leads us into Section 5, the projected yield, and so let's just go ahead and dive in.

First, I just want to talk about model performance, and particularly its convergence was very good, one of the better that I've seen ever, and it was really stable in its solution, and both in its producing the long-term time series and benchmarks, but also in that it found a very consistent solution, even when I jittered the parameter values, and I gave them a very strong jitter. First, I started small, and it barely blipped, and then I gave it a very strong jitter of parameter combinations, to see where it would land, and you can see, most of the time, it lands on this red line, and we're looking at the negative log likelihood, and so the objective function, and so lower is better here.

The red line was the base model that I wound up projecting out, and you can see that, most of the time, across all parameter combinations, it wants to go to that point, and there's a few where it sort of has a less-informed estimate there, less information, but, overall, it hits this, and you can see that the right line gives me some confidence that we're at the parameter maximum likelihood estimates.

Again, it had good agreement with the time series, both trends and scale, discounted for the change that I explained in the previous section, and it's Hessian solved, which allowed us to get the parameter variance-covariance estimates for uncertainty around our estimates, and, therefore, as I mentioned, given this model performance, we would determine that the following estimates are at the parameter maximum likelihood values.

The overall fits to the indices of abundance, and so, here, we're looking at the three fleets that were fit to and the shrimp effort, which was a driver by that, and, again, so it's perfect, so you're not wondering why that one fits exactly, is because that's how it was defined, as an effort driver, and you can see the commercial handline, and these are shown on the log scale, and so this is how the model is fitting them, and I apologize if that's not labeled well, but, essentially, we're looking at the log scale, such that zero represents about the average of the time series, since they're scaled to their mean.

We can see this sort of cyclical pattern in the commercial handline, and we see that, overall, SS3 does a pretty good job of fitting the trends in the indices, and we can see that it captures that peaking and lull in the commercial handline and rebound, and it does a good job with variation over time periods for the recreational headboat, which is the longest time series for an abundance index, and it sort of -- It captures that drop at the end and rebound, but not to the scale -- It captures the trend, but it misses the magnitude of it, and that's not surprising, given the integrated nature of this model with the different -- All the different sources of information, and these overall are pretty acceptable fits, I think, the fact that it's getting the trend.

Then, when I look at the SEAMAP trawl survey, it appears to me that there's a little bit of this offset, and I spoke with Tracy and everyone, and there is some -- I had put a research recommendation in there to evaluate the age references on here, and there was some mention that they've got new indices that are perhaps ages zero and one, split differently, and I'm wondering if there is some effect of age-ones in the index being modeled as recruitment. Anyways, it's a research recommendation, but, overall, it fits the general patterns across the indices quite well, but it just misses the sort of magnitude of change in some years.

Fits to the fleet length composition, here, on the left, I'm showing the overall time aggregated distributions by fleet, and this is an output directly out of SS, and so the different colors on the chart are actually the genders, and the male is blue, shown in mirror image on the bottom, or I shouldn't say mirror, but reverse image on the lower side of the access, and the red is the female, shown in the upper panel, and then the green are the ones where gender was not assigned, and so, again, I want to highlight the fact that the bulk of our data is in the handline, and that's, of course, where we see these really great fits to the data.

Overall, we see that the model is capturing the size ranges of the fleets quite well, and, where we have a lot of information, it actually captures -- The fits are quite good, and they overlap between the model predictions of the lines, and the observed data are the gray distributions. We do have some divergence for fleets, particularly the ones where we don't have very good information, like the gillnet, but, overall, it gets the range right, which is really important for the fleet selectivity estimates, vulnerability to the fleets.

There's nothing too divergent. On the tournaments, we do see a bit of a skew here, and I'm not sure whether that's a sampling issue or what, but, overall, I would say fully acceptable fits, and then, when we look at the residual patterns on the right, we see sort of these large bubbles. The filled bubbles are the positive residuals, and the open bubbles are the negative residuals, and so we're looking for sort of a random pattern, and, again, the colors are the gender-specific, and the gray, I think, are the combined.

Where we see the real large residuals are actually at the tails of the distribution, which is not surprising. We have few fish observed in those, but, when we get to the -- Really, within the modes of the distribution, we see quite good residual patterns overall, and this is discussed, I think, in detail further in the report, but the take-home here is that, where we have the best information, we actually have quite good fit, and we don't have a lot of fish with gender identified in the charter/private, and so that's some of what accounts for that, and we have quite a few samples, but you can see, most of it, the vast bulk of it, is unidentified gender, and that's the green distribution, but, overall, we're capturing the modes and the ranges well, particularly for the main fleets.

Now, for some of these, I have tried to really highlight the model consistency, and that is its stability to produce historic or overall patterns, given new data, and I have sort of put the estimates from the previous SEDAR and then the updated versions below them, and so there's going to be several slides like that. These are some of the busiest graphs that come out of SS, and so I'm going to walk you through them a little bit. We're just going to focus on the overall general patterns, and the first thing I want you to know is that the estimates of selectivity did not change between the two assessments, even though we've added five years of data, which was roughly about 20 percent more data for that time period, I believe.

Overall, we get very consistent selectivity estimates by fleet. On the left are the length-based, and so this is centimeter fork length, and age in years is here, and so this is actually fit to an SS, and this is derived within SS, based on the growth and the length composition.

The next thing to note is really this strong doming pattern for most fleets, and that is the largest fish aren't really vulnerable to most fleets, except for the tournament, and it is the one that is defined to be asymptotic, the one that gets the largest, oldest fish, and that's these gray lines. The dashed versus solid is male versus female, and dashed is male, and so let's just take a focus on our main fleets here, and the black line is the handline, and so it's ranging about sixty centimeters to somewhere in -- It's the predominant vulnerability within there, and that equivalates to about ages-two to roughly six, where these fish are highly vulnerable.

The tournament on the other hand, these orange lines here, and then it turns to red here, we see that they're not really vulnerable to the tournaments, and they become fully vulnerable somewhere around 100 centimeters, or that's roughly six years of age or so.

The domed selectivity becomes really important for these recreational fleets, obviously, and having that tournament fleet to ground having that one asymptotic fleet becomes really important for SS. Otherwise, it sort tells it what the length composition of the largest age classes would be, size classes.

Overall, the fish enter the fisheries around twenty-four inches of age, and then the tournaments get the largest, oldest fish, about three feet and bigger, ages-five and older, and so it's about -- Again, if you have some clarities, go ahead and stop me as we start to get to the mortality biomass.

DR. SEDBERRY: Thanks, Matt. We do have a clarifying question, first from Marcel Reichert and then Fred Serchuk.

DR. REICHERT: Thanks, George, and thanks, Matt. You may have mentioned that, but, on Slide 23, and I think that's the Figure 4.7 in the report, the variability in the commercial handline, and it looks like also the recreational headboat seems to be constant, and is that a function of the graph, or was that a function of a set variability around the catches?

DR. LAURETTA: You're absolutely right. For the headboats, it was estimated from the GLM, and, here, I'm just showing two standard errors. I am just showing a normal distribution at two standard errors around it. For the handline, I think it had a fixed CV, and so you're right that it would sort of scale appropriately, but, again, these are just showing the two standard errors around the means coming out of the GLM, and so it's within --

DR. REICHERT: I was just wondering what the sources of the error bars were, because it seemed like, in the commercial handline, they're all the same, and, in the others, it looks like they are --

DR. LAURETTA: It's a great question, and I tried to give a visualization to the inputs in the SS, and so, in this case, I just calculated the standard errors and doubled it to put on there, but, yes, these were based on the inputs, and you're absolutely right, and, in some cases, I think it was fixed, where perhaps like the GLM produced a very small standard error, for one reason or another, and, you know, it can happen just based on your structure of your standardization models, and then the

decision is made that that's not a biologically realistic source of uncertainty for this fishery-independent data. Anyway, these are decisions documented in SEDAR 38, and I was just -- It's absolutely just me visualizing the assumed error structure onto the graph, if that makes sense.

DR. REICHERT: Okay. Thanks.

DR. SERCHUK: It's about this thing about the age-zero versus one index in the SEAMAP trawl survey, that figure right there, and I'm just wondering how reliable -- Without even talking about this particular circumstance, but how reliable do you think any index at age-zero is when, based on the Lorenzen curve, the natural mortality is above 0.8? I would expect there would be a lot of high variability in survivorship when natural mortality is that high, and I'm just wondering, because it becomes very much less natural mortality in age-one, whether any index at age-zero, in terms of a recruitment index, is really valid, and I just wondered if you have any thoughts on that.

DR. LAURETTA: Yes, I have a couple of thoughts, to your comment there. I mean, one thing is it shouldn't matter that the natural mortality is high. What would matter is the interannual variability in natural mortality. As long as it's high and constant, your relative abundance is, in theory, unbiased, right, and, if survivorship was constant every year, then the number of fish you observed would be related to the births.

Obviously, any index could undergo this scrutiny, and the key here is the art of modeling is understanding the information content in a data source. I will say that, having an age-zero fishery-independent index of abundance, from a theoretical standpoint, is extremely advantageous. As I mentioned, having a survey out there that observes the trends in the population prior to entering the fishery is not something we have very often, and it's actually a huge beneficial.

Now, if you're asking me how accurate is that survey to the true abundance in the population, I actually have no idea. You can see SS, an integrated framework, can account for the interannual changes, for the most part, but that doesn't mean that it's an accurate fit, that it's accurate to the biological reality. I mean, you would have to endeavor a pretty large scale, sophisticated tagging study, perhaps like a gene tagging would be a gold standard, and, if you did that over time, then you would understand the bias associated with the data series, but, as of right now, it's a relative abundance time series with associated observation error, and a lot of what you're talking about should follow into the observation error.

For example, if what you said is almost certainly occurring, and you didn't think that the survey-estimated observation error was accounting for that process error, then you would add that into the data input, but that was not a decision that I was at liberty to make. Overall, the model seems to use the index to inform the most recent recruitment, like within the last year or two, because we don't have any other information to inform that, and I'm going to show you, later on down the line, that maybe potentially there is some cause for caution in the most recent two recruitments, because we only have this index.

As the fish enter the fishery a couple of years down the line, we start to see it in the length composition data and the fisheries-dependent indices, but you're absolutely right, and we can see that in the retrospectives, that the model wants to diverge from the SEAMAP trawl survey as it gets more information, but I still would argue that having a juvenile survey to give you information on trends is an extremely valuable thing, in theory. Now, going out and validating your fishery-

independent survey, to understand how its detection probability changes over time, that is definitely an endeavor that we need to be thinking about, but it has to be designed very cleverly and in an a priori experimental framework, so that you really get at what you're trying to estimate, and that is the change in catchability from year to year.

DR. SERCHUK: I wasn't trying to dismiss the fishery-independent trawl survey information. What I was trying to get at, and I've seen this in other surveys that have a much longer time series, that there often tends to be a poor correlation between what you see at age-zero and what you see at age-one with respect to what recruits to the fishery afterwards, and now I know we have a shrimp fishery that takes very young individuals, and so that complicates things as well, but I am just wondering whether you have congruence throughout the series between what you see at age-one, let's say better than average, and does that also translate into age-one, and, if not, one wonders if one index is better than the other, or should we just go forward with both of them combined, and that's all. Thank you.

DR. LAURETTA: This, again, is a strong argument for why you would use SS3 over say a production model or something, and that's you want all the sources of information to act together to form your objective function, and that's why we don't just run a production model on the handline CPUE, because there's a lot going on with this stock, and particularly what we think might be changes in catchability associated with regulations, not completely unrelated to the biology and the environmental processes that go on with the stock, and this is why the charter/private, we decided in SEDAR 38, was probably not a good representation, and there's arguments for each one of these.

We went through a lot of this stuff during the last assessment workshop, and a lot of it is well documented, and I won't say it all is, but a lot of it's well documented in the SEDAR 38 data workshop report on what led us to these indices in particular. I do want to emphasize the update assessment, and I didn't go through the trouble of evaluating alternative indices, data inputs. I really stuck to the script, again, but these are the kind of things we should know.

I did note -- To me, I couldn't get off the fact that I want the SEAMAP trawl survey fit to be shifted over one year, and then discussions with the data provider mentioned that they're doing new indices that are age-zero and one separately, and then that's a very good recommendation for a research assessment, and I didn't want to go change the data structure and model parameterization, because it actually was a huge mission creep, and so I put it in as a research recommendation, is let's dive into this. Is this just a model tracking issue, or is there something in the data that we're not defining, or is there a better way, I should say, to parameterize it, but, either way, these decisions were made, and it is a -- It is the one fishery-independent survey in the model, and so, in theory, it's the one that should be less likely to be influenced, but, again, it's the small juveniles, and we know that their distribution can be quite variable, I'm assuming.

Anyway, it's a very good question, and it's a research recommendation. Obviously, we would always, always love to have a time series of relative abundance that had a calibration factor for changes in catchability, and we don't have that, and I can't tell you one index is better than the other. We did make some decisions in SEDAR 38, for example that the headboat was better than the charter/private, because the headboat is less likely to be influenced by bag limit regulations, and the bag limits for the whole boat is high enough that that's going to be less of an effect than

the charter private, which has very strict and changing over time, a critical component, and that's hard to model.

You could try and put a GLM effect in there, but there's no guarantee that you're going to get the effect right, and it just doesn't, and the best thing to do is -- Oftentimes, we split them, when we have strong changes in regulations, but, anyway, we didn't think that the recreational headboat would have that same bias in catchability, or really data reporting, I should say, and so observed catches of the charter/private, but, again, a lot of these decisions are well documented in the data workshop report.

I led the indices workgroup there, and we had lengthy discussions, and I think a couple of days even, amongst all the folks, the expert stakeholders, and I think we did a pretty good job of documenting the decisions during SEDAR 38.

DR. SERCHUK: Thank you, Matt.

DR. LAURETTA: Any other questions?

DR. SEDBERRY: There is another question, but I would just like to remind the committee though that we're going to have plenty of time for discussion of this, and, right now, we would like to get through the presentation, and, if there are clarifying questions that need to be answered so that we can move forward, that's the kind of questions we're taking now, and so, Amy, did you have a question?

DR. SCHUELLER: I have two. One, the index fits -- I know we talked about them quite a bit, but was there any decisions made at all to try to look at how to fit these a little bit better, maybe upweighting them or -- There's a number of things that could be done, and I think that the fits are not terrible, but they're not great, meaning that they have a lot of runs and residuals, and then my second question is, for the selectivity, how well estimated were the parameters? You have one flat-top selectivity, and then you have a plethora of dome-shaped selectivities, and I'm just wondering whether or not those parameters were well defined or not. Did you run likelihood profiles?

DR. LAURETTA: Okay. To answer your first question, I did not modify the index weighting settings at all, and so what you're looking at are sort of the fits, the data parameterizations, we had for SEDAR 38. In theory, you could upweight the indices and fit them much better, and you would probably be at the cost of another data source. It's that fine line between operational and research, where certainly I could upweight it all to the indices of abundance, and it would fit them much better, and the question is what information do you lose in the other data sources.

The one thing we have grounding this is we assume the length compositions are fairly representative of the fish being landed by the different fleets, and so the short answer to your question is no, I did not change the weightings on the data series.

The answer to your second question is most selectivity parameters were well estimated, with fairly good variance around them. There were a few that were not, and they are highlighted in the report, and there's a couple of the ascending limbs, and they are usually for ones where we don't have good information on them, and I don't think it was for the commercial handlines, because we have

so much data on them, but there certainly were -- They are highlighted in the report, and I would draw your attention to the model performance section of the report, where I think we discussed them in detail, to highlight the ones that seemed to be really uncertain, but you're right.

Most of them looked really good and well estimated, but there was a couple that it just didn't have a good grasp on, but, again, overall, the length composition fits are pretty good, to where we have the information, and we seem to be capturing the general overall pattern in the fishery, but, yes, certainly you can't get them all perfect, and there are a couple out there that had high variance around them, and the nice thing about -- I have to go back that the nice thing about SS is that it is accounting for these kinds of uncertainties in the integrated framework, and not all models do that, and it is a balance between information sources in your objective function fitting.

I mean, you're going to have to -- These things sort of squeeze against each other, especially when you throw a recruitment index into the mix, and it wants to try and fit that recruitment index, but the length compositions could be saying something completely different, and it has to sort of balance its information, as we defined it, and I did not alter that balance. I left it as it was defined. Does that answer your question, Amy?

DR. SCHUELLER: Yes. I mean, point taken. I understand that this is an update, and there is limitations, but it just seems like there is some potential to do quite a bit of exploration with this model in the future, which is true of every assessment, of course.

DR. LAURETTA: Yes, but, again, I have to emphasize that, actually, the room for exploration in an operational assessment is not as great as people would like it. These are the reasons we have research recommendations, is that multiple sets of eyes will see multiple things, right, and that we all have our preferences, and you would probably prefer to see these CPUEs fit better, at the cost of other data, but the panel's decision at the time was that we would rather have this balance and how much goes back to the argument that I just got before yours, is that how much do we believe the CPUEs, and you're telling me that you would like to believe them very strongly, and you would like the model to fit the CPUEs at the cost of other data fits, and other people have a different opinion on that. These are why we need our research track assessments, to explore things in a framework where the ability to provide the timely advice is not necessarily of the essence and the main objective.

DR. SEDBERRY: Thanks, Matt. I don't see any other hands raised, and so I think we're ready to move on to fishing mortality. I believe that's where you left off.

DR. LAURETTA: This is my favorite part of the job, in all honesty, is looking at what it all means, as far as from an integrated information standpoint, what can we infer, and this is my favorite part. Fishing mortality, let's just take a quick look at the graph here, and, again, we have the time series down on the X-axis, and we have the exploitation rate, which is, again, the proportion by numerical abundance removed annually.

Here, one thing to note is, again, we're seeing very consistent scale of the F estimates from the SEDAR 38 and the F benchmark, which means SEDAR 38 and this update, and there's nothing that diverged considerably, which a comment by Eric yesterday is it's often surprising when we can get such great replication of a historic series, and here we see, overall, that F has been relatively constant since 2010, somewhere around 4 to 5 percent of the stock abundance per year removed,

and that's this block of points here in the terminal, and, really, it represents the lowest estimate since about the 1970s, and so the stock, according to SS3, is at its lowest exploitation rate in decades, and has been for a good part of the entire decade here.

This is well below our target FSPR 30, and it's a little lower -- It seems a little lower, but it's kind of not. It's sort of a rounding thing. It was like 0.144 here, and I think it was 0.147 from SEDAR 38, and so, yes, we dropped half a percent in the rounding change, but, overall, we're within that same target, and you can just then infer that, if our fishing target is up here, which this model says the stock has never been exploited to its target rate, and, again, that includes age-zeroes in the numerical abundance, we see that the determination is that the stock is not undergoing overfishing. It's quite clearly lower than the target of SPR, the fishing rate that leads to an SPR 30 at equilibrium recruitment.

Stock recruitment, here, I just want to reiterate the main assumption in the model, and let me just come out and say I did not explore alternative stock recruitment assumptions, other than as a sensitivity of the steepness value of both the profile and the time series effects, and that's in the supplemental information, if we want to review it, but, overall the lack of recruitment pattern observed, both in the VPA and from SEDAR 38, led the panel to recommend that steepness be fixed at 0.99.

Why it's important to acknowledge the VPA in that is because the VPA actually makes no assumptions about the stock-recruitment relationship, and so the plot of that may give us some indication of if one exists, versus here we've told it a Beverton-Holt recruitment fixed steepness of 0.99, which gives us this sort of scatter around a long-term mean that I spoke about showing up in the upper-right here.

Our assumption is we have never gone down to the point where recruitment -- We have never fished spawning biomass down to the point where recruitment would decline, and that's the basic assumption, and that was retained for the update. The estimate of unfished recruitment is about 9.8 million age-zero fish per year, on average, and, when we look at that time series of data inputs, we actually see this cyclical pattern emerge in the recruitment deviations, and that is the amount of deviation around this mean from year to year, according to the data fits, and what we see is this strong cyclical pattern that appears to be broadening, to me, a little bit, but here's where we left SEDAR 38, right in this lull down here, 2001 and 2002, and so right here was our last data point, and we spent a lot of time discussing what future recruitment might be.

SS says that this cyclical pattern sort of followed -- It has bounced right back up from the average and into a period of above-average recruitment, and then it's trending back toward the average now, and so this is a complete flip in the recruitment trends from SEDAR 38, whereas the discussion was we were at an all-time low, and now the discussion seems to have flipped about these are two of the highest recruitments on record here for 2015 and 2016, and so it's a pretty strong change. I wouldn't say change in trend, but a change in condition, the ending condition, of where we think we are.

Then this becomes really important as we sort of go into our discussions about yield, because, in my viewpoint, this is one of the -- Let me just go back to that for a second, or we can focus here on the recruitment estimates. These are in thousands of age-zeroes, and so this is the ten-million fish mark here, and here's where we left SEDAR 38 estimates, in this period, and, here, what we

see -- The number-one thing to note is that, even after five years of data, we're still seeing a terminal period of low recruitment, and so that's really important, that we're not seeing a complete divergence in near-term recruitments when we add data to the model. Then we see this sharp change to what I was mentioning are two of the highest recruitments on record, upwards of twice the unfished average level estimate.

This is a pretty drastic -- From the lowest to some of the highest, and my intuition is that this kind of effect in reproduction of the population wouldn't go unnoticed. When we saw this decline here, and, again, is it a model tracking landings with this other information, and say, well, landings declined sharply, and so recruitment has declined, and landings are increasing, but the landings didn't increase at this level. When you see the information, the support for the recruitment in the juvenile survey in 2016, not so much in 2015, but this becomes really important on the amount of yield available to the fisheries in the near future, and so I think some sort of validation by observations on the water to help sort of give a reality check on this.

You're going from one of the lowest points on the time series to some of the highest, and I think that that would be observed on the water, most likely, and I would be curious for this stakeholder feedback. When I was looking back at SEDAR 38, and sort of tracking then and now, and I read through Amendment 26, I think it was, and I read about the stakeholder feedback indicating high juvenile abundance compared to previous years, that would have been in 2014, and that would have been right around -- Before these events, and so this swing was already seeming to have some observations, and I'm curious of what the -- What we think the anecdotal evidence from the field might support that large swing.

I mean, it's rare to see such a drastic change in state of reproduction, and we know these things have large cycles and good and bad years, but we're talking about multiple years in a row of bad going into multiple years in a row of good, and it seems to be trending in the landings, and now we think that a lot of these fish are going to start entering the fisheries, probably last year or this year, and then the tournament fisheries over the next upcoming year, and so these are the kind of things that, to give the groundtruth to the models, a very strong prediction on what yields are going to do when you go from sort of that five-year low to a five-year high average, and, obviously, the prediction is that yields should increase quite a bit. There is no way to validate those predictions on my end, and it has to come from the observations of the population.

As just a quick side note, I did go out mackerel fishing on a charter, and they were incredibly abundant, and that was January of 2019. Anybody who has ever fished with me is going to know that, if I'm catching a lot of fish, it's not because it's special, but it's because the fish are incredibly abundant, but it was the mixing zone, during the mixing period, out of Islamorada, and so I don't have a good reference, but I know there's a lot of people that have fished this stock for many, many decades, and I'm curious if this is -- This is what I would love to focus on in the discussion when we get to projected yields, and that is, is this effect seen on the water. Anyway, I see some hands up, if you want to just --

DR. SEDBERRY: Yes.

DR. SERCHUK: Matt, could you just tell me what the -- When you say fishing mortality, what metric is that? Is it over the entire age groups or the entire recruited age groups, or is it an apical F? I'm not really quite sure.

DR. LAURETTA: It is removal by abundance, and it's numerical, across all age groups, and so it's going to be your total abundance removals. Does that make sense? It's proportion of stock across all age groups.

DR. SERCHUK: Let me make a statement, and then could you tell me whether this statement has any merit? Even at its peak, when F was 0.1, or 0.12, that is still below the natural mortality rates assumed for the stock for all recruited age groups, and so it seems to me, if that statement is true, that the -- Throughout the history of this stock, the primary determination of stock dynamics is natural mortality. Would that be a fair statement?

DR. LAURETTA: It's certainly a major factor, but it's the interannual variations in recruitments, I would say. Obviously, this fishing mortality that occurred here occurred during periods of really good recruitment, according to the model, and that's why we had sort of this bulk buildup, where here we've got a sort of low period and high period, extreme low lull, into sort of a couple of -- But, for example, if this thing went down to here during that time, F would shoot through the roof.

Yes, natural mortality is, of course, an important factor in defining what we think the estimated abundance of the population is, and it's a fixed assumption, and there's no doubt about it, and then the fishing mortality gets sort of estimated off of that, and so it's not insignificant at all, but we're seeing recruitment estimates that track landings, right, or is it landings tracking recruitment?

I mean, this is always a question in my head. Is this thing creating patterns of recruitment based on landings trends, which could be related to regulations or environmental vulnerability or availability of fish type things, and, obviously, we don't have shifting availability by year, except the management regulations in here or there in size, but, yes, it's a great comment. Certainly you are right in your interpretation that the stock is estimated to have never undergone overfishing, and therefore it's never been overfished.

DR. SERCHUK: Matt, it's not that it's never undergone overfishing. My point is that adult estimated fishing mortality varies between 0.2 and 0.16, based on the natural mortality curve that you used, and the fishing mortality in any of that period is never even approached, perhaps just once, by any of those natural mortality rates for fishes that are age-one and above, and I'm just saying that it seems to me, if there is parsimony here between the assumptions of natural mortality and what we're seeing in the fishing mortality rates, it must be that natural mortality is the predominant factor in driving the dynamics of this population.

DR. LAURETTA: Of course. I mean, there's no denying that fact, and you're absolutely right on that. For example, if we dropped natural mortality to 0.05, it would make up less recruits, and not as many fish would have had to die to see the landings we're seeing right now. The removals are a fixed input, and it would say, okay, recruitment is lower, because not as many fish die, and you took this many, and there's fewer in the system, and F would go up, and you're absolutely -- There's no denying that that is a major assumption in the stock assessment.

SEDAR 38 did sensitivities on it, to evaluate alternative natural mortality assumptions, and those are all well documented in the SEDAR 38 stock assessment report, but there is absolutely no denying your statement as being true, that your natural mortality assumption has great bearing on your abundance and fishing rate estimates.

Seeing no more hands raised, let's go ahead and look at the stock biomass. Again, I want to highlight the consistency in scale between the trend, the time series trends, from SEDAR 38 and the update, and so the SEDAR 38 results are shown in the upper panels and the update in the lower panels. Spawning output, again, is in millions of eggs, and so approaching that somewhere around eight-billion eggs in unfished equilibrium, and its lowest point is around 2.7 million.

Total biomass is in metric tons, and what we see is we saw this sharp decline at its lowest point in 1998, when the fish was expected to be at its lowest biomass level, both total and spawning, a fairly sharp increase, and then some downward trend up until SEDAR 38. Since then, the model has estimated a few more years of decline and then this quick turnaround and rebound related to those recruitment events that it's estimating, and so here is the effect of the low recruitment on the spawning biomass, and, again, they're maturing starting at age-two, and so it just takes a couple of years between age-zero recruits to start affecting the spawning biomass, and then the total biomass goes a little sooner, because of the age-zeroes and ones and accounting for that.

Overall, the thing is that the stock was determined to be not overfished during SEDAR 38, and spawning biomass had increased fairly steadily since 2013, and so, naturally, we would say the stock is still not overfished, and the current spawning biomass ratio is about 1.7 times that SPR 30 target, which is around 2.4 billion eggs production.

Our overall -- Let's just take a look at the big-picture overview then on the update. Fishing mortality is estimated to be around 4 percent in 2017, with a target around 0.14, and so, obviously, the stock is not undergoing overfishing, according to those estimates, and the ratio is about 0.29, with a 95 percent confidence interval of about 0.2 to 0.4, and so the fishing is roughly a third of the target right now.

Recruitment is at 9.8 million age-zero fish, on average, and that's the unfished average equilibrium, and that's the value that was used to project yields into the future. Spawning stock biomass is 8.1 billion eggs in an unfished state, according to that equilibrium recruitment, and our target is 30 percent production, egg production, and it's not exactly a one-third ratio of the biomass, but it's 2.4 billion eggs produced, and our current estimate of egg production capacity is 4.2 billion, and so the stock's biomass spawning output is higher than its lower threshold, and the stock is not overfished.

The ratio is about 1.7 biomass to target, with the 95 percent confidence interval of about 1.6 to 1.8 ratio, and so about 70 percent higher than the target. The yield in 2017 was around nine-and-a-half-million pounds, and the equilibrium target is 18.3, and an optimum yield, which is 75 percent F SPR 30 equilibrium, is 16.7 million pounds. Let's go ahead and take some questions.

DR. SCHUELLER: I just wanted to make sure that everything regarding fishing mortality is on the same page and we're comparing apples to apples, and so, when you say fishing mortality 2017, you mean exploitation rate in 2017, and then, for the fishing mortality for SPR 30, is that also the exploitation rate?

DR. LAURETTA: Yes, that's absolutely right, and so, as I had sort of tried to highlight in the methods, fishing mortality here is in overall exploitation rate by numerical abundance, and so that's for both of those metrics. The 2017 is actually the 2017 fishing year, and so that would have

been March 1, 2017 through February of 2018, but, yes, they're both on that same scale, and so the ratio is valid.

DR. SCHUELLER: Okay. I'm just making sure that I know exactly what you mean by fishing mortality rate.

DR. LAURETTA: Exploitation rate, or harvest rate, and maybe harvest rate is -- Harvest rate by numerical abundance is a good way to sort of clarify it, I think.

DR. SCHUELLER: Okay.

DR. SEDBERRY: Thanks, Amy. I don't see any other hands raised, Matt.

DR. LAURETTA: Okay. Well, then let's go ahead and look at what the previous comment was suggesting, and I think this would be a main area of really a question that I have for the group. You know, I entered into the scene for SEDAR 38, and I know there's a lot of people with very good institutional knowledge of both the stock and the fisheries, but here we look at the two graphs on the right are showing the ratios compared to their target benchmark, SPR 30 benchmark, and so the top one is spawning biomass in eggs produced.

The ratio, you can see that it approached the target from a biomass in 1998, but it was still above the management threshold, indicating that the stock has never been overfished, and, on the lower one, we see that the fishing mortality was exactly as the comments. The stock hit its peak in 1998, but it never really -- It never got to its estimated benchmark.

Part of this may be the numerical effect of young, and we don't have fisheries for the young, and so there's always a lot of somewhat non-vulnerable fish, but the real question -- Here is where you start off in an unfished state, no fishing mortality, and so the ratio is zero and a high spawning abundance, and, as you up fishing mortality, it sort of follows that abundance decline towards the target, and here is -- If you were managing perfectly, you would be circling -- I should say if you're managing according to your objective, it would sort of dance around here, and maybe in the green, but what I would say is we approached the target in 1998, and then it sort of took that turn towards a higher abundance, and so it's moving back along the abundance, and F has gone down, and so it's sort of moving further into the green, you would say, which is the green is where you want to be, fishing rates just below your target abundance, spawning biomass abundance target.

I mean, the question, to me, is what I don't know, and I've read through the historic assessment reports, to try and figure out was this -- Is this a valid goal, and the goal is to bring it lower than it has ever been on the biomass, and that's really a question for the experts outside of my expertise. You know, I can interpret the model to the best of my ability, but I don't know that, but, right here, there's never been a problem, and, if the stakeholders have identified a problem here, then maybe this red line isn't where we want it, but, anyway, that's where the discussion comes in.

The stock hit its peak in 1998 of mortality, in its low of abundance, and the management target says push it even further into the yield estimates, and so that's where we're at now, is the projected landings. Any other questions on the updated assessment?

DR. SEDBERRY: I do not see any hands raised.

DR. LAURETTA: Okay. Given everything that I've shown you, it should be no surprise that the projected landings are going to be very positive, given the fact that there's this change in recruitment dynamics from the last assessment cycle, or maybe not dynamics, I should say, but current recruitment state, and the advice in SEDAR 38 was to observe this lull, and there was proposed different levels of recruitment, whether they thought that lull was going to continue into the future, whether it was going to go back to best to project at the equilibrium or go to a high state, and the stakeholders had a lot of really good feedback.

I would say, overall, that that assessment was the best interaction between stakeholder engagement in the stock assessment that I've been a part of, and I appreciated that fact when I was updating this model, that so much of the historical decisions that had to be made were made by people who had been observing the resource for a long time, but, anyway, I diverge. The point is here that we've seen a complete change in recent recruitment, and that is going to lead to a very strong change in landings projections.

Let's just go over the specifications really quickly. Here, I just assumed future recruitment equal to that unfished equilibrium mean, which is sort the mean across the time series, as we've defined it anyway, and so some sort of average level there at 9.8 million fish produced a year, is the assumption into the projections. If we're only doing short-term projections, the most recent couple assumptions are somewhat invalid anyways, because they're just discards, and they don't have bearing on landings until they become vulnerable to the fleet.

I kept selectivity and the relative allocation among fleets averaged over the terminal two years, consistent with SEDAR 38. The fishing year landings from the last three years, I rolled over the 2017 estimates. I had no other information to diverge from that, and so we'll take that into account, and I looked at four constant exploitation rate scenarios, no fishing, fishing near the current level, around 4 to 5 percent, and it's about four-and-a-half percent, and, as I mentioned, it's pretty steady for a decade, almost, and fishing at 75 percent of the target, 0.11, roughly a twofold increase, 2.2-fold increase, from our current rate, and then fishing at the management target of 1.4, again an exploitation rate by number.

Let me run you through these graphs again. Over here on the right, we have the projected landings, and so, up until here, where we see the lines, are just the model estimates of what SS is doing its conversion into -- I've actually done the conversion in millions of pounds for us here, since it's the advice, and I wasn't going to leave it metric, and, here, this is billions of eggs, and I scaled it, and so it's 8.2 or whatever it was, the base, and so here we're looking at the four scenarios.

The dashed-blue line, of course, is no fishing, and landings goes straight to zero, and the dashed red line is fishing at the management target SPR 30 rate, and that predicts a large jump in landings as all -- For two reasons. Not only are we well below that fishing target currently, roughly a third, but, two, it's got the cohort effect from all those recruitments entering into the fishery and becoming available, and, as they move through, we see a gradual decline towards the equilibrium, as those average recruit assumptions are taken.

When we look at SPR 30, it says similar, a spike in the next year's fishing landings, up to around twenty-five or so, and so it's considerably more conservative, from a yield standpoint, and then tapering to landings below the SPR, and I think it was 16.7 million pounds.

Then the current F , the bold, black line, suggests that, at the current about 5 percent removals, we're going to see a fairly consistent landings level near the recent levels, and that would lead to about biomass being approximately stable, versus, of course, no fishing, and it would slowly build towards that unfished equilibrium, and, if it went under $F_{SPR 30}$, it would immediately begin dropping towards the management target of 2.4 billion eggs, as I have shown here, and so I've sort of shown these dashed lines represent the lower as the target and the upper as the unfished, and then, of course, at 75 percent, that target fishing mortality, we get somewhere of a lesser biomass decline, to it looks like right around where it was at its lowest point, or maybe slightly higher. It's a lower equilibrium yield, 16.7, but it's slightly higher spawning biomass, near its historic low estimate.

Then here is sort of a visualization of the uncertainty around those estimates, and so each of these down on the X-axis is the projected landings in millions of pounds for the next five years is each a panel, 2021 to 2025 fishing years, again. Here, I'm showing the estimate with its observed error structure, normal distribution, given its parameter estimates, and I have highlighted the P^* of 0.5, and that represents the overfishing limit, and that would be the mean of the distribution, and I have put the values over here in the text on the graph.

Each of these dashed lines represents a different P^* value, to really visualize how the ramp-up in landings then needs to taper down as those recruits move through the system and the sort of equilibrium assumption kicks in. Here, we can see the next fishing year's overfishing limit is thirty-four million pounds, tapering down to about thirty, twenty-six, twenty-four, twenty-three, and then eventually towards eighteen, as I showed in the previous graph.

The forecasted yields for the next five years are much higher than the current yields. Again, this is the fact that the exploitation rate is significantly lower than the estimated target, as well as the recent trends in recruitment, and then the catches would decline each year, as those cohorts move through the fisheries and depending on what's coming behind them, and, right now, it's just assumed that 9.8 million fish per year.

What that would then look like in a decision table across our different quantiles of uncertainty, again, here is the overfishing limits by year, and here are the numerical values that represent each of those P^* lines overlaid on the distribution. Are there any questions, or can I just run through the summery slide here to sort of wrap it all up? I am not seeing any hands at the moment, and so I'm just going to go through it.

In summary, the South Atlantic stock of king mackerel is determined to be not overfished, and the fisheries are not overfishing. The current exploitation rate is estimated to be much lower than the target of $F_{SPR 30}$, and $SS3$, as it's configured from SEDAR 38, proved to be quite stable, both in its long-term estimates as well as its ability to find a consistent solution, and that's our first really desirable property in a management tool.

The second point is that it's really responsive to current data, and so it's -- Even though it's stable in its long-term trends, it's picking up a signal in recent data, and so it has the potential to be a very useful management tool, and the goal now is to validate it, and it's a unique opportunity to see this cycle go from such low to such high that I'm curious if it has been observed in nature, and, again, recruitment cycled from a five-year low in SEDAR 38 up to a four-year high recently, at

least two of the highest recruitments on record in 2015 and 2016, and these high recruitments lead to a large increase in near-term catch projections. If fully exploited, target landings in 2021 will increase sharply, followed by the steady reduction to 18.3 equilibrium estimates.

With that, I would just like to thank, again, all the people that have worked on this project and worked really diligently to make sure that we met the timeline, and it certainly is a group effort, and I'm proud to be able to present their work, but, at the end of the day, the team really rallied, and I thank them, and so let's go ahead and take questions, I'm assuming on projections, and so let's start there, or any questions, actually.

DR. SEDBERRY: Thanks, Matt. That was a really great presentation, and there was a lot of information, and it was well presented and very well organized, and I appreciate the presentation. At this point, since we've been going for over two hours, I would like to take a biological break, as I know that I need one, and I'm sure other people do as well, and then, when we come back from that, we'll take public comment and then any final clarifying questions and then the discussion, and so let's take a fifteen-minute break now, which would put us back at five after eleven, and then we'll take public comment when we come back from that, if that's okay with you, Matt. Does that fit into your schedule?

DR. LAURETTA: Absolutely, and thank you for that feedback as well, but, yes, I would be happy to take a quick break and grab a glass of water and whatnot, and, again, thank you for that feedback. It actually means a lot to me. Presentation is half the game on this stuff, and it's easy enough to run a model, but getting the message out is often challenging in and of itself, and so thank you.

DR. SEDBERRY: Thank you. We will be back at five after eleven.

(Whereupon, a recess was taken.)

DR. SEDBERRY: Welcome back, everybody. Again, thanks to Matt for that presentation, and we're going to continue to take clarifying questions and then discussion of that. Before we do that, I wanted to see if there was any public comment on this agenda item for the stock assessment for king mackerel. Rusty Hudson, you may unmute yourself and go ahead.

MR. HUDSON: Great presentation, Matt. Being at SEDAR 38, I remember that we tried to do our due diligence with everything, and one of the first things that I wanted to comment about is the South Atlantic Council has been trying to be innovative, so that we can make management adjustments to have a better opportunity with our trip limits to catch the allocations, because, here on the Atlantic side, we've had an inability, for a variety of reasons, smaller boats and what have you.

Historically, I saw a lot of things occur, back in the 1960s and 1970s and 1980s and 1990s, but, back in 1977, we had those big gillnet boats for the king mackerel and spotter planes and stuff like that. All that ended by 1989, but that did have an impact, and so the little bit of gillnet fishing that's going on now is not near what it was back then, nor do we have the foreign fleets in here that Magnuson in 1976 and 1977 eliminated.

On another historical view, and Dr. Chip Collier can comment to this, and Chairman Sedberry and everybody else that's been part of the fishery by the South Atlantic Council citizen science, and

the pictures from the 1940s to the 1970s measuring king mackerel will probably be something that will evolve out of that, once they get confident about the way to do all of that.

One of the things about the tournament fish, and we had a little period there where these large fish were said to have mercury issues, and that's without discussing selenium negating some of it, but the larger fish didn't always dominate the market for the commercial landings. It was usually the different age classes, and, to that effect, Matt, in recent years, a lot of my fishermen and fish houses have been seeing numerous fish age classes, and that's a good thing.

One of the downturns of things, like let's say the most recent one, Hurricane Dorian at the end of August, and then we get set in on this east coast of Florida and up the coast in the South Atlantic region by a whole bunch of 'nor'easters, the most wind we've seen in decades on a pretty sustained basis for several months. That has impacted stuff. Earlier on, we had a lot of cold-water effects, particularly down here in Florida, and I heard the North Carolina stuff came up, and, a few years back, Jeff Odin up there was telling me about the need to have to go further and offshore to get to the king mackerel, and they weren't in where they had normally been, but I would have to talk to him again to zero-in on the years.

This coming fall, we're going to be increasing our trip limits, once National Marine Fisheries Service finalizes the rule, and that second-half quota of the year will be getting better. Back to Wilson's comment about things with where to find the age-zeroes and age-ones, SEAMAP uses a net a lot like what I used there in the 1980s, and I had a big mongoose tongue net, hundred footers on each side, and normally I wouldn't see these little mackerels, except in the wings when I was hauling back, particularly if I was inside of sixty foot of water, which wasn't very far off the beach overall, but the point is that I just really never saw much in the bag, and those that were in the wings were just the right size to get gilled in that shrimp net, just on the haul-back.

There's another thing that I'm wanting to point out. We have had a huge reduction in the number of shrimp boats on the east coast of the United States here since the late 1980s and early 1990s, and even the places that built a lot of these boats are totally out of business now, and so there's a lot of merit to, later on in the future, with the next stock assessment, an operational or research track would probably be better, and there could be more information that could be made available in the model, but I think this SS3 and what you've got going here looks very good, and I'm happy that you all did this work, and so thank you very much, and that's my comment.

DR. SEDBERRY: Thank you, Rusty. I appreciate your input and your historical perspective, too. Matt, did you want to reply to that?

DR. LAURETTA: I just wanted to say hi to Rusty and thank him for that. Absolutely that kind of historical perspective is something that I could never provide to this committee, and I agree with you. My overall take on SS3 is it has major potential here to be a useful tool for management, because of its stability, and it's long-term, and yet it's responsive to the data sources, and those are two things that you absolutely want.

The last picture has to come from those folks out on the water, to say that absolutely there's more mackerel. I mean, the predictions are that there is boatloads of mackerel in the system that there haven't been, and the people with the boats have to validate that, but I agree with you, and I

certainly appreciate your comments and your insights over the history of the stocks, things that I could never provide, to provide groundtruth to the model.

MR. HUDSON: It's my pleasure. I was part of SEDAR 16 also, and then SEDAR 38, and, as far as the update, I think you did a great job with what you had to do. Thanks.

DR. LAURETTA: Thank you, sir.

DR. SEDBERRY: Thanks again, Rusty. We have some hands raised, and I have Amy Schueller first and then Alexei.

DR. SCHUELLER: I wanted to just clarify what the target and what the limit, or threshold, was here. It seems like the word "target" is getting used interchangeably, and is the F SPR 30 the threshold or limit, and then 75 percent of that is the target?

DR. LAURETTA: Well, I guess I'm looking at the overfishing target. That's actually a council - I will let the committee answer that. I was providing the benchmark of -- Here, I'm talking about target F as being SPR 30, the exploitation rate that leads to an equilibrium spawning potential ratio of 30 percent.

DR. SCHUELLER: So it's a threshold value.

DR. LAURETTA: Threshold, fair enough. Threshold. The upper limit.

DR. SCHUELLER: Okay.

DR. SHAROV: I have two questions. Number one is not a question, but a compliment. That was a really great presentation, and thank you so much. It was very detailed, and I really enjoyed it. A question is the presentation of fishing mortality -- The choice of presentation of fishing mortality is different from the standard, and so I would like to ask why was the choice to essentially go with the overall population exploitation rate rather than using some sort of -- The full selectivity at the fully-selected age or some weighted by age value, but, here, it's just all lumped together, all ages combined, where, obviously, the effects of the fishing mortality on different age groups -- The overall population dynamics are quite different, and the consequences are different, and that's number one.

Related to this, it's half question and half sort of a comment, but it is quite unusual to see a stock where the whole history of its exploitation, where you have more than a hundred years of data, and it seems to be a pretty good commercial value and of great recreational interest, we have never even approached the optimum levels of exploitation, and certainly the overfished and overfishing is even far above that.

Therefore, you normally would ask a question of how come, how could this have happened, and it seems to me that this is all hanging on, as Fred Serchuk said earlier, the natural mortality rates, and, specifically, for the younger ages, and so you have relatively high natural mortality on younger ages, and yet, because they grow fast, they are already entering the fishery, and so the natural mortality rates are dominating relative to the fishing mortality rates, and that is sort of the

driving sort of factor for the interpretation of what happens in the dynamics of the stock. Would that be the correct interpretation?

DR. LAURETTA: To answer your first question, why I chose that rate, it's purely consistency. As Rusty mentioned just now, there was a lot of deliberation during SEDAR 38, and I chose consistency as the go-to, and so we can look back. The one reason -- What I recall, and, again, this is six years ago, and more has happened than I have time to explain, but one reason to use exploitation rate is it's intuitive to people, when you're talking about a proportion of fish removed versus some sort of continuous F , which sometimes seems higher than a percentage and can vary by fleet, and those are important to look at, because, obviously, each fleet targets -- Well, different fleets target different sizes and ages, and most of them target the range.

The other reason why it seemed quite appropriate is that the fisheries don't really harvest the fish at any significant rate, besides the discards, until they start to mature, and so the target exploitation rate seems to be a good option, and not that we don't want to explore alternatives, but that certainly would push this into the realm of benchmarks. What I didn't want to do is come in here with a whole lot of alternative parameterizations, and that was actually the opposite of my objective.

These are, obviously, great recommendations, and we should make sure to get them on paper, but we do have to operate within the terms of reference, which have been posted, and I was very strict on that. I agree with you completely that natural mortality will have an effect on our estimates, and we know this. Decades of fisheries scientists have demonstrated it, over and over, and there was quite a bit of effort in exploring alternative natural mortality rates, as well as documentation on the justification of why we chose this one and the scaling rate we used and the reason why we thought it should be based on length, for example, versus an alternative constant.

The goal was, to some level, replication and update, and it was not for me to come explore as many different parameterizations as I could, but that doesn't mean that we shouldn't document them for a research track, but we do have to operate in the terms of reference simply to meet the objectives.

The fishing benchmarks, absolutely, those, again, were put in here for consistency. When I read through everything that was done previous, I marked those, and I went with those, and are there alternatives out there? Absolutely. As you mentioned, the fishery is never estimated to have been overfished or undergoing overfishing. If that doesn't match the state of the resource, then it could be an indicator that maybe your target is not where you want it to be, but, again, that's not a modeler's decision, and so consistency really was the key here.

We spent I think close to a year-and-a-half during SEDAR 38 talking about from data workshop to assessment workshop, and we had a really highly skilled expertise review panel, and so a lot of these decisions even came out during the review panel, based on the peer reviews, and I just didn't take the liberty to revise those, but, that being said, if somebody with a very expert knowledge of the system says that it was absolutely overfished in the past, then there's a good chance that the thresholds might not be where the stakeholders or managers want them to be, and I could let -- The committee maybe could respond better to some of that, but did I answer the majority of your question and comment?

DR. SHAROV: Yes, definitely, and thank you, and I understand that you were quite limited, in terms of what you wanted to do, and I appreciate your coverage. Thank you.

DR. LAURETTA: You're welcome, but we should absolutely document these, because a research track assessment would be the perfect place to explore alternative parameterizations, and I even wrote down several in the stock assessment report that were, after months of modeling the data, questions that came to my head that I thought should be documented, and so we should keep a running tally of those, so that we have that record, if we do move it to a research track in the future.

DR. SEDBERRY: Thanks, Alexei.

DR. NESSLAGE: Thanks, Matt. This is a great presentation, and I had a question about model convergence, and so, on page 29, you mentioned that the model gradient for the final run was 0.015, which is higher than the typical target for your max gradient, and so I'm just a little worried, because that's usually an indication that some of your parameters are correlated, and I was wondering if you had a chance to examine the file and whether you have any idea what parameters are correlated and how that might be impacting model convergence.

DR. LAURETTA: There is definitely some strong correlations in parameters, and, obviously, mean unfished recruitment is highly correlated with steepness, and it's sort of -- It's important to note that we fixed natural mortality, and we fixed steepness in the model, and so we're essentially asking it to give us that mean level, given these vectors, population dynamics, and so those are inevitable, but, yes, there's a whole lot of them.

It wasn't where I wanted it to be, but that was -- It was actually better than the gradient, and we were able to achieve prior, and it was so stable in its solution, even when I jittered it to a pretty far space and did some other manipulations to the model, and it came to a pretty consistent solution, and so I sort of wasn't as concerned about the gradient being above the threshold, because it was so consistent in its solution space. Even when I started it in many, many different parameter spaces, it went to the same one.

DR. NESSLAGE: Right, but I think that's a bug and not a feature, and so, if several of your parameters are highly correlated, then it's going to probably settle on a very similar solution, and so that might not be an indication of something good going on. It could be an indication that you can't estimate all the parameters that you're trying to estimate, and I'm a bit concerned about that.

DR. LAURETTA: Okay. Well, I would have to dive into the matrix itself to really highlight all the -- Yes, certainly some of them, the usual ones, and then -- But most of the parameters were well estimated. Again, it's sort of configured in a stable sense. Like I said, with natural mortality fixed and steepness fixed, it's automatically in a stable configuration, but it's still picking up the signal in the data.

Now, I can't tell you that there's not a -- Everything that I did to the model, I got it to the best solution, as configured, and, as far as is it above, I would say no, and it's possible, but I did a lot of due diligence in this model, and nothing gave me a red flag of what you're indicating, but that doesn't mean there's not something that I missed. I can tell you this, that it was a very good replication of SEDAR 38, in its both methods and results, but it was limited to that parameterization, and I didn't diverge from that really at all, as per the terms of reference, and so, while, yes, I probably could have configured it to get a lower gradient, I was a little bit concerned that I was sort of stepping outside the realm of the objectives of the update.

I mean, certainly we could dive into the covariance matrix, if the committee thought that would be really useful, and highlight those ones that seem to have high covariance on them and look for outliers, but I can tell you that my sense -- My modeling sense is that it was stable and responsive, and those are two promising things, but would you get a different answer with a different configuration? Well, yes, and it is SS3.

DR. SEDBERRY: Genny, did that answer your question?

DR. NESSLAGE: I think we'll probably want to discuss this more, but I'm good for now. Thank you.

DR. SEDBERRY: Okay. Thank you. Any additional questions before we look at the action items for this agenda item? I don't see any hands raised. We have several action items for this assessment review, and for all the assessment reviews, and it would be good for the SSC, for the committee, to have a response to each of these action items. I think Mike should be able to display the Google Doc as we talk about this, to make sure that we address the agenda items, but, then again, you can just look at the overview document in your briefing book, and you can see what they are.

Just starting off, our first action item was to review the assessment, which we just heard a great presentation on it, and I think everyone's clarifying questions have been addressed, but there might be additional questions that arise as we go through this, but the first question that we need to answer in our review of the assessment is does the assessment address the terms of reference to the SSC's satisfaction? Is there a general consensus that, yes, this SEDAR did address its terms of reference, or does anyone have any concerns about the terms of reference being addressed?

DR. REICHERT: I think they did. I mean, the terms of reference were, as Matt said, somewhat limited, but I think they were addressed in both the presentation and the report.

DR. SEDBERRY: Thank you. Does anybody disagree? I think the consensus of the committee is that the assessment appropriately addressed the terms of reference. The second question is does the assessment represent the best scientific information available? Again, it would be good to have a consensus statement on this from the committee. Is there anyone on the committee who does not think that this assessment represents the BSIA, or is there anybody that would like to have additional discussion on that question?

DR. REICHERT: I think it is important for us to say that perhaps within the terms of reference, because I think there were a couple of items that we discussed earlier, for instance such as the natural mortality that Jeff brought up and maybe some other items, that, given the current assessment -- If this would be an old benchmark, or maybe even a standard, we would probably make different choices, and so I think it's important for us to recognize the fact that best scientific information available given the terms of reference, and I would like to hear from some other members how they feel about that and whether we should mention those caveats here, or maybe later, under uncertainty or some other items.

DR. SEDBERRY: Mike is typing up, on the Google Doc, that statement, and you're right, Marcel, that this will come up again probably during the uncertainty discussion.

DR. NESSLAGE: I guess I am still a bit concerned about the model convergence issue, and I would love to hear some other people's thoughts, if they think I'm making too much of it. As a modeler, I would be concerned with that, but perhaps I am being overly cautious.

DR. SEDBERRY: A little caution is a good thing. Did you have something to that point, Marcel?

DR. REICHERT: Well, yes, to what Mike just said. I did not suggest this. I was just going to make a remark that we should include the terms of reference, and that's a caveat, in whatever we are going to say here, but I did not, at this point, make the recommendation to consider this best scientific information available. I am not quite there yet, because I would like a little more discussion on the effects of the terms of reference, and so I just wanted to clarify that, real quick.

DR. SEDBERRY: Thanks for clarifying that. Again, we're still drafting this as we speak, but good point, and I'm glad that you emphasized that and clarified that.

DR. SERCHUK: My feeling, Chairman, is the answer to that is yes, because, if the best scientific information available is that which was given in response to the terms of reference, I can't see that we could challenge that. Maybe we didn't craft the terms of reference to be more expansive, and maybe we didn't allow enough liberty on it, but that -- To me, that's the same thing as if the king were queen or if the queen were king, and the assessment addressed all the terms of reference, and the terms of reference were very explicit about keeping to the existing model, and so my feeling is it is the best scientific information available, in the sense that it fulfilled completely the terms of reference.

Any uncertainties we have could either be identified in the next bullet or in research recommendations, but I think we're being a little bit too harsh, when we've asked -- When we either crafted the terms of reference or accepted the terms of reference and then saying, well, they could have done more, and I don't think that's the case here. I would prefer that we just answer this yes. Thank you.

DR. SEDBERRY: Thank you, Fred. I'm inclined to agree with you.

DR. SCHUELLER: I don't disagree with Fred. I think that this probably is the best scientific information available, and this is the best data we have to make management decisions, and so I don't disagree with that. However, I do agree with Genny and her concerns. I think that -- I mean, in the document, there is one table listing initial values and estimated values.

In a lot of cases, those values are almost the same, and that's why I asked the question about the likelihood profiles on the selectivity parameters as well, because I think that those parameters are probably not as well defined as we might think they are, and so that might be where some of the issues are coming in, and I don't know. It's hard to tell, based on what is in the tables in the report, and so I do think that we need to have a full discussion on that and that we need to outline it as a concern.

DR. SEDBERRY: Okay, and is that a concern in regard to being the best scientific information available, or is that an uncertainty? I'm not a modeler, and so I don't know.

DR. SCHUELLER: It depends on what it looks like, in my opinion.

DR. SEDBERRY: Okay.

DR. SCHUELLER: I don't know. I mean, there's not a clear answer without actually looking at it in more depth.

DR. LAURETTA: If I could just add one quick comment to that. Amy, I think why you're seeing the agreement in the input values is because, towards the end of SEDAR 38, for convergence, they were sort of set, and the starting values were set at best estimates, and that's why the strong jitter became really important, to understand how the estimates would diverge under a pretty strong range of variability in those starting values, but, for convergence efficiency, I do believe the starting values were set at the previous assessment's estimates, and so if that might explain why they look so familiar, is because it's returning to that solution, even across the jitters. I don't know if that helps or not.

DR. SCHUELLER: Yes, and that's what I suppose, is that they're set at the SEDAR 38 values, and I have been requested to run a jitter on a more recent assessment that I was doing the review workshop, and I think the folks that were on my review panel would probably have taken issue with the jitter figure here, because it looks like, if they were around the review table, they would say there is three stable solutions here, and, again, I don't know what the likelihood profiles look like with some of these parameters, and so it's hard to know if this is landing in the right spot or not, and, again, that they're correlated as well. Anyway, this is one of those sorts of art of assessment type of things, too.

DR. REICHERT: I had a very time hearing Amy, and I'm not sure whether that was my audio or others had the same problem, and so that's just as an FYI. Relative to what Fred said, I agree with him, but I still would like to add the caveat of given the terms of reference, to that second statement, and then -- Given the fact that we will discuss some of the things in the next bullets, as Amy and Genny have started doing, then I'm comfortable with the second statement.

DR. SEDBERRY: I agree that we can keep the statement that Mike has typed there as a draft and as something that we might return to once we discuss the uncertainties. I understand Fred's point, but it doesn't give me any heartburn to say given the TORs, and it does go without saying, but, in this case, I don't think it hurts to say it again. Anne, if you're speaking, I can't hear you.

DR. COLLIER: Anne, if you would like to type your question in, I will read it to the group.

DR. SEDBERRY: Thanks, Chip, and, while Anne is typing, we can take a question from John Walter.

DR. WALTER: I want to just touch on the points about model stability, which Amy and Genny bring up really good points, and one of the -- On the jitters, one of the reasons that we show what the impact of not finding the global minimum in that jittering is because, quite often, in these models that have a lot of parameters, they can have some instability to initial starting values, and, in this case, what we found is, while there were other states that the model could find, other potential minima, it had almost negligible and undetectable impact on the actual results, and so, in that case, we probably could have tuned it further, to address some of the correlated parameters,

which are likely all of the Fs that have to get estimated, but the actual impact of that correlation was likely to be minimal, because, when we look at the SSB trajectories, and that's a figure that's in the actual report, in those jitters, and I believe it's Figure 4.2, on the parameter estimability of the selectivity parameters -- Ideally, in a full benchmark, we would have wanted to have had all of those parameters to profile across those parameters.

Right now, we had to rely on the standard errors of those parameters, or the CVs, and, when you calculate the CV, there was only really one parameter that had a CV of above 0.5, and that was the ascending limb of the tournament selectivity. It wasn't like the dominence of selectivity, which would have particularly given us pause about how those parameters -- Whether their inestimability would have had implications, and that's where this model is not perfect, and it has a lot of parameters, and there is a lot of room for improvement, but the question that I think is before the group is, is that inestimability a deal-breaker, or is it not likely to have substantial impact to the results, and I think I will just give those two pieces of information for the panel to consider, that Figure 4.2 and then the CVs, if you divide the standard error by the estimate of the parameters, and that's actually -- You would have to do the math, but it's also in the table in the report, and I just wanted to address what are valid modeling concerns. Thanks.

DR. SERCHUK: I don't mean to be a bug-bear, but I don't particularly like, given the TORs -- The fact is that something has come up here, in terms of what I believe, in saying, well, we should have thought of these TORs a little bit differently and given them a little bit more latitude, but they were interpreted very strictly, and my feeling is that you ask a group to address terms of reference, and, if they have addressed the terms of reference to the best of their abilities, and it's gone through a number of webinars and so on and so forth, then I think that the results that we've gotten here are robust, quite frankly, and I think the information in addressing the TORs is the best scientific information available, and, given the TORs there, we could put that on every assessment that we have, and so I think it's not needed, and, from my own personal point of view, it hints at, well, it could have been better, but they did address the TORs.

Every assessment and every evaluation that we get can certainly be done differently and so on and so forth, and I think it gives a negative ring to it. I will accept it, if that's the consensus of the committee, but I'm not happy with it. Thank you, Chair.

DR. SEDBERRY: Thank you, Fred. Those are great points, and I don't think we're at consensus yet, but I just wanted to have that in the notes right now as a placeholder, so that we make sure that, after we've discussed all the other aspects, that we're comfortable, one way or the other, that we can come to consensus about whether we should include that "given the TORs" statement, and I understand what you're saying.

DR. LANEY: To Fred's point, I mean, I think Fred is correct. I think that "given the TORs" language applies to every single assessment that we review, and so I guess I would say, Fred, I don't think it has a negative connotation at all. In this case, I think it's just stating the facts, and so, to me, it's elucidating, or articulating, the obvious, and so I don't have any objection to including it. Thank you, Mr. Chairman.

DR. SEDBERRY: Thank you, Wilson. As I said, we can come back to that BSIA consensus statement after we -- I think after we discuss all of the aspects of this, and we will word that in a way that makes -- That everybody can agree with, or at least we can come to consensus with, and,

again, I don't think this is that big of a deal, and we will -- This is not the final wording. We will come back to it.

Our next part of the review assessment is answering the question of does the assessment provide an adequate basis for determining stock status and supporting fishing level recommendations? In my opinion, that's just a yes, but I would like to hear from other committee members what they think about that.

DR. COLLIER: George, I did get a comment from Anne in the chat box, and she agreed that it is BSIA, and she also agreed with leaving in "based on TORs".

DR. SEDBERRY: Okay. Thank you, Chip. Thank you, Anne. In regard to the basis for determining stock status and fishing level recommendations, can we state that the SSC considers that the assessment does provide an adequate basis for determining stock status and supporting fishing level recommendations? You know, to me, "adequate" is not a very strong word, but that's the wording -- I guess that's the wording we're dealing with. Does anybody have any questions or comments regarding this statement? I don't see any hands raised. Moving on --

DR. COLLIER: George, Genny had a question or a statement.

DR. SEDBERRY: I'm sorry. Go ahead, Genny.

DR. NESSLAGE: We didn't really talk about, and maybe I missed it, but did we talk about how uncertainty is estimated, because that feeds into the fishing level recommendations, right? I am just curious, because, compared to many others here, I'm relatively new on the SSC, and they are using -- If I understand correctly, the asymptotic standard error is coming out of ADMB, basically, out of SS3 and not the full MCB-type uncertainty characterization that the Beaufort Lab does, and so I'm wondering how we reconcile management of king mackerel with much tighter confidence intervals relative to other fish that the council manages that use MCB-based uncertainty estimates, or is it a moot point? Thanks.

DR. SEDBERRY: Are you asking whether this is a management uncertainty rather than a scientific uncertainty? Is that what you're getting at?

DR. NESSLAGE: I guess maybe I can wait, if this isn't the right point in the discussion, but fishing level recommendations usually incorporate some aspect of uncertainty, right, in our -- The way we --

DR. ERRIGO: Genny, this is Mike, and I might be able to answer that.

DR. NESSLAGE: Yes, please. I'm floundering.

DR. ERRIGO: I understand what you're saying. You're saying that this particular assessment did not use the MCB analysis that would typically be used from the Beaufort Lab, and it's not as thorough of an analysis. The analysis here is not as thorough of an analysis. The question here is whether you think we can get stock status and fishing level recommendations, and they are run when you're looking at P^* , and there are questions about characterization of uncertainty, and, if you don't think the characterization of uncertainty is at a high enough of a -- As high of a level as

the MCB, you can downgrade your answer, and therefore reduce the P^* , in order to put in a larger buffer for the ABC.

DR. NESSLAGE: Brilliant. That is exactly -- I will hold my comment. Thank you.

DR. ERRIGO: You're welcome.

DR. SEDBERRY: Great. Thanks, Mike. Okay. Now we're identifying, summarizing, and discussing the assessment uncertainties, and so the first sub-bullet under that is review, summarize, and discuss the factors of this assessment that affect the reliability of estimates of stock status and fishing level recommendations, and we have a draft statement there that says there is an issue with the convergence gradient being too high, which raises concerns over correlation of parameters. Any additional discussion or comments regarding this statement?

DR. ERRIGO: I just want to say that I'm going to just put in like very broad statements, and then the SSC can go in with their notes and flesh it out.

DR. SEDBERRY: Right.

DR. ERRIGO: As long as you think my broad statements are correct.

DR. SEDBERRY: Right, and so the way we usually do this is Mike just does what he just said that he was doing, and then we'll get the notes from the notetakers this evening and tweak these statements with those notes, if that's necessary, and then the committee will have the chance to review this on our final day and, of course, review the written draft after the meeting. This is really just kind of the start here.

DR. SERCHUK: It seems to me, if we raise the comment in this first bullet, then we are going to have to come back to exactly -- For every comment we make in the first bullet, we will have to have a comment in the second bullet about the consequences of that with regard to a status of fishing level, and am I interpreting the sequence correctly? In other words, we say that there is an issue with convergence being too high, but we don't talk about what that means with respect to risk and consequences, but we will, and is that what you're -- I'm hoping we do that, and is that your concern as well, Chairman?

DR. SEDBERRY: Yes, and that's what we're trying to get here, is what are the scientific uncertainties in this assessment, and so one factor is this convergence gradient, and so what uncertainties does that create, and we do need to address that. We can't just say, yes, we think there's an uncertainty, but where does that uncertainty lie, and we need a little more detail about it.

DR. SERCHUK: My comment, Chairman, is that, if we say there's an issue with the convergence gradient being too high, and that may be the case, then, in the next bullet, describe the risk, that means we shouldn't believe the fishing level recommendations, and they're too high or too low, but we will have an accompanying statement that addresses what the issue is with respect to the risk, and is that correct?

DR. SEDBERRY: Yes, if I understand what you're saying.

DR. ERRIGO: Yes, you do need an explanation about what the risk is and if the uncertainty could cause large --

DR. SERCHUK: Exactly. Okay. I just want to make sure that we're going to follow-up with that in the next bullet below, in describe the risk and consequences. Thank you.

DR. SEDBERRY: Thank you, Fred. We are at the next bullet below, describe the risks and consequences of the assessment uncertainties with regard to status and fishing level recommendations.

DR. ERRIGO: Before we jump there, George, if I remember, early on in the presentation, there was a question about the mixing zone and mixing, and would the SSC like me to add that here, and is there any wording that you would like me to follow in regard to that?

DR. SEDBERRY: That was an issue that Church brought up, and he has his hand raised, and so go ahead, Church.

DR. GRIMES: I was about to bring that up again, because -- Correct me if I'm wrong, but, to the best of my knowledge, there are a couple of papers published on using otolith shape analysis, and also microchemistry, that say that, within what is now considered to be the winter mixing zone, and that is basically the Florida Keys, that there is temporal and spatial variability in that mixing zone, and so doing it 50/50 doesn't recognize that, and I don't know that it's a big deal at all. Probably, in your next bullet down there, you will say that it isn't -- It's not terribly consequential, I suppose, as that proportion of the total landings in the fishery in that mixing zone is not -- I don't think it's all that large, and so, anyway, that's my two-cents worth, but correct me if I'm wrong.

DR. SEDBERRY: I think you're right. In this bullet, we're summarizing the factors that affect the reliability, and, in the next bullet, we're describing the actual risk and consequences.

DR. REICHERT: I'm okay if that needs to be mentioned somewhere else, but Jeff brought up the estimate of natural mortality, and I had a similar comment when I reviewed the assessment, the choice or the selection of the Lorenzen versus Charnov, and we made that earlier remark, that that may have resulted in a different estimate of natural mortality and that was one of the research recommendations, and so that may come back later, but I think that would be one of the factors that may affect the estimates of stock status. Jeff, chime in if you disagree.

DR. BUCKEL: I don't disagree. I'm just thinking about, given the TORs, if this comes in here or is saved for the research recommendations section. I think it's okay to have here, given that it's a factor that can affect the stock status and fishing level recommendations.

DR. REICHERT: Thanks, Jeff. I was thinking about the same thing, whether this or down below would be more appropriate, but I just wanted to make sure it's captured somewhere, and we can move the text, if the committee feels more comfortable with that.

DR. SEDBERRY: Thanks, Marcel and Jeff.

DR. SERCHUK: I would prefer that this comment be placed down below, for this reason. When we do an update, we say don't fiddle around with things and use the same formulation and use the same approach, but just update it for the new survey data or for additional catch data, and I'm feeling now that, had we thought about it, maybe it would have been -- In any case, we need to think about the terms of reference, I think, with much more scrutiny, because I heard, time and time again, that, well, I looked at the terms of reference and they basically said keep things -- The model formulation, keep everything as it is and just update the assessment, and that's what was done.

Then here, about the assessment uncertainty, I agree that it's an uncertainty, but I think it belongs in the research recommendations, Chairman, and I think it serves as a flag to us to think a little bit more closely about, when we have anything about a benchmark, how much latitude we really want to give the assessment team in terms of fiddling, but I think this M, Lorenzen M, is -- I agree with the statement, but I think it was not -- I think it was considered that to do so would be violating the terms of reference, and that's what I thought I heard, or what I surmised, from the assessment presentation. Thank you.

DR. REICHERT: Since I brought this up, I'm comfortable with that.

DR. ERRIGO: I will put it when we get back down below to the research recommendation.

DR. SEDBERRY: Okay. That sounds good. I am looking at the clock here, and I had hoped that we could finish at least the sub-bullets under this bullet before we broke for lunch, and I don't -- I really don't think we can -- I think we still have a lot to do here to finish this up.

DR. ERRIGO: We might be able to get through the P* questions before lunch, and I don't know how long that might take though.

DR. SEDBERRY: We can try. I mean, it's not like anybody is traveling anywhere for lunch, but they may have had planned things to do, like take care of family and their lunches and lots of other things going on, and so, if it's okay with everybody, maybe we could try to continue on to get through the P* discussion. Would that be okay with everyone? Is there any objection to that? Just speak up.

DR. ERRIGO: What I mean are these four questions to get the P*. Hopefully that won't take terribly long.

DR. REICHERT: That means -- What you're saying is running it through the ABC control rule?

DR. ERRIGO: Yes.

DR. REICHERT: Okay. Thank you.

DR. SEDBERRY: You're suggesting that we skip down to that or just try to get to it?

DR. ERRIGO: Yes, I'm suggesting we skip down to that, because I think some of these other questions could be done overnight or by SSC members, to flesh out some of this stuff, and it doesn't necessarily have to be done right here.

DR. SEDBERRY: Okay. I think that's a good idea, and let's go ahead and do that, run it through the ABC control rule, and then we'll break for lunch and come back to the other questions, and how does that sound to everybody? Any objections to that? I don't hear any. Okay, Mike, do you want to run us through this?

DR. ERRIGO: Okay. The first one is the assessment information, and so the type of assessment, quantitative assessment, which provides estimates of exploitation of biomass and MSY-derived benchmarks, and the next category is reliable measures of exploitation of biomass, and you don't have MSY benchmarks, but proxy reference points, and then the one below that is you have relative measures of exploitation of biomass, but you don't have absolute measures, and you have proxy reference points, which I don't think applies here, and it just gets less implicit from there, and so reliable catch and then scarce or unreliable catch records. We are using SPR values, and so those are proxy reference points, and they're not MSY-derived reference points, but that's up to you. I just wanted to make sure that the SSC concurs with that.

DR. SEDBERRY: In terms of assessment information, this sounds like a two.

DR. REICHERT: I would agree with that, and that's consistent with how we have approached other assessments, because, for instance, steepness was fixed, and I think that was one of the reasons in other assessments why we went to 2.5, and so that is consistent with other assessments.

DR. SEDBERRY: Thank you, Marcel. Anybody else? Any disagreement with that? I don't hear any, and I don't see any hands raised. Are we ready to move on to uncertainty characterization? Okay.

DR. ERRIGO: All right. Genny, this might be the area where you would say the uncertainty characterization is not complete, or not high, perhaps, and you would give a lower score, but complete, key determinant, is that the uncertainty in both assessment inputs and environmental conditions are included. For high, it reflects more than just the uncertainty in future recruitment.

For medium, uncertainties are addressed via statistical techniques and sensitivities, but a full uncertainty is not carried forward in projections, but there could be some uncertainty carried forward, but it just doesn't carry forward all. Low, you have distributions of FMSY and MSY, and they're lacking. This just means they're lacking, and then none is you have point estimates.

DR. REICHERT: Since I'm not hearing anyone, I would go out on a limb and say -- I would recommend high. Genny?

DR. SEDBERRY: Thank you, Marcel. Let's hear what Genny has to say.

DR. NESSLAGE: Thanks. I am trying to process these definitions, and that's why I'm being slow. I am thinking that, given what we typically consider, full uncertainty being carried forward in the projections, I would say medium, because -- I don't know. I don't -- If we consider the MCB-based Beaufort-style uncertainty estimation to be the standard for the South Atlantic Council assessments, then I think it fits better in medium, myself. They did do quite a lot of sensitivities, and I really appreciate that, and they estimated the 95 percent confidence intervals, and that's great, but that's coming out a very tight -- It's coming out of the asymptotic standard variation in the

assessment, and there was no additional exploration of -- Unless I'm missing it, and that's why I'm also scanning the report, to see if I'm missing anything, but I guess my gut instinct, at first, would be to say medium, actually. Thanks.

DR. SEDBERRY: Do we have any other --

DR. REICHERT: I just wanted to say, because I recommended the high, or suggested the high, earlier, I agree with Genny, and so I'm comfortable with that.

DR. SEDBERRY: Do any other members have recommendations or comments?

DR. DUMAS: I am just trying to follow along here. What is this document that we've got up on the screen here?

DR. ERRIGO: I'm sorry. I didn't put it in the briefing book, and I probably should have. I forget that there's a lot of new folks on the SSC, and a lot of people have seen this many times over, and I can send this out. This is the ABC control rule as it stands now, and this is Table 1 from the ABC control rule, and it's how we calculate the P*.

DR. DUMAS: Okay. Yes, that would be a great to get a copy of that, just so I could -- I am not as familiar with applying this in the context that we're doing it now, and so I'm just a little -- It's taking me a while to keep up. One other thing. In our previous discussion of the TORs, where are the TORs actually listed? I have been looking through all of the information, and I can't find the TORs that you guys are talking about, and so I can't really tell whether this meets the TORs, because I can't find the TORs. Did I miss something, or where are they?

DR. ERRIGO: I did send them out to everybody in an email earlier, but I thought they were also in the assessment report.

DR. REICHERT: I don't think they are, Mike.

DR. ERRIGO: Okay. I might be mistaken, but I did send them out this morning in an email.

DR. DUMAS: Okay. I will check for that. Thanks.

DR. SEDBERRY: Sometimes the assessment reports have the terms of reference and how they addressed each one and sometimes they don't, and this is one that does not, but Mike did send that out, and thanks for the reminder, Mike.

DR. SCHUELLER: I was just going to say that I concur with medium here, for the reasons described.

DR. SEDBERRY: Thank you. We've had several suggestions of medium, and I haven't heard any objections to that, and so that gets a medium, Mike, 5 percent.

DR. ERRIGO: Yes, I've got it.

DR. SEDBERRY: Okay. Moving on to stock status, it's neither overfished nor overfishing, and it never has been overfished or overfishing, as far as we know, and so this seems to me like a no-brainer. Any comments? Okay. I think we are in agreement on that one then, Mike. Stock status is zero percent.

DR. ERRIGO: Yes.

DR. SEDBERRY: Then productivity and susceptibility.

DR. ERRIGO: Yes.

DR. SEDBERRY: Low, medium, or high?

DR. REICHERT: I tried to look up the PSA reports, and I couldn't find king mackerel in there, and so I am wondering if I have missed something or someone else was able to find any information that we can base our choice on.

DR. ERRIGO: One second. I am looking to see if I can find it.

DR. REICHERT: Okay. I didn't see king mackerel listed in the MRAG report, and also not in that NMFS report, or at least not a straightforward PSA value.

DR. CROSSON: Isn't this one we got into trouble with the PSA? I am seeming to remember that, last time we came around to meddling with the ABC for king mackerel, that the MRAG had something that we didn't like, that it put it into a risky category.

DR. REICHERT: That's why I was asking if anyone had any notes or remembered that, and I couldn't find that, because it's been so long ago, but I think you and I were both on the review, right, or on the panel.

DR. CROSSON: Yes, we were on the panel, and I see this as a low-risk species.

DR. ERRIGO: What year did we look at king mackerel? I could look up the old report.

DR. CROSSON: Oh, god. It's in an SSC report two or three years ago, but the whole committee ran into this, and we went -- The last time we adjusted the king mackerel ABC, we went through the control rule, and we looked at the PSA score, and I am pretty sure there's one in one of the PSA reports, and I thought the MRAG report, and we didn't like it, and this kind of caused problems for the committee, because we ended up applying a risk from the PSA score that we were uncomfortable with.

DR. REICHERT: I agree, and that's why I was asking if anyone had found some notes. For some reason, I was unable to find it.

DR. ERRIGO: I will tell you what. If you want to take a break, I can find it. I will be able to find the old SSC report, and I don't want to take up time now.

DR. REICHERT: Okay. I think that would be really helpful, so we're not running into that same problem. Do you agree?

DR. SEDBERRY: Yes, and so I'm going to suggest that we break for lunch now, and I know that we don't have to travel anywhere for lunch, but I still think that people had planned on having an hour-and-a-half for lunch, to take care of everything else that's going on with being stuck at home, and so let's meet back at 1:45. Anne, did you have something before we break? We still can't hear you, Anne, although it looks like we should be able to, but I just can't hear you. I'm not sure what's going on there. Okay. Let's recess for lunch, and we'll jump back on the webinar at 1:45. Thanks, everybody.

(Whereupon, a recess was taken.)

DR. SEDBERRY: Welcome back, everybody. I hope you had a great lunch. It wasn't Jim and Nick's, but it was lunch. So we're a little behind schedule, and we have a presenter scheduled to present the next assessment, but we wanted to finish up the king mackerel review before we move on, and so this is where we were, I believe, looking at the productivity and susceptibility and trying to decide where we were on that. Is that right, Mike?

DR. ERRIGO: That's right, and I found the PSA, the MRAG PSA, for that, and it was high, and so that gave a score of three for Tier 4, with a penalty of 10 percent. This configuration here gives the exact same P* that you got last time, and so the adjustment is 17.5 percent, and the P* is 32.5 percent.

I took the liberty of putting in a couple of lines here, and the SSC can look them over and change them, delete them, whatever you want, but it kind of goes along with the discussions that were had the last time that we reviewed king mackerel, which was in October of 2014.

DR. REICHERT: I looked it up, and we did discuss that MRAG value, but it didn't make it into the notes, or the detailed discussion didn't make it into the notes, and I would like to mention that the MRAG PSA score came out of the Gulf of Mexico, because it was not -- I don't think it was listed in the South Atlantic, and so that's a caveat there, and we did discuss that, at the time, we felt that that was a little high, but we also just discussed the ABC control rule, and I think the consensus was that we should not deviate from a control rule that we had literally just finished discussing, and so I just wanted to give a little bit of background for those that were not part of the SSC at that time and jog the memories of those who were, and, if people are interested, I can -- There is some discussion in the minutes of that October 2014 meeting.

DR. SEDBERRY: So I think we can still agree that this is not a high-risk stock.

DR. ERRIGO: The question is do you want to deviate from what the MRAG PSA score is, deviating from the control rule, which you can do, if you justify it, and I think you probably have justification to do it, or do you want to stick with the control rule, noting that you are addressing this issue during the ABC control rule amendment development and that, the next time this comes up, this issue should be resolved? If you do change the score, I need to know what you would like to change the score to.

DR. SEDBERRY: What is the pleasure of the group? Does anybody have any thoughts on this? Does anybody disagree with the wording that Mike has on the Google Doc?

DR. REICHERT: If we do, we need to come up with another score. Otherwise, we need to change the text in the SSC concurs with the MRAG PSA findings.

DR. SCHUELLER: I am sorry if I missed this, but the bullet that's at the top of that page says the SSC does not concur -- It says, especially since the MRAG PSA score came from the Gulf of Mexico analysis, and I don't know what that means, and so the parameters in the Gulf of Mexico differ that greatly from those of the South Atlantic? My knee-jerk reaction is that it can't be that different, and so I think we need to be a little bit more clear there if we're going to say that.

DR. ERRIGO: It was simply noted that this MRAG score came from the Gulf of Mexico analysis, and so I noted it here, and I could change that wording to say that, or I can remove it completely.

DR. SCHUELLER: I think we just remove it completely. I mean, if we just don't agree that the PSA score is high, just state that and then give a reason why we don't and what the score should be. The reason why should be related to the stock itself and nothing to do with what anyone else has done.

DR. REICHERT: Amy actually made my comment, but I just wanted to mention that the MRAG score did come out of the Gulf of Mexico, just as a reminder, and so that was all.

DR. SEDBERRY: It's just a note that you're making and not an explanation for why we don't --

DR. REICHERT: Correct.

DR. SEDBERRY: Okay. Right.

DR. REICHERT: Because someone with more knowledge about the species may tell us that there may be some differences between the two regions, but I'm not sure that that's the case, but I just wanted to make a note of that.

DR. SEDBERRY: Understood.

DR. SERCHUK: I certainly don't agree with the Tier 4 score that says that this is a high-risk stock, and I think we have an obligation to base our evaluation on the stock assessment we have in front of us, and I don't think that's so difficult, particularly if the existing valuation of risk comes from the Gulf of Mexico, and so I know that, up above, we have Tier 4 as three, a high-risk, but there's no way that this is a high-risk stock, and it's never been overfished, and overfishing has never gone, and we have good recruitment, and why can't we just proceed with coming up with our own score?

DR. SEDBERRY: I don't think there's any reason why we can't come up with our own score, and those three reasons you just gave could be a justification. We can come up with our own score, we just need to be able to justify it.

MS. LANGE: That's my suggestion as well, and, relating to the second bullet, "However, the SSC does not wish to deviate from their established ABC control rule", I didn't think the actual score per species, or per stock, was part of the rule. The rule is that we're going to look at that Tier 4 and one of those three options, and so I don't see that we're actually going against that. We're looking at the same rule, and Tier 4 --

DR. ERRIGO: In the actual ABC control rule, it says that the productivity and susceptibility is gotten from the MRAG PSA analysis.

MS. LANGE: Okay, and so that's where it specifically says that.

DR. ERRIGO: Yes, and it's in the language in the control rule, but you can deviate from that.

MS. LANGE: Okay, but, again, as Fred said, the stock has never been overfished or having overfishing, and so it doesn't appear that it is highly susceptible.

DR. SEDBERRY: It seems very strange, to me, to label this a high-risk stock. We have a pre-recruit index of abundance, which relates well with the recruitment to the fishery and the landings, and that index is good. There is fluctuations, but, as everyone has said, it's never been overfished or undergoing overfishing.

DR. REICHERT: I don't necessarily disagree, but we need to be very careful with our language. Overfishing and overfished is already captured under another tier. If you look at the PSA scores, those are not elements that were part of coming up with that PSA score, and so I think, if we want to deviate from the PSA score, I think it would be good to look at the elements that go into the PSA score and base it on that. I just want to make sure that we are not double counting, because that's one of the issues we've had with the ABC control rule, and so we should be careful with that, because now we are justifying changing the PSA score on something that we have already accounted for in our ABC control rule, and I would like others who were part of developing that ABC control rule way back, other SSC members, to chime in and let me know if they have potential heartburn over that or not.

DR. ERRIGO: Marcel, I agree that you do not want to double-count, but the tier here that asks for overfished or overfishing, Tier 3, is the current stock status. What I think people are using to justify here is over its entire history it has never been overfished or undergone overfishing, and the PSA is its susceptibility to becoming overfished, and so, if it's never been overfished, then how could you say that it has a high susceptibility to becoming overfished?

DR. SEDBERRY: That's right, Mike, and my perception of the discussion here has been that we're talking about the history of the fishery and not just the current stock status, for this particular bullet.

DR. REICHERT: Okay.

DR. SCHUELLER: Every time we talk about these PSA scores, I don't remember what is typically included in them, and it seems to me what I always think is included in them is life history stuff, and what is included in the decision-making for the PSA? Could somebody remind me of that?

DR. REICHERT: I have it in front of me, and sorry to butt in.

DR. SEDBERRY: Go ahead and butt in.

DR. REICHERT: Productivity, age at maturity, size at maturity, maximum age, maximum size, fecundity, reproductive strategy, and trophic level. Then there is a productivity score, and that's availability, encounterability, selectivity, post-capture mortality, and the scoring of those elements come up with a susceptibility score and an overall risk score, and so you've got a productivity score and a susceptibility score, and that creates an overall risk score, and that's then ranked into high, medium, or low.

DR. SCHUELLER: So I guess I do have a question, and maybe this will lead the discussion in a different direction, and I guess I will apologize upfront for that, but productivity is often discussed with respect to steepness, and steepness, in this assessment, is fixed at 0.99, and I don't know, but, if you look at the likelihood profile, it doesn't necessarily make sense, and then, if you look at the likelihood profile from SEDAR 38, it makes even less sense, and I was trying to find the sensitivity runs, and I thought there were some sensitivity runs done with different steepness values in here, but maybe I'm getting it mixed up with one of the other assessments, but I can't find any of the plots, and so I think that it would be nice if we could see those, because I think it should inform the uncertainty related to that, and I'm just not sure that the wisest decision was made related to that parameter. If Matt is on still, maybe he can talk about that steepness decision during SEDAR 38.

DR. LAURETTA: That's a really good point, Amy, and excellent observation. The sensitivities were done across ranges of steepness, because I had the same thoughts that you did, that, when you pull the profile, it appears that the parameter is estimable within SS3, and that was the case in SEDAR 38 as well, and that was the original configuration of the model presented to the expert review panel.

The discussions that occurred there really focused on the estimates of recruitment in relation to spawning biomass, which showed a very large shotgun scatter pattern, and that led them, from the best of my recollection, to say why are you fixing steepness, when any line would fit through this pattern, or why are you estimating steepness, when any line would fit through this, and you would be better to fix it at 0.99. To my recollection, that is how the discussion went at the expert review panel, and, subsequently, the model was then completely revised to give the advice off of a steepness of 0.99.

I mean, the assumption is there that the recruitment will stay at that sort of constant, even as biomass drops, and that's what we projected forward. Now, if you look at all those runs, on the other hand, the sensitivity runs, and I have it on a slide, and I did put it in supplemental material in that presentation, and I don't think the profile is there, but the sensitivities are, and I could bring those up, but, essentially, it doesn't change your current determination of stock status.

I did a range from 0.4 steepness, which is pretty low productivity, all the way up to the one assumption, and we still find ourselves in the state, and what does change, on the other hand, is, if you use a steepness of 0.4, the target F rate is 0.09, and so it drops considerably, and it shows you that, at one point in the history of the time series, right around that 1998 lull, that it did cross the threshold. That's if you take the MSY benchmark that was done in SEDAR 38, but it sort of

changes your magnitude of depletion, for sure, and I think it does cross the threshold at a steepness of 0.4, but it still -- Biomass still builds in the current two above it, and so it doesn't change your current determination, but it does change your perception of where you were in the past and, obviously, what future recruitment will be.

DR. SEDBERRY: When it did dip below that threshold, it would have come right back.

DR. LAURETTA: Yes, and it just hit it. Right. It hit the target at that lower productivity. That's right.

DR. ERRIGO: Is this the slide you were referring to?

DR. LAURETTA: Yes, that's correct, and so the top one is the profile of R_0 , and it's showing that base model does provide our maximum likelihood estimate for our unfished average recruitment. The bottom panel is what the stock trajectory would be under alternative assumptions of steepness, and so running the model with fixing it at the range by 0.4 to 0.95, and you can see we get the same sort of depletion, except how far you went down in the recent period when the data started, and that's the real difference.

DR. SERCHUK: I would like to look at this from a life history point of view, Chair. This is a fish that matures very early, age-two, and is fully mature, and it has a relatively modest longevity, to at least age-twelve and perhaps more, and I don't see anything in the size compositions or the age compositions to say there is any evidence of truncation, which you normally would see to be susceptibility, and, based on the life history characteristics, and the age and size compositions, to me, this seems like a low-susceptibility stock, in terms of our low vulnerability, and it's not as if it matures at age-eight and only lives to age-ten, and it's not a shark species that has a very low productivity, because of body size, and we still see lots of individuals in the population, females, of large length that are highly fecund, and they are contributing. I can't see giving this a high risk productivity and susceptibility score. I just cannot. Thank you.

DR. SEDBERRY: Thanks, Fred.

DR. SCHUELLER: I mean, I don't disagree with Fred on the productivity discussion. I think, if you base it on life history, it seems pretty low risk. My question was more back at that steepness figure. It says fraction of unfished, and so is that an exploitation rate over the exploitation rate at which the threshold --

DR. LAURETTA: It's relative spawning potential depletion, and so your 0.3 SPR would be in the dashed line, and it would sort of, obviously, start off at an unfished state at one, and so it's the relative spawning depletion.

DR. SCHUELLER: Okay. I just wanted to make sure that I had that right, and you mentioned that, the lower the steepness, the more likely it's fallen into a range of being overfishing in that late 1990s phase.

DR. LAURETTA: That's right. Just to clarify, the FMSY at the steepness estimate, at the best estimate, around 0.6, was about 0.09, and so considerably lower than the 0.14, and, if you think back to that time series, where we got up to close to 0.11, there would have been that period in the

late 1990s of overfishing, and it would have pushed it towards the threshold, and then, as fishing pressure declined, it sort of bounced back above it.

DR. SCHUELLER: I am just trying to wrap my head around that steepness decision, because it's not really congruent with how it would have been handled for the other species that we look at.

DR. LAURETTA: Amy, if it helps, in the addendum to the SEDAR 38 report, that's where the discussion was held between the expert reviewers and the analysts, and I think they did a pretty good job of documenting that decision. My recollection, from being in the room, is there was no evidence for a stock-recruitment relationship, and we sort of invoked one when the reviewers didn't see it, and that was the main argument, but I do think you could refer to that addendum section, which has the explanation of why the model was changed, per advice.

DR. SCHUELLER: I will take a look at it. I already pulled up that report. Thank you.

DR. REICHERT: I think most of my points were made by others, and the only other thing that I wanted to mention is that the current -- We mention it in our minutes, and the overall risk score is just above medium for king mackerel, and we noted that in our previous -- In the minutes in the October discussions. I agree with Fred about the productivity aspects, and the other element of that PSA score was the susceptibility, and there is some reason to change those also from the values that were given in the PSA score in the Gulf of Mexico. For instance, I think the post-capture mortality was high, and I don't believe it is very high, and so there are some other reasons why we should deviate from that PSA score.

DR. SEDBERRY: Thanks, Marcel.

DR. ERRIGO: Correct me if I'm wrong, but I think that the fishery in the Gulf of Mexico is under different regulations, and I think they have more gillnetting and things like that we do here, which would change post-capture mortality and some of the other susceptibility parameters.

DR. REICHERT: I agree.

DR. SEDBERRY: Regarding the bullet there about no evidence of age or size truncation, maybe I'm confused, but, when I look at Figure 2.9 in the stock assessment report, it seems to me that the age frequencies that are given there for the handline fishery does show age truncation in more recent years. Is that real, or am I looking at this wrong? It's Figure 2.9 on page 21 of the stock assessment report.

DR. ERRIGO: Isn't that a function of selectivity of the fleet? Didn't we say that it's dome shaped and they were capturing the larger individuals, purposefully?

DR. SEDBERRY: There may be an explanation for it that I've just forgotten, and that's what I was asking, is what the explanation for that is.

DR. LAURETTA: I can maybe just comment quickly. We're talking about the age truncation in the data, and is that correct, 2.9?

DR. SEDBERRY: Right.

DR. LAURETTA: I had thought that was quite suspicious myself in the most recent years, and so what I did is I looked at the distribution of biological samples coming in, and I saw a clear truncation, such that -- It's mostly from Florida east coast, and it's been about 50/50 North Carolina/Florida east coast, up until 2016, and then the North Carolina samples dropped for our previous discussion, and so there's definitely a sampling change that occurred there that led me to think this was awfully suspicious, where all the older fish went, and I think it's related to that lack of sampling in North Carolina, is my best guess.

DR. SEDBERRY: That's right. We had already talked about that, and I just forgot about it. Is that all we have to say about the difficulties in applying the control rule?

DR. BUCKEL: I think everybody is onboard with going with the lower score here for Tier 4, but I just wanted to add one other piece of evidence and address Amy's question, with her concern about the steepness. Just to note that this period of recent high recruitment happened at fairly low biomass levels, and so it does support the high steepness that you're getting, and I think Matt may have said it was the highest or second-highest recruitment in recent years, and that's coming at a time when the biomass isn't super high, and so it would support the higher steepness.

MR. BROWN: I just wanted to add -- Just to clarify, in case it was lost, and you can also find it in the assessment report, that, because of this disappearance of the ages from North Carolina, Matt did not use those ages for that 2016 to 2017.

DR. SEDBERRY: Okay. Thanks for that clarification.

DR. ERRIGO: I think we're good with your justification. The one thing I need now is an actual score. Do you want to go with low or medium?

DR. SERCHUK: I suggest, Chairman, that we go with low. Thank you.

DR. ERRIGO: Thank you, and you had suggested that previously, and so that's -- There is one suggestion out there.

DR. CROSSON: I concur with Fred.

MS. LANGE: I concur as well, and, again, the final bullet that says it's going to be looked at soon, and we may change it, but, at this point, low seems appropriate.

DR. SEDBERRY: Thank you.

DR. SHAROV: Same here, based on all the discussion for half the day, and I agree.

DR. SEDBERRY: Thank you, Alexei. Are there any objections to setting this as low? I see no hands raised. Then low it is, Mike.

DR. ERRIGO: Very good. I just changed that, which makes the adjustment factor 7.5 percent and the P* 42.5 percent.

DR. SEDBERRY: Okay. Thanks, Mike. Now, what do we need to do here to finish up our review of king mackerel? Are we -- We don't need to discuss rebuilding.

DR. ERRIGO: I think research recommendations. We don't need to discuss rebuilding, and that's right. It doesn't need to be, and I think the research recommendations we can do by -- When we're doing the report, and we can fill that in. A lot of people had good ideas there.

DR. SEDBERRY: Right, and we captured several along the way, and we just need to make sure that we have those in the notes and to add them to this.

DR. ERRIGO: Yes.

DR. SEDBERRY: Okay.

DR. ERRIGO: Advice on monitoring, does anyone have any ideas on this? It's pretty standard. I mean, we can do that after the fact also, just to get moving, because we do have another assessment we've got to get to.

DR. SEDBERRY: I would agree with that. I don't think we need to -- We can look at that as we put the report together from the few things that we've mentioned regarding SEAMAP and some other things, and then we can send that out for review by the entire committee, to make sure that we've captured everyone's ideas.

DR. LANEY: I just wanted to run a habitat suggestion, I guess it is, or question by the SSC. With respect to habitat considerations for king mackerel, Steve and I had some offline emailing back and forth, and it does appear that it is largely a pelagic offshore species, although, as somebody else pointed out, "offshore" is a relative term here, and so oceanic maybe would be a better way to put it, and it -- Obviously, the stock seems to be in pretty good shape, and we've had really good recruitment recently, and so I was asking Steve if -- It seems that maybe the only habitat factor that we would need to be concerned about in the future would be just to keep an eye on what the temperature is doing, from a climate change perspective, and I believe, and George and Steve can correct me if I'm wrong, but I think king mackerel is on the list of those species that we are currently doing a climate vulnerability analysis for, and so we should have the results from that analysis sometime later this summer, and so maybe just a research recommendation to consider the results of that climate vulnerability analysis with respect to king mackerel is a research recommendation, and it may mean not a whole lot, because it seems to prefer warm water anyway, and so it may benefit, and who knows. Thank you, Mr. Chairman.

DR. SEDBERRY: Thank you, Wilson, and king mackerel is included in that climate vulnerability assessment, and so we'll see what happens from the report on that activity, which should be coming out soon.

DR. REICHERT: Two notes that we can deal with in the report, and one is that normally the SSC makes a recommendation when the next assessment should occur, and so maybe we can think a little bit about that, and I would put a comment in the report, and the other thing is, because of the nature of the review, we usually try to shoot holes in the review, and so I think it would be good to add some language to commend or thank Matt for the extensive and thorough presentation and

the report, and those are just two quick comments I have, and those can all be addressed via the report.

DR. SEDBERRY: Thank you, Marcel. Two very good points. Thanks.

DR. GRIMES: I was just going to add that, to the monitoring, you might want to insert to monitor in the mixing zone, in the winter mixing zone, the relative contribution of Atlantic and Gulf stocks.

DR. SEDBERRY: So some kind of mixed stock analysis for the winter mixing zone. I'm sure Mike can come up with some words there.

DR. GRIMES: I was thinking more of just actually sampling and monitoring.

DR. SEDBERRY: Determine the relative contributions.

DR. ERRIGO: That might be a research recommendation rather than a monitoring.

DR. GRIMES: Well, it's actually been done before, and it just -- But it's been shown to be variable spatially and temporally, and so, if you don't monitor it -- You can't do it once and have the right answer.

DR. SEDBERRY: Right. Any additional recommendations to add to the list now? Again, if some things occur to you overnight, send them along, and we can add them in and revisit this when we look at the -- When we give a final look to our consensus statements and recommendations and also when we review the draft report.

DR. SCHUELLER: Matt had brought up looking into the SEAMAP index, with respect to age-zero versus age-one, but it seems to me, whether it's covering one age or the other, it's an early indication of if there is low recruitment, and so it seems to me that should be something to watch. That index could be watched, in case there is a low recruitment class coming in, because it certainly seems like fishing is following that recruitment pulse, if I was going to say what I think is happening, and so that's just my suggestion.

DR. SEDBERRY: That's a good one, and I don't know what SEAMAP funding is looking like, but I certainly would like to see that program continued. Any other suggestions at this time?

DR. BUCKEL: I was just looking back at the notes, and I'm a notetaker for this, and one -- With respect to Church's comment on monitoring the mixing zone, I had a note to examine the model sensitivity to changing the 50/50 ratio in the mixing zone, and so maybe that's going to be a research, but it could be a qualifier. If the model is not sensitive to it, because, as Church mentioned, maybe the landings are so low that it doesn't matter if it's 100/0 or it swings the other way, to 0/100, but it just would have very little impact on the assessment results, and, if that's the case, if it doesn't have an impact, then there wouldn't be a need to monitor the mixing zone, and so maybe, if the model is sensitive to the ratio, monitor the mixing zone, for that second bullet that you had there.

DR. SEDBERRY: We are typing that up as a separate bullet, but we can look at the wording of that and see if it needs to be like an add-on to that previous bullet or if it should stand alone.

DR. BUCKEL: Thanks.

DR. ERRIGO: I am putting it under research recommendations to investigate the model sensitivity, and that's a research project. I can add that in, that, if the model is sensitive, then monitor the mixing zone.

DR. SEDBERRY: Very good.

DR. BUCKEL: Excellent.

DR. SEDBERRY: All right. Mike is doing that right now, and anything else? Have I forgotten anything important, or are we ready to move on to the next assessment? It looks like we're ready to move on.

DR. ERRIGO: Great, and I will fill in this table when I get the chance. We have Kevin Craig on the line and ready to go.

DR. SEDBERRY: Okay, and so this Agenda Item Number 6, and I have Churchill, Anne, and Tracy as notetakers for this agenda item, and there is two attachments associated with it, the SEDAR 59 assessment report and the presentation that you're about to hear from Kevin Craig. Again, we are asked to review this assessment, just like we just did for king mackerel, and so we'll kind of follow the same procedure. We'll have the presentation, and we can ask clarifying questions during the presentation, but let's not get into a huge discussion. Let's just see if we can stick to questions that help clarify the presentation, and then we can take public comment, and then we'll have a further discussion and address the action items after the public comment. I think we're ready to go if Kevin and the system is ready.

SEDAR 59 GREATER AMBERJACK ASSESSMENT REVIEW

DR. CRAIG: This was a standard assessment for South Atlantic greater amberjack, and the last assessment was a benchmark that was done as part of SEDAR 15, which had a terminal year of 2006, and so it's a fairly old assessment, a little over ten years old, and, at that time, greater amberjack were not overfished and not considered -- There wasn't indications of overfishing, and you can see the status indicators from SEDAR 15.

In the intervening time, there has been a change in the benchmark, and we're using 75 percent of SSB MSY for MSST, as opposed to one minus M times SSB MSY, and so that's the benchmark for the current assessment, and, just to kind of cut to the chase, the results from SEDAR 59 are in line with those from SEDAR 15, in that we don't see a lot of indication of overfishing occurring or that the stock is in an overfished state, based on the assessment, and that seems to be because the landings, with total removals, for greater amberjack have remained relatively constant since the end of the last assessment, perhaps with a bit of an increase in the last three or four years, but we've also had -- Our assessment is picking up higher than average recruitment in the mid-2000s, which is leading to the higher spawning stock biomass, and the status indicators are also suggesting there is not any overfishing or it's in an overfished state.

That was the main result from the assessment. For the presentation, I have outlined it this way, and so I will go through the data review and update, and I will show the catch-age model, and so this is the BAM assessment model, and I will go through the base run of that, the uncertainty analysis, both the sensitivities and the ensemble modeling, as well as the projections, and, if there are questions, I can try to check the chat box periodically, but, after each one of these sections, I have put in a placeholder, so that we can stop after each one of these, if we need to go back or if folks have questions.

DR. SEDBERRY: That sounds like a good plan.

DR. CRAIG: Okay, and, if I miss something on the chat box, George, feel free to jump in, or, if somebody has a question as we go along, feel free to jump in. Stock definition is greater amberjack are a globally-distributed species, and they are found in sub-tropical waters and tropical waters throughout the world. For the purposes of this assessment, the U.S. distribution in the South Atlantic extends from the Florida Keys to the North Carolina/Virginia border, and so those are the stock boundaries, and they are essentially the jurisdictional boundaries, and that's unchanged since SEDAR 15.

In terms of the regulations, the major regulations have been a thirty-six-inch minimum size limit for the commercial fleet and a twenty-eight-inch minimum size limit for the recreational fleet, and both of those were in place in 1992, and they played a role in this assessment, because, during the assessment process, we didn't see a lot of indication that that size limit was having an effect on the size composition of the landings, and so that affected the structure of the assessment model, and I will talk a little bit more about that later. There has also been some bag limits that are pretty specific to the April timeframe, when spawning is occurring, as well as some commercial trip limits.

This shows the data available for the assessment, and I would characterize greater amberjack as a fairly data-poor species, and you've probably seen this sort of a table before, but the top panel shows the landings that you have for headboat, general rec, and commercial. They go back to 1946, which was the start year for the SEDAR 15 assessment. Discards, we have for headboat and general recreational that are hindcast from the early 1980s, and no commercial discards for the line fishery, and then the other thing that is worth pointing out is the composition data.

The solid colors show years where length comps are available, and the shaded colors show where there is also age compositions, and so the age compositions are fairly spotty, and so, for headboat, we have age compositions starting in 2006, 2009, 2010, and then a couple of years after that. Then, for general rec, similarly, we have a series of years in the early 2000s where we have some age comps, and then a few at the end, and then commercial handline probably has the best coverage, in terms of age compositions.

This question-mark indicates a couple of years where we were on the margins for what would be retained, in terms of a sample size, and we did end up retaining those, and I think these were compositions that had eight trips, and one had twenty-nine fish, whereas we typically use a ten-trip cutoff and a thirty-fish cutoff, and so the compositions exist across these fleets, but they are fairly limited.

For the indices, we had three indices, a headboat index, which extended from 1980 to the terminal year, 2017, commercial handline, which starts in 1993 and extends to 2017, and then one of the TORs was to consider the SERFS video index, which extends from 2011 to 2017. One of the first things that we discussed as a panel was the start year, and that discussion led to a decision that we alter the start year of the model, and, again, the model in 1980, as opposed to 1946, which was the start year for SEDAR 15, and that was because there was fairly uncertainty in the landings and discards prior to 1980.

The only real data prior to 1980 is shown here, and so this is the commercial landings, and everything else would have been hindcast or estimated via a ratio or model-based approach, but the only real data prior to 1980 is commercial landings, which, during that time period, they account for around 2 percent of the total removals that have been observed over the assessment period.

We did change the start year, and then the recommendation was to look at the effect of that via a sensitivity analysis, and so we did run a sensitivity analysis with the original start year, which I will show later, but the net result was it had very little effects on the assessment, and so, instead of picking a year and assuming a virgin condition, starting the model when the data actually starts.

We did update a number of the life history parameters and also kept some from SEDAR 15, and so we updated the growth curve, we updated the maturity schedule with samples that had been collected since 2006, and we updated the natural mortality vector as well. In SEDAR 15, a Lorenzen age-based mortality vector was used, and updated that to Charnov, which has been used in a number of recent assessments, and it's based on a more recent meta-analysis that includes those studies that were part of the Lorenzen meta-analysis.

The remaining life history information stayed the same as for SEDAR 15, and so discard mortalities were at 0.2 for all the fleets, and peak spawning is assumed to occur in mid-April, and there's a 50/50 sex ratio, and then we modeled ages-one to ten, with ten being a plus-group, and so those are all consistent with what was done in SEDAR 15.

This shows the growth curve, and so there's a von Bertalanffy growth curve, and we used a Diaz correction to account for the effects of the size limit on the fishery-dependent samples, and it was inverse weighted by sample size. You can see the von Bertalanffy parameters on the right, and the top one is the current, or the updated, parameters for SEDAR 59, and then the bottom shows those that were used in SEDAR 15, and so not a very large change in the growth curve, but the thing that did want to point out, because I think it bears on some of the decisions that we made later regarding the length compositions, is the really high variability in length at-age of greater amberjack.

This occurs for a lot of species in the South Atlantic, but I think amberjack seem to be an extreme example, where you have a 600-millimeter fish that can essentially be any age, from one to ten years old, and so there's a lot of variability in size-at-age, and that was one of the contributing factors that I will explain a little bit more later, as to why we ultimately removed the length compositions from this assessment and just relied on the age compositions.

At maturity, we updated that from SEDAR 15, and you can see the SEDAR 15 curve shown here is the solid line, and then the updated curve for this assessment is the dashed line, and so we are

assuming a younger age at maturity, and so about 50 percent of one-year-olds are mature in the current assessment, versus around 15 percent or so in SEDAR 15, and that seemed justified, because the updates in the sampling, or the additional sampling, really increased the coverage of these younger ages, and so, in SEDAR 15, there were only 66 fish that were less than two years old, and so that's informing this part of the maturity curve, whereas the updated data had 342 fish at those younger ages, and so a younger age at maturity compared to what was used in SEDAR 15.

Again, as I mentioned, spawning occurs from January to June, with a peak in mid-April, and, as the measure of reproductive potential, we used the mature female biomass. There was some information on batch fecundity, but it's fairly limited, and so we stuck with what was used in SEDAR 15 and went with mature female biomass.

Mortality, natural mortality is on the left, and so this was updated to the Charnov age-based natural mortality vector, which you can see in the red here, versus Lorenzen, which is in the blue, and so we're assuming higher natural mortality at age, and that was scaled to give about 1.5 percent, or 1.5 percent survivorship, to the maximum age, which is seventeen years old, following the methods in Hoenig.

The discard mortality was the same as from SEDAR 15, and so 0.2 for both the recreational and commercial sector, with a range of 0.1 to 0.3, and that was primarily based on literature reviews that were available at that time.

One of the other changes that we made that was a departure from what was done in SEDAR 15 was the fleet structure, and we basically simplified the fleet structure, and I think it's more commensurate with the type of data that we have available for the assessment, and so, in SEDAR 15, there were separate commercial dive and commercial line fleets and separate general recreational and headboat fleets and then separate discard fleets associated with each one of those, and so, in SEDAR 59, we pooled all of the commercial gears into a single commercial fleet, and we did the same with the recreational fleet, and so we had the charter boat, private, and the headboat data combined as a single recreational fleet. Then the discards were modeled separately from their landings, but combined in the same way as the landings.

I will go a little bit into sort of the reasoning behind that, and so this shows the proportion of different gears that made up the commercial landings, and you can see the orange is basically handlines, and there is a few years where there is some longline, shown in red, and then the dive landings are shown in black here, and so these are what was modeled as a separate fleet, but they reflect about 2 percent of the total commercial removals. It's also pretty limited composition data, and it's estimated selectivity for the dive fleet, and so we only had twelve trips, and about sixty-six fish, and, just looking at the limited length compositions, they were fairly similar to handline, and so, given that, and the really small amount of total commercial removals, we pooled dive gear as well as other gears into a single commercial fleet.

We did a similar thing for the recreational fleet, and so, again, headboat is a relatively small portion of the total recreational catch, and it's been about less than 4 percent of the recreational removals in the last ten years or so, and you can see that in this figure here at the bottom, where the dark orange is the general recreational landings, the sort of yellow-orange is general recreational dead discards, and then the blue is the headboat landings, and the headboat discards are on here, and

you can't see them, because they are small, and so you can see, over the time series, headboat have been a fairly small component of the recreational removals.

Similar to the commercial side, we had limited age and length compositions, and we had a little bit more for headboat, but, looking at those, there wasn't really strong indications of differences in the age compositions between those two sectors, and you can see that here in the bottom-left panel, that the age compositions looked fairly similar, both in terms of the modality, or the mode, of the ages as well as the range.

We did have a discussion with the panel, and some of the fishermen on the panel, both from the recreational -- Well, from the recreational and from the for-hire sector, and there wasn't any indication that there were significant differences in the fishing practices with respect to greater amberjack, and so we did pool headboat and general rec as a single recreational fleet.

This kind of shows the summary of that, and this is the removals by the fleets that were modeled over the course of the assessment panel, and so commercial in orange, dead commercial discards in yellow, the combined general recreational and headboat in gray, and then the combined general recreational and headboat dead discards.

That was the fleet structure that we used in the model, and we also looked at the recreational landings, and I should have mentioned this earlier, but the MRIP, using the recalibrated MRIP landings in the current assessment, whereas, in SEDAR 15, those were MRFSS recreational landings, and so you can see the comparison here. The blue is the updated landings using the recalibrated MRIP estimates, and then the orange is showing the ones that were used in SEDAR 15, and so a similar pattern, but some of the -- Particularly some of the high years are much higher with the new estimates than was available for SEDAR 15.

Indices, we had three indices of abundance, the headboat index, shown at the top left, from 1980 to 2017. Commercial handline is on the top right, and so 1993 to 2017, and then the SERFS video index in the bottom right, and we did consider a headboat at-sea discard index, but it was excluded due to low sample sizes, and we also looked into the SERFS trap index, but it was excluded as well, and I think there were only fifty fish caught in the traps over about a twenty-year period.

The other thing relevant to the indices is the CVs, and so there's been a -- This is a fairly common practice to modify the CVs on the fishery-independent indices and set them at 0.2 or 0.3, primarily because the sample sizes tend to be larger, and you can get really low CVs, just due to the sample sizes, and so we did set the CVs for headboat and commercial handline to 0.2, primarily so that we weren't considering the fishery-dependent indices to be more certain than the fishery-independent video index.

This just gives the summary of the modifications, and we have eleven additional years of data, 2012 to 2017, and general recreational landings include the recalibrated MRIP estimates, and we have updates to the maturity, growth, and mortality vectors, and we did modify the start year to start when the data started, as opposed to an earlier start year, where we would have seen some sort of virgin condition for the stock, and then we did model a single commercial fleet and a single recreational fleet, and those decisions were primarily because separate fleets, particularly the dive and the headboat, accounted for a small portion of the landings, and we didn't really have the composition data to effectively model the selectivity of those fleets.

Then the same thing for the discards. They are separate discards for commercial and recreational, but pooled across those different sectors, and we included the SERFS video index, and so this is a summary of the modifications and updates that were made since the SEDAR 15 assessment, and so I can pause here for a minute and see if there's any questions about the data or any of the inputs.

DR. SEDBERRY: Thanks, Kevin. Are there any questions from the committee? Any clarifying questions from the committee at this point?

DR. BUCKEL: I'm just curious, and, if you mentioned this and I missed it, I apologize, but the ID issues, was that discussed at all by the panel, in terms of the other jacks are sometimes -- The almaco and lesser amberjack and the confusion that sometimes folks have with the ID of those?

DR. CRAIG: Yes, and thanks for mentioning that. I didn't include a slide, and that was something that -- It's been a while, but we dealt with it on the first webinar, and Vivian went through a pretty involved process to come up with a methodology to separate the greater amberjack from the other jacks. I might have a PowerPoint on that that I could pull up, if you wanted to discuss it further, and I don't know all the details of how she did that, but it was sort of the first thing that we dealt with off the bat in the first webinar.

DR. BUCKEL: Thanks, Kevin. No, you don't have to bring up the PowerPoint, but I just wanted to make sure that it was dealt with, and so it sounds like it was, and that's great.

DR. CRAIG: Yes, and it was definitely a recognized issue from the start. I think it may actually have been one of the TORs, too.

DR. REICHERT: Thanks, Kevin. Two quick questions, and so the majority was a result of sampling and not a result of changes in the maturity over time, correct, and that's what you were saying?

DR. CRAIG: Yes, that's right, and so we don't really have maturity samples that are sufficient to develop the maturity for different points in time, and so this was an increase in the sampling, and there has been quite a bit of increase since the last assessment, and I think that's coming from SEFIS and from MARMAP, and SEDAR 15 -- I think it was Pat Harris's samples that were used primarily, from a published paper that I believe was in 2007, and I think it was 300 to 400 increase sampling, and so it was almost a 50 percent increase in the sampling since that, and that really captured a lot more of the one and two-year-olds, because it was coming from some of the fishery-independent surveys, as opposed to the fishery, which tends to be getting some of the older fish, and so it really helped to fill out the sample sizes of those younger ages, which are typically pretty limited for the maturity schedule.

DR. REICHERT: Okay. The other question, and I just found it, because I knew it was in the report somewhere, was the age and length comps for the video, and those were used from the chevron trap, correct, but the N was relatively low.

DR. CRAIG: Yes, and so this is something -- I guess I could have brought it up here, but I will get to it a little bit later, but typically what we would do is use the age compositions, or perhaps the length compositions, from the chevron trap to inform the selectivity of the video index, because

we don't have a measure of length from the video, and we did look into that for amberjack, and they were very limited, and I think there were only fifty fish that were aged, and 105 lengths, over like a twenty-year period, and so it was very limited, and the panel had a discussion about the size selectivity of the chevron traps, and amberjack are a fairly large fish, and so it did seem like there could be some bias towards younger, smaller fish in the traps, relative to what was seen on the video, and so what we ended up doing, and we can come back to this a little bit later, but we did assume a selectivity of one for the video index, in the absence of really good composition data. I will come back to that a little bit later.

DR. REICHERT: Okay. Thanks. I appreciate it.

DR. SEDBERRY: Thanks, Marcel. We have a few more questions.

DR. SHAROV: Could you please tell us where was the maximum age that you used in the calculation of natural mortality, and, also, it seems like there is sort of the general trend at the Center to switch from Lorenzen to Charnov, and could you, in a few words, explain why?

DR. CRAIG: I think the T_{max} is seventeen years, and I think the -- I am not sure that I know all the reasons why there has been a switch to the Charnov, and the primary reason seems to be that it's a much more recent meta-analysis, and so the Lorenzen -- The studies that were used in the Lorenzen paper, which was published I think in 1996, were fairly old, and it included a lot of fish from freshwater and north temperate systems, and it didn't have as many species from more southern systems, and so I think the expanded number of studies that were available in the Charnov paper that included and extended beyond those that were used in the Lorenzen study, and so I think that was the primary reason.

DR. SHAROV: Did it use the same method?

DR. CRAIG: I'm sorry?

DR. SHAROV: Was the same method used in the calculations, and so it just updated the information more, more stocks and more species included, but the same model was used, right, in the estimation?

DR. CRAIG: It's a slightly different model. It's a different equation that is based on the L_{∞} , the length at-age, and I believe it has some of the life history parameters in the Charnov model, whereas I think the Lorenzen is more of a weight-based just weight at-age, and so it is a slightly different model for getting the mortality estimates.

DR. SHAROV: All right. Thank you.

DR. SEDBERRY: Thank you, Alexei.

MR. ADDIS: Thanks, Kevin. I read in the report that the MRFSS index wasn't considered in SEDAR 15, and it was removed, and why not consider it again for this assessment?

DR. CRAIG: I mean, that's a good question. I think it borders on that kind of gray area between benchmarks and standards, and so there was a general rec index that was developed for SEDAR

15 as part of the data workshop, and it was ultimately excluded during the assessment phase, because it conflicted. I believe the conflict was with one of our indices, and I can't remember if it was headboat or commercial handline, and, for practical purposes, we didn't have somebody onboard to do that, and so we didn't really revisit it.

I think there may also be some questions. Some issues, because of the MRIP recalibration, that would result in a truncated index, because I don't believe those have been done for all of the years, going back to 1981, and I may be wrong on that. For the MRFSS index, it's always an issue trying to identify the appropriate metric of effort, and so I think those are reasons that we didn't revisit it, and just to kind of -- We have the TORs that are a guide, and part of the TORs were reconsidering the age and length comps and including the SERFS index, but not really revisiting all of the indices, the potential indices, and so I guess the short answer is we didn't think it was as useful, given that we had indices that have performed better in the past, and we have a fishery-independent index now to revisit an MRIP index.

MR. ADDIS: Okay. Thanks.

DR. SEDBERRY: Thanks, Dustin. I don't see any additional hands raised, Kevin, and so I think you can move on to the assessment model.

DR. CRAIG: Okay. During the assessment phase, I think there were two primary issues, and I alluded to these earlier. One is that we did remove the length composition data from the base model, with the exception of the headboat at-sea discard length comps, which we used to inform the selectivity of the recreational discards.

There are a number of reasons for that. One is the highly variable length at-age, and it's shown in the von Bertalanffy curve, and we don't have issues, necessarily, with ageing greater amberjack, and the confidence in age determination seems to be pretty high. We're not really estimating a growth curve internally, which would be one reason to retain length compositions in the model, and we did see degradation of the fit to the age compositions, and also to the indices, when we included all the length data, and it seemed that what was happening is the length data was -- because there is so much variation in length-at-age, it was dampening some of the recruitment signal that was coming through in the age compositions.

That was a primary reason for removing it, and we also -- When we started looking at the 1992 size limit regulation, we weren't really seeing any indication, or much indication, that that regulation was really having a strong effect on the lengths of the landed fish. When a size limit regulation goes in, you would expect to see a shift to the larger fish after the regulation, compared to before, and I will show some more on that in a minute, but we didn't see strong indications of that.

With the blocking, selectivity blocking, around that size limit regulation, we had a difficult time estimating selectivities in the early period prior to the regulation, because we just had very limited composition data, and we had no age composition data, and we had fairly limited length comps. I think, for commercial handline, we had three years, and then, for recreational, we had six or seven years.

As we went through that exercise of trying to estimate those selectivities and those different stanzas, we found that there's a mismatch in the data that's informing the selectivity, and so, after 1992, or post-size limit, the selectivity is being informed primarily by the age comps. Prior to the size limit, it was being informed by the length comps, and, when you look at those selectivities, we saw a shift to younger fish after the size limit in the selectivity function, and so it didn't really make sense with what you would expect, based on a size limit regulation, and so I will show a little bit more on that.

This is for commercial handline, and so this is the proportion of the commercial handline landings that are below the thirty-six-inch size limit, and so you can see here the size limit went into effect in 1992, and we've only got three years prior to that, and the size of the fish is actually increasing after the size limit went into place, which is not what you would expect, and we did dig into these three years of length compositions a little bit more and found out that there was fairly disparate sampling among different regions, and I believe one year all the samples came from Florida, and another year they came from North Carolina, and then, in a third year, they were kind of a mix.

Some of the size compositions in the early years seem to be driven more by spatial variation and sampling and not really reflective of the stock as a whole, or at least the way that we were modeling it, and so a similar type thing for general recreational, and so, again, we have a few more lengths prior to the size limit, but, again, it's highly variable. If you look at the average of the proportion of lengths below that twenty-eight-inch size limit, it's similar to what you see after, and so we weren't seeing really strong shifts in the lengths of fish and the landings, and then, when we tried to model that as two separate selectivity blocks, we were getting more younger fish, or a shift in selectivity to younger fish after the size limit, in both the commercial handline and then the recreational landings.

I think that results from this mismatch between the lengths and the ages, where length compositions are informing the early period selectivity and age compositions the later period, and so we're not really seeing the shift to older, larger fish that you might expect after the size limit, and so the panel spent a decent amount of time on this, and the recommendation was that, since we weren't seeing real strong size limit effects, and there wasn't any explanation, really, for why we might be seeing more younger fish after a size limit regulation, we ended up removing the blocking and not having separate selectivity blocks around that size limit. Are there any questions on that? I will just stop here for a minute.

DR. SEDBERRY: Any questions from the committee?

DR. CRAIG: I think this sort of issue is particularly evident here, because we do have these periods where we have only lengths, and then other periods where we have ages, but it's been sort of a perennial problem, or issue, I guess, is what are the length data actually providing, if anything, and, as I said, unless we're estimating a growth curve internally, or unless we just don't have any age compositions to rely on, then they're not really providing that much, and, in some cases, it may be dampening some of the signals coming from the other data sources.

We did remove the lengths and remove the selectivity blocking, and we did -- The only lengths that we retained were from the headboat at-sea discards, and so, in this case, we had five years, and you can see here at the length are the length compositions for discards in the headboat fishery, and we pooled those, and we weighted them by the number of trips, and we made the assumption

that the lengths of those discarded fish reflected the lengths of discarded fish in the pooled general recreational and headboat fleet, in the recreational fleet, and so these are the only length compositions that were retained in the model, and it was primarily to inform a selectivity of the recreational discards.

I will just go through the setup for the base run and then show the results from that, and so the assessment period is 1980 to 2017, and we're starting in 1980, and so we're estimating an initial F as the geometric, or we're assuming an initial F as the geometric mean of the first three assessment years, and we're modeling ten age classes. We're using the Charnov age-based mortality vector, the measure of reproductive potential is mature female, biomass, and we have two fleets, commercial and recreational, with associated commercial discards and recreational discards.

There are three indices, the headboat, commercial handline, and video. There is no selectivity blocking, and we assume constant catchability, and there's a Beverton-Holt stock-recruitment relationship.

We estimated the recruitment deviations from 1990, the deviations from the stock-recruitment curve, from 1990 to 2017, and so those early deviations -- We started in 1990, because that's when there was information from at least two of the data sources, the headboat and the commercial handline index, and then the age compositions, which start in 2000, inform those later recruitment deviations.

The initial age structure was fixed, and we didn't estimate deviations around that, because we didn't have composition data available in 1980 to do so, and so we assume an equilibrium age structure at the start of the model, and the landings selectivities were both logistic for commercial handline and general recreational, and the discard selectivity -- This top one should actually be for commercial, and it was assumed to be the probability that fish were below the size limit at a given age, and then, for general recreational, we used a negative exponential model to fit to the headboat discard length comps, the pooled headboat discard length comp that I showed earlier.

The index selectivities were set to the same as those for the fleet, and then an issue that was brought up earlier is we did assume the selectivity of the video index was one, and this goes through the reasoning for that. As I mentioned earlier, we had very limited trap catches of greater amberjack, and so we have fifty ages and 105 lengths over a nineteen-year period, and there is a thought that the traps might be biased to smaller, younger fish relative to the video, and this shows the length distribution of the fish that were caught, those 105 fish that were caught, and so you can see there are some large fish that show up in there, and so 1450 is out -- That's above the L infinity growth curve, and so that's a pretty big fish, but it's mostly younger fish, and these would be around one or two-year-olds, based on the growth curve.

Then these are the ages that were caught, and so a fairly broad range of ages, and there are only fifty of those, and so we saw fish out to age-five in the traps. We didn't see many older fish in the traps, but, given this level of sampling, you wouldn't expect more than a couple of fish in these older age classes, and you can see that age-five are already down to two or three fish, and so it's not surprising that we didn't pick those up, with an N of fifty.

This pooled age composition looks similar in shape to the natural mortality vector on the left, which would imply that there is fairly equal selectivity across the different ages, and so that was the reason that we assumed that the selectivity of a video index was one.

This is a little more on the base run setup, and so this is the BAM catch-age model, and it's fit using a maximum likelihood. We are using the Dirichlet multinomial to fit the composition data, where the sample sizes are equal to the number of trips, and that's a self-weighting method that accounts for zeroes in some of the composition bins, and it's a little better able to handle the correlation, the draw correlation, in the ages, or the lengths, and so this is something that is fairly new, but we've done it in the last few assessments.

For the abundance indices, we're using iterative reweighting to weight the three abundance indices, trying to modify those weights until we get SDNR, standard deviation and normalized residuals, and so approximating one. Then we're fitting the landings exactly using a low CV and then trying to fit the composition and index, to the degree that we can.

These are from the base run, and this shows the commercial landings on the left and the recreational landings on the right, and so the commercial landings had a pretty large ramp-up from the 1980s to about 1990, and then it kind of dropped off and has been fairly stable since then. The recreational landings, they're pretty variable, but they're higher in the 1980s and declined into the 1990s and have kind of been increasing recently. This shows the discards, and so, for commercial discards, they are showing that same increase in the 1990s, and then the recreational discards are on the right, and so those are the four removal streams.

This is the fit to the pooled commercial handline on the left, and the recreational age compositions, and so this is pooled over the years that we had commercial age comps and recreational age comps, and so the fit is pretty good for the commercial age comps and a slight underestimation of the age-fours, and we have lower sample sizes for the recreational age comps, and we have a few years in there that are just really at the margins, in terms of the sample sizes, about ten to twelve trips, and so I think we don't get quite as good of a fit to the recreational age comps, but it's still reasonable.

Then this shows the fit to that pooled headboat discard length composition, and so it's capturing the general pattern, but, again, because we only have five years and about a hundred fish, it's -- There's a lot of variation in that composition data.

These are some of the annual age compositions and length compositions, and so this is the one that I just showed for headboat discards, which is a pooled composition, and it says 2013 through 2017, and then, starting at the left, this is age comps for commercial handline, going down, and so there's eleven years of age comps for commercial handline. If you look at these effective sample sizes, and so it's sample size as modified, based on the Dirichlet multinomial parameter, you get about seven to thirty-two trips for commercial handline, and we capture the mode in most years, which is usually age-three or age-four, and so there's fairly reasonable fits to most of the age compositions from the commercial handline.

We do overestimate three-year-olds a little bit, starting in 2015, and we underestimate four-year-olds, and we can see that here as well, and so it's slightly overestimating threes and fours and underestimating fours and fives, but, again these are years where we have fairly low sample sizes, and so we did look at potentially modifying the timeframe over which the recreational deviations

were estimated, to try to hit those better, and there's no obvious change in the regulations that would account for that, and so I think this is just sort of a function of the low sample sizes that we have in some of those years towards the end of the assessment.

We had eight years of general recreational age comps, and so, when you have sample sizes in the twenty to forty trips per year, which are these early years, you can see that you have relatively good fits, in most cases, and the mode is typically around two-year-olds, or three-year-olds, but then some of these years, like 2009, is an effective N of four, and there's only ten trips that were observed, and so it's an unusual age composition there, as well as some of these later years.

Overall, the fits to the age compositions aren't terrible, and they do seem to fit fairly well, for years where we have reasonable sample sizes, but, again, this is a pretty data-limited stock, with only, in many years, ten to fifteen trips to form the basis of the age comps.

These are the fit to the fishery-dependent indices, and so headboat is on the left, and commercial handline is on the right. You can see that headboat gets downweighted quite a bit in the interactive reweighting, as indicated by these larger error bars, and the commercial handline gets upweighted quite a bit, and so we're tracking the commercial handline, or the model is tracking the commercial handline, index fairly well. It may be capturing some of the pattern in the recreational, but not really tracking the annual patterns, and we did get some years where we have some runs of residuals, in the early part and the later part of the time series.

This is the fit to the SERFS video index, and so you can see you get a really good fit the last four years, but a lot of annual variability in those first few years of that index. These are the selectivities, and commercial landings are on the left, and then discards are below on the left, and then recreational landings and discards on the right. Older fish from the commercial sector, A50 is about three, compared to two years for the recreational, with full selection at about five, and about four for recreational.

This is an assumption about the commercial handline discards, that the selectivity is proportional to the probability that the fish are below the size limit at a given age, and then this is the -- On the right is the recreational discards, which are mostly all one-year-old fish, based on the headboat at-sea discard length comps.

This is the estimated numbers and biomass at-age, and so numbers on the left and biomass on the right. You can see the numbers declined in the early to mid-1990s, and this is following a period of high commercial landings. If you think back to the landings, the highest landings occurred in 1990, and then they've been fairly constant since then, and abundance and biomass have increased since the mid-2000s.

There is some indication of a recent decline, in the last four or five years, but, overall, the stock appears to be above historic levels, and the other thing that you can take away from this graph is that there's little evidence of age truncation, and so we're seeing all the ages, pretty much, showing up throughout the time series, and there is some relatively strong year classes that are occurring in the 2000s, and so if you look at these red bars. This is 2001, and it's a fairly strong year class, and we also see that in some years in the mid-2000s, as well as 2012 and 2014, and, overall, the recruitment in the 2000s seems to be above average, above the average over the time period.

We weren't able to estimate steepness in this assessment, and it was estimated in SEDAR 15, and it was estimated at 0.74, but it wasn't well estimated, and it was a pretty unstable estimate. Here, you can see the stock-recruitment curve, on the upper-right, and so we have a lot of years where the stock has been driven down to a biomass where we would be able to estimate a steepness, and then the bottom-left shows the steepness profile, and so there's no indication of a minimum here, other than there is a low probability of low steepness, less than 0.5, and so, for the assessment, we have fixed steepness at the midpoint of this range, from 0.74 to 0.99, and those are the two likelihood units along the flat portion of the steepness profile. The steepness for the assessment was fixed at 0.87, and then we used that range of 0.74 to 0.99 in the ensemble modeling.

This shows the recruitment time series on the left, and the recruitment deviations are on the right and so, again, you see some low recruitment in the 1990s and early 2000s and higher recruitment in the more recent years, perhaps returning to more average recruitment towards the end of the assessment period, and we see these fairly strong year classes, and there is the 2001 and 2012 and 2014 that seem to occur throughout this period, since the early 2000s.

As a result of that, and this is just sort of belaboring that same point, but this is the -- You see these cohorts, where there is some evidence of these year classes that are moving through, and this is the 2001 year class, and these are the year classes in the mid-2000s, 2006, 2007, and 2008, a fairly big one in 2012, here in the blue, and then one in 2014 as well, and this is coming from the commercial handline age compositions.

This shows the spawning stock biomass on the left, relative to SSB MSY in green and the minimum stock size threshold in purple, and then on the right is total biomass, relative to BMSY, and so you can see that we're well away from any sort of overfished state, and the biomass has been increasing since the early 2000s, and that seems to be a function of this sort of constant landings, which you see here in the left-bottom panel, and the landings have maybe a slight increase, but they're relatively constant since the 2000s, and so constant landings and high recruitment seems to be driving this increase in biomass.

This is the fishing mortality, and so the left is F over F_{MSY} , and so we're currently well away from any overfishing status. The highest, F over F_{MSY} , occurred in the early 1990s, and this is when there was a large ramp-up in the commercial handline landings, and we had some discussion on what might be causing that on the panel, and it seemed like there might have been more of a commercial market for amberjack during that period that was associated with declarations of gamefish status for speckled trout and red drum in the Gulf that led to an increased demand in the blackened fish market, and I'm not sure if that's the case, but that was the suggestion from some of the discussions with the panel members.

On the right, it shows the fishing mortality by fleet, and so, again, in that early 1990s period, the commercial fleet is contributing mostly to the fishing mortality, but, in the early time period, it's primarily recreational, and then, in the more recent years, if you look at the recreational landings and discards, the recreational sector is accounting for about two-thirds of the fishing mortality.

This is a comparison of the benchmarks to the SEDAR 15 assessment, and so, on the left, it shows the fishing benchmark, F over F_{MSY} , for SEDAR 15, which is the dashed line, and SEDAR 59 is the current assessment, which is the solid line, and so it's a similar shape, but there's some differences, but, overall, it's fairly similar patterns between the two, and even more so for the

biomass benchmarks that track each other fairly well between the earlier assessment and the current assessment.

This shows the age structure relative to what you would expect at equilibrium, assuming, under MSY, the maximum landings were at maximum sustainable yield, and so the solid line with the black circles is the equilibrium age structure at MSY, and then the various dashed lines are the age structure across these decades, from 1990 to 1999, 2000 to 2009, and so forth, up until the current year of 2017. There is really no indication of an issue with the age structure, and the age structure is fairly filled out, and I think there's one year where it's below for age-four, but there's not an indication of a problem, with particular age classes being missing or relatively low abundance.

These are from the retrospective analysis, and so the panel had recommended a four-year retrospective, particularly given some of the data limitations within the assessment, and there's not a really strong pattern in the recruits over the last four years, and you do see a slightly lower spawning stock biomass in the terminal years of 2015 and 2013, although 2016, 2014, and 2015 seem to match pretty well, and I'm not sure why that is, and I think one potential explanation is we don't have consecutive years of general recreational age comps, and so, when you start peeling back years -- For example, when you peel back 2015, there's not another age comp until 2012, and so I think that's resulting in some of these discontinuities for particular years.

This is the same thing, and these are the retrospectives for the benchmarks, F over F_{MSY} at the top and SSB over SSB_{MSY} at the bottom, and there's not a lot of evidence of any sort of problem, although we do have a couple of years, 2013 and 2015, where both the fishing and the biomass benchmark appear to be underestimated a little.

These are the management quantities, and this is just pulled from Table 18 in the report, and so F_{MSY} was estimated at 0.69, and I think F current is around 0.25 or so, and so we're currently below the F_{MSY} level, and MSY was estimated at 2.3 million pounds, and then you can see the status indicators at the bottom, and so F over F_{MSY} , based on the last three years, is 0.4, and SSB , the terminal year, relative to $MSST$, is 2.8, and so no indication of overfishing or an overfished state. That's what I have for the base run. Are there any questions about any of that so far?

DR. SEDBERRY: Any clarifying questions regarding the base run?

DR. LI: Thanks, Kevin. This is really a great presentation, and my question is about the weighting for the abundance index, and you said that you weight it over time to reach an objective, and so my question is like you list the weighting process, and so do you weight the component for each abundance index, or do you weight, do you adjust, the parameter of the likelihood function for each distribution you assume for each index, to be able to adjust, change, the likelihood value for each index, and I don't know if I'm making sense or not. Like how do you do the weighting process?

DR. CRAIG: I think, if I understand your question, this is the iterative reweighting process that is described by Robert Francis in a number of papers, and I think it's 2011 where it's first described, but it's changing the CVs on the -- It's using the weights as a divisor to change the CVs on the indices, in order to meet this criteria that the standard deviation of the normalized residuals is equal to one, and so I don't know if I'm explaining that that well, but that's just basically trying to adjust

the CVs, via these weights, in order to acquire the best fit to those three indices that we could get simultaneously.

DR. LI: Okay, and so which means you did not assign any weight to each likelihood component?

DR. CRAIG: Did you say assign any weight?

DR. LI: When you have the likelihood -- When you fit the model, the total likelihood equals to the summation of each likelihood component, for each dataset, and I know in Stock Synthesis that you can assign a weight to each data component, or likelihood component.

DR. CRAIG: There is no adjustment.

DR. LI: Okay.

DR. CRAIG: There is no direct adjustment to the likelihood component. It's only via the CV on that particular data source, which is modified by changing the weight.

DR. LI: Right, and I know the Francis weighting in the paper has like two-stage weighting, and so, in the first stage, you assign a CV, and, in the second stage, you adjust the CV by changing some parameters in the likelihood function, and that means that you have to assign a CV first to each abundance index, right, to be able to change -- What is that CV for each one? Are they equal or different for each dataset?

DR. CRAIG: Yes, and so each one of those abundance indices is based on a standardization, either a delta GLM for the fishery-dependent indices and a zero-inflated negative binomial for the SERFS video index, and so that standardization typically gives you the CVs in the first stage of the iterative reweighting that then get adjusted in the second stage.

In our case, what we did was modify those CVs on the fishery-dependent indices, and so, instead of using the values from the standardization, we set those to 0.2, and then we retained the CVs on the fishery-independent index, and the reasoning for that is that fishery-dependent indices typically have very high sample sizes, and so there's a lot of data, and so the CVs from the standardization itself can actually be quite small, compared to a fishery-independent index, and so we try to avoid the situation where we're fitting the fishery-dependent indices more closely than the fishery-independent index, simply because there is higher sample sizes, and so I guess -- I hope this answers your question, but we set the CVs to 0.2 for the fishery-dependent indices, and we use the CV from the standardization for the fishery-independent video index, and those are the stage one weights, going to Francis, and then those get modified, via the iterative reweighting process, in order to get the SDNRs as close to one as we can.

DR. LI: Thank you, Kevin. I am just thinking that, with the Francis weighting, the whole purpose of doing the weighting is -- The paper is because usually you have a lot of composition data, versus your abundance data, and so the composition data always like -- In terms of the quantity of the data, it's outweighs, outnumbers, the abundance data, and so that's why the paper says we need to focus on -- The fitting process should focus on the fitting to the abundance data and not the composition data, and that's why the whole paper suggests Francis weighting the whole process.

I am thinking like, in this case, in the case here, since we don't have much composition data, because you use only one composition data, and so it's not outnumbered, and it does not outnumber the abundance index data, and so I'm thinking like -- Do you think it will make any difference if you don't do the weighting at all and just let the data be what it is?

DR. CRAIG: I'm not sure. I think I agree that the original -- One part of the weighting for the iterative reweighting method was to give primacy to the indices relative to the compositions, and so we don't really address that here, because the compositions are fit using the Dirichlet multinomial, which is a self-weighting method, and so we do have multiple compositions.

We have age comps from the general rec and from commercial handline, and we have length comp from the headboat discards, but those are all weighted internally via the Dirichlet multinomial dispersion parameter, and so they're kind of taken out of the iterative reweighting framework, but we still have three indices, and those indices differ from each other, and so there's only one abundance trajectory out there, and we're trying to approximate that by an index, and we just don't know which one of these indices is best representative of that unobserved abundance trend, and so that's kind of the reasoning behind using the iterative reweighting to try to fit each one of those indices as best we can, because we don't know, a priori, obviously, what the abundance trend looks like.

I think your question would be an interesting one, as to how -- If we didn't modify the weights on the indices, would that make any difference to the assessment, if we just allowed them to be fit according to their CVs derived from the standardization, and that's a good question, and I don't actually know the answer. We didn't do that in this assessment.

DR. LI: I was just thinking, without any justification about the sampling bias, or you have -- If there is obvious problems with the data itself, then, when you weight the data, usually you justify why you weight these data higher than the other dataset. Thank you so much.

DR. CRAIG: So that would be an exercise that would be worth doing, and certainly it would be an easy sensitivity analysis to do, and I guess we do make the assumption that the fishery independent index is probably more reflective of the trends in abundance than the fishery-dependent indices, and so that's why we kind of modify those first stage weights from the standardization, to avoid fitting a high sample size fishery-dependent index more closely than a low sample size fishery-independent index, but, beyond that, that kind of criteria, we haven't really explored other ways to weight or not weight the indices or what effect that might have on the outcome of the assessment.

I guess the other thing I would say about the indices is they are kind of tracking different components of the population. I mean, the headboat index, if you think about the selectivity, the A50 there is around two, a lot of twos and threes, and the commercial handline is more fours and fives, and then, for the video index, we're assuming a selectivity of one across all ages, and so they are presumably, to some extent, tracking different components, or different aspects, of the population. It would certainly be worth not weighting those and seeing -- I am not sure how you would decide then what to do, and the iterative reweighting is an objective way to maximize the fit across different data sources, and so we could do it the other way, but I'm just not sure how we would interpret that or how we would necessarily choose.

DR. LI: To me, weighting is good, and it's useful, as long as we have the justification for our decisions, and you just gave me the justification, and so thank you.

DR. SEDBERRY: Thank you. Amy Schueller has her hand raised.

DR. SCHUELLER: Kevin, can you go back two slides to the retro status? I stepped out for just a minute while you were on this slide, and I was wondering if you could just speak to what's going on in these slides, and I know that clearly we're under one by a vast amount, but, if we were near one, this would be pretty concerning to me, and so I just wondered if you could speak to that, of if you know what's driving that.

DR. CRAIG: I don't know exactly what's driving that. I mean, my suspicion is a couple of things. We don't really see a consistent departure as you peel back the retrospective, and so, by that, I mean, if you have 2016 as the terminal year, you're getting a good estimate, and 2015 and 2014 and 2013, and so these aren't actually in order, right, and so, in the bottom panel, it's kind of a good example. You can see that, if the terminal year is 2014, we're right on the line.

If it's 2016, we're right on the line, but, if it's 2013 or 2015, we're below, and so it's not a consistent departure as you remove those years, and my suspicion is, and I don't know this for sure, but we are -- We do have an index at the end that is only seven years, and so the video index goes from 2011 to 2017, and so we're losing a lot of that index, particularly as we remove three or four years, and the other issue that might be contributing to that, and I'm not sure I can articulate exactly how it's contributing, but, normally, we would have consecutive age compositions up until the terminal year of the assessment, and we don't have that, and so we have cases where, if you remove 2015, there's not a 2014 and a 2013 age comp. There's just a -- You don't have an age comp until 2012, and so I think some of that discontinuity in the age sampling at the end might be causing some of this.

If you look at the index, once you get back to 2013 or so, you're getting into this part of the index, and so you're going from an increasing abundance to a decrease in abundance over the course of those retrospective peels, and so I think that might be causing some of that effect, particularly on SSB. I'm sure you know it's difficult to identify a mechanism. Because we were so far away from the status and because it wasn't a -- You know, we weren't seeing a consistent pattern, where every year that we peeled back was leading to a more severe kind of underestimate of the benchmark, and it didn't seem like something that was really that concerning, at least not to me, and it's not that different than some of the other retrospectives that I've seen.

DR. SCHUELLER: Point taken. I would say that hopefully it's one of those reasons, but, in my most recent experiences, I thought that the issue I was having was one of those reasons, and it turned out to be something else, something that came up with menhaden that we were like, wow, we should keep an eye on that in the future, and it was like an indicator of model performance and things like that, and so I just say that to put on the record here that perhaps in the future, if this appearance keeps showing up, we probably should look into this a little bit more.

DR. CRAIG: Yes, and, if you have ideas about how to sort of diagnose the underlying causes of a retrospective -- I mean, I kind of focused on the age compositions, for the reason I mentioned, that they're non-consecutive, but we also have very low sample sizes in the age comps for some of those most recent years, and we're not necessarily fitting them very well, and so I think that

might cause some of this kind of jumping around, but I always have a hard time making too much of it, because it's four years, and it's not a consistent pattern. It just seems like 2013 and 2015 -- If it was other years, you wouldn't worry too much, but it's just this 2013 and 2015 that look a little bit odd, but, again, where we're at in terms of the benchmarks, it didn't seem like it was a deal-breaker. Certainly, if we were close to a threshold, then it would be more of an issue.

DR. SCHUELLER: Thank you.

DR. SEDBERRY: Any additional questions on the base run before we move on? I don't see any hands raised, and so what I think I would like to do now is take a ten-minute break, if that's okay with you, Kevin.

DR. CRAIG: Yes, that's fine.

DR. SEDBERRY: We'll take a ten-minute break and then come back and take up the next subject, which is sensitivities. Let's meet back here at ten after. Thanks, everybody.

(Whereupon, a recess was taken.)

DR. SEDBERRY: Welcome back. I feel energized, now that I've had a giant chocolate chip cookie, and I'm ready to continue on with Kevin's presentation of the greater amberjack assessment, and I believe the next part of his presentation are the sensitivity runs.

DR. CRAIG: We had a list of sensitivities that the panel agreed to that are shown here, and first was to evaluate the effect of the change in the start date, since that was a departure from what was done in SEDAR 15, and so we started the model in 1947, as opposed to 1980, and then we have the standard suite of variation in natural mortality and steepness and discard mortality, and so we had a low and a high natural mortality, which was 0.5 and two-times the vector that was used in the base run, as well as the SEDAR 15 natural mortality vector, which was based on the Lorenzen M.

Then we had a low and a high steepness of 0.74 and 0.99, which was based on the likelihood profile that I showed earlier, and then, for discard mortality, this ranged from 0.1 to 0.3, around the value that was used, which was 0.2 was taken from the SEDAR 15 assessment, and, since we did change a number of the life history parameters, we looked at that as well, and so we looked at the effect of those in combination, as well as separately, and so we looked at the effect of growth, maturity schedule, natural mortality, and steepness, and then we did have a request to look at variation around, in the selectivity function, for commercial handline, and that was because there was some suggestion that, particularly in the early years, there might have been some selection for smaller, younger fish, because of the perception that they might have a lower parasite load, but then, if you think back to the length data, those early years, there seemed to be some suggestion that what the commercial fishery was actually taking in those early years was larger, perhaps older, fish, and so we looked at variation around the A50 for commercial handline.

I will step through those here, and so this is the start year. The start year has virtually no effect. The top panel, and all these graphs will be the same. The F over F_{MSY} is at the top panel, with the 1.0 line shown as a bar, and then the SSB over SSB_{MSY} is the bottom panel, and so no effect on the fishing status of the start year, and then there's a slight departure for a few years in the

1980s on SSB over SSB MSY, but there's really no effect on the status indicator over the assessment period.

This is natural mortality, and so this is one that was fairly consequential, and so the base run is shown as a solid line, with the circles here, and the lower natural mortality is this top line, and so, when M goes down, F goes up, and so this actually did result in a change in the status if you assume a much higher natural mortality, and then the opposite for the lower M . Then the SEDAR 15 M , based on Lorenzen, is intermediate, and it's shown here as this dashed and dotted line.

Then these are the patterns for SSB over SSB MSY, and so lower M leads to higher biomass, and higher M leads to lower biomass. Steepness was one that didn't have a huge effect, but we didn't have a really large range in steepness, and so the steepness, high steepness, was 0.99, and then the low value was 0.74, and then the base run of steepness was fixed at 0.87, and so you see the directional effects that you would expect, and the lower steepness suggest a lower productivity stock and a higher F , and then vice versa for the higher steepness, but not really any fundamental change in the status associated with a range in steepness that we addressed here. Similarly, discard mortality had very little effect. Low discard mortality results in higher F over FMSY, or sorry. Lower F over FMSY and higher biomass and then vice versa for the high discard mortality.

This was the combined effect of the SEDAR 15 curve and maturity schedule, which, if you remember, was slightly -- Maturation was occurring at slightly older ages, and steepness, which was a bit lower in SEDAR 15, 0.74, as opposed to 0.87, and M , which was lower in SEDAR 15 compared to the current assessment, and so all of these would suggest a stock with lower productivity, maturing at older ages, lower steepness, and lower M , and you do see the combined effect of those in the line here, and so FMSY for those combined, compared to the base case here.

It does result in a change in status in the assessment, and it would also suggest that the stock has been overfished for most of the time period, since the 1980s, and then, similarly, for the SSB over SSB MSY, it would suggest it's been below the threshold for most of the time period.

We did separate out what was driving that, and it seems to be the natural mortality, primarily, and so this -- It's a little bit of a busy graph, but the red is the base run, and then the green and the yellow, which may be difficult to see, and they're right on top of the base run, and those are the effects of the growth curve, and the maturity schedule, which were very small, small, if any, and then the dashed-blue line shows the effect of that growth and maturity plus the lower value of steepness, and then the purple line, which is where you do see a big departure from the base, shows the effect of the growth and maturity schedule from SEDAR 15 plus the M from SEDAR 15, and so that's the purple line, which suggests F over FMSY, and it may be higher, close to, but not approaching -- Not an overfishing state or overfished status.

Then, for the commercial selectivity, again, there's some effect, particularly of assuming higher selectivity on the younger ages, and it drives F over FMSY down and SSB over SSB MSY up, and then shifting to older ages had very little effect relative to the base run, because most of the mortality has already occurred by the time the fish are four years old, which was the shift to older ages for the commercial handline selectivity.

Then this is the phase plot, and most all of those selectivities, except for two, are in the not overfished and not overfishing quadrant of the phase plot, and the exceptions are, as I alluded to

earlier, the low M , and the low M combined with a low steepness, which are sort of borderline, in terms of the biomass threshold, and that would suggest that overfishing has been occurring and has been occurring for, based on the time series plots, the duration of the assessment period. I will just pause there for a minute. Any questions about the sensitivity analysis?

DR. SERCHUK: Just an observation, Chairman. The sensitivity analyses that show the differences between the growth, maturity, and M values, a couple of slides back, I think are very instructive, because they have a large -- There's been a large difference between what was used in the previous assessment and what was used in the updated assessment, and it seems to me that, because we can get significant changes in the status of the stock relative to F versus F_{MSY} and SSB to SSB_{MSY} , that, when we do updated assessments, I think it would be useful in the terms of reference for other stocks to note if there are any new analyses that relate to growth or maturity or natural mortality, and they should be investigated, rather than simply going forward with the values that were used in the previous assessment, because it can give us a completely different picture of the stock relative to the metrics of where we are relative to the biological reference points. Thank you.

DR. SEDBERRY: Any other clarifying questions?

DR. SCHUELLER: I will just add on to what Fred just said, in that, if the growth and maturity and M information had been the same, that retrospective would be a big deal, and so being clear in the terms of reference and being clear about what changes are authorized is pretty critical.

DR. SEDBERRY: Thank you, Amy. Any other clarifying questions?

DR. ERRIGO: I just wanted to make clear that this assessment was a standard, and the previous one was an update, and so these are actually the last standards that are ever going to be done, and they will be operational assessments from now on, and you're right that the terms of reference are critical in that, but that's why this one was able to take a lot more liberties in what was changed.

DR. SEDBERRY: Thanks for that clarification, Mike.

DR. ERRIGO: As you can see, it makes a big difference.

DR. SCHARF: George, just a clarification. As I understood this, and I was looking through this earlier, and Kevin can comment, but all of these changes combined made very little difference, if you don't include M .

DR. CRAIG: That's right.

DR. SCHARF: This is just reflecting uncertainty in M , in terms of the real effects on stock status, which really isn't much different than any other assessment that we do.

DR. CRAIG: Yes, that's right, Fred. I think we can kind of see it here. There is the effects from growth and maturation. Because these are ratios, they are miniscule, and steepness -- You know, we don't have a huge range of steepness, and we went with the estimate from SEDAR 15 versus the maximum steepness, and so that's 0.74 to 0.99, and then you're right that the natural mortality, as is often the case, has a very large effect, because it basically dictates the fishing mortality. I

would also say that I thought that the last assessment, Mike, and I could be wrong, but I thought that SEDAR 15 was actually a benchmark assessment.

DR. ERRIGO: SEDAR 15 was a benchmark. I'm sorry, but I'm talking about the last assessment that was discussed by the SSC this morning, which was king mackerel.

DR. CRAIG: Yes, and it is something that -- This assessment is ten years old, and so it's pretty difficult to be ten to twelve years out and knowing that there has been additional sampling and there's been improvements in methodology, and I do think some of the natural mortality estimates are actually an improvement over what was used earlier, and not actually use those, but I would agree with Fred that it is useful to evaluate those.

I guess the other thing is that there is -- It's dangerous to rely on this too much, but there is a sniff test here as well, and I think greater amberjack is, in many ways, a bycatch fishery, and it's not a heavily targeted -- Although it is targeted, but it's not necessarily as targeted as maybe some other species, and there are some reasonably restrictive size regulations and then some bag regulations, bag limits, during the spawning period, and, given the life history, that you have 50 percent maturity at age-one, and it's growing fairly rapidly, it doesn't seem to make a lot of sense that the stock would have been overexploited for the last forty years, and I know that's something we're trying to determine with the assessment, but, given that life history and the considerable amount of reproduction that we think is occurring before exploitation is really kicking in at two, three, and four-year-olds, it does seem -- And, given the nature of the fishery, it seems unlikely that it would have been in an overfishing state for this long. That's just my two-cents.

DR. SEDBERRY: Okay. I don't see any additional hands raised, and so, Kevin, I think you can proceed forward here.

DR. CRAIG: Okay. This is the uncertainty analysis, which we're calling ensemble modeling, or what used to be the MCB analysis, and there's a -- There's two components to this. There's a boot strap of the actual data sources, and so developed new time series of the landings and discards and the indices, based on the CVs that were either provided by the data providers or provided in the base run, and then, for the composition data, we resampled the actual number of fish that were observed and assign them to those length or age comp bins, based on the probabilities from the original data.

Then the Monte Carlo component, we included three components in this part, the natural mortality, discard mortality, and steepness, because had fixed steepness in the base run, and so the natural mortality is based on the uncertainty in the regression parameters from the Charnov meta-analysis, and so what we did is take the slope and the intercept from that regression model and drew from a normal distribution for each one of those parameters, based on the reported 5th and 95th percentile confidence intervals for all of those studies that were included in the Charnov paper, and then those are used to recalculate a natural mortality vector for each one of the model runs, and that's what you're looking at here.

This bottom graph is showing the actual values of M, or the M vectors, that were used in the ensemble modeling, and so the black is for the base run, and then the one thing to notice is that the upper range of this is pretty high, and I think this will come up in some of the MCB -- Some of the distributions for the MCB results later, but these Ms at-age are -- We're assuming, in the base run,

they're around 0.8 or so, but then, in the ensemble modeling, they're two to two-and-a-half fold, that value, and so there's a lot of variation in M that's being incorporated here.

Discard mortality was drawn from a uniform distribution, based on the ranges of 0.1 to 0.3 that were used in SEDAR 15, and we'll assume the same for the commercial and the recreational fleets, and then, as I mentioned before, steepness was fixed, and we based the range of steepness values on two likelihood units from the midpoint, which was used for the base run, and so steepness in the base run was 0.87, and the range of steepness values included in the ensemble modeling was 0.74 to 0.92, and those values were drawn from a uniform distribution with those limits.

This shows the distribution of the benchmarks from the ensemble modeling, though the solid line is the base run, and the curve is the distribution from the runs that made up the ensemble, and so I don't have the medians of those on here, and I'm sorry for that, but you can see the median for the MCB runs, for FMSY, is shifted to the right, relative to the base run, and so the base run has an FMSY of I think it's 0.69, and then SSB is the right panel, and the bottom-left shows MSY in thousands of pounds, and then the bottom-right is MSY in thousands of fish.

One thing I think is, because there is such a range in natural mortality, particularly at the high end, a lot of these runs are -- The median is shifted to the right in the ensemble modeling, because it's suggesting a more productive stock that could sustain a higher M value, and so there is a shift to the right in the FMSY, and then a shift to the left in the SSB MSY, relative to the base run, and I think, because we're including such a wide range of variation in M , particularly at the high values, which would suggest a more productive stock than what we assumed in the base run.

These are the same distributions for the status indicators, and so SSB in the terminal year relative to the minimum stock size threshold, relative to SSB MSY in the middle panel, and then the terminal F was taken as a geometric mean over the last three years relative to MSY in the bottom panel.

Then this shows the time series of the biomass status on the left, and the fishing status on the right, with the 5th and 95th percentiles shown by the shaded area, and so, even with this range in uncertainty in M , steepness, and discard mortality, we're not seeing a lot of indication, or any indication, that there it is approaching some change in the status of the fishery, both in terms of the biomass or the fishing status.

This shows a phase plot based on all of the ensemble runs, and so, again, most of the runs fell into the upper-left quadrant, which is not overfished and not overfishing. The green lines show the 5th and 95th percentiles of those runs, and a few suggested overfishing status, but it's a relatively small proportion, about 2 percent or so, and very little suggested an overfished state. I think the last thing I have are the projections, and are there any questions about the uncertainty analysis?

DR. SEDBERRY: Are there any other clarifying questions?

DR. SHAROV: I am a little bit confused by the results of the sensitivity analysis on the natural mortality. When you used the constant values from SEDAR 15, I believe, with the Lorenzen method with the lower M , you ended up with the SSB, in general, being below SSB MSY for most of the time period, and vice versa for F . Here, you have a large range of M s that you are re-

sampling, and they are -- Sorry. I see now that most of them are even higher than what you are estimating. That's why your status is different. Okay. I think I got it.

DR. CRAIG: That range of M is not asymmetric. There's a lot more runs that have the high M , and so it would have to be a pretty productive stock to have the observed landings with an M this high, and so I think that's why there is this shift between the base run and then the median of the SSB, which is suggesting a higher fishing mortality rate and a lower spawning stock biomass, because it's seeing a much more productive stock, because there is so many of those M vectors at the upper end of the range.

DR. SHAROV: That's right, yes, and I've got it. Thank you.

DR. SEDBERRY: Any other clarifying questions on sensitivities before we move on to projections? I don't see any hands raised, and so, Kevin, I think we're ready to move on.

DR. CRAIG: Okay. I think I grossly underestimated the time this would take. I was assuming about an hour, and I think we're going on three hours, two or three hours, here, but I only have a few slides left on the projections, and so the projection methodology is the standard projection methodology that has been used for most, if not all, of the SEDAR assessments.

The projection model is the same structure as the assessment model, and it's based on the parameter estimates from the assessment model, and it does carry forward the uncertainty from the ensemble modeling, in terms of each of the individual runs that varied most of the data sources, as well as the steepness, the discard mortality, and the natural mortality, and that includes uncertainty in the abundance at-age in the first year, et cetera, and it uses a single selectivity curve, which is a weighted average across the fleets in the last three years.

For the purposes here, the fishing rates that define the projections were assumed to start in 2020. They are seven-year projections, starting in 2018 and going through 2024, with 2018 and 2019 assumed to be our interim years, where the landings are fixed at the average landings that were observed over the last three years of the assessment, and so the first year of management is assumed to occur in 2020, and there were three scenarios that were listed in the TORs, and one is F equal to F current, where F current is the geometric mean F over the last three years of the assessment, F equal to F_{MSY} , which is equivalent to a P^* of 0.5, and then F equal to 75 percent F_{MSY} , and so a slightly lower fishing rate than the MSY level.

These plots have a lot of lines on them, and so I did put a little description here, so you could get oriented, and the thick blue line is the benchmark from the assessment, and the green line is the median from the ensemble modeling. The thin line with the solid circle, that's the base case deterministic projection, and then the thin line with the open circle is the stochastic projections, the median from the ensemble with the confidence intervals shown by the thin solid lines here.

This is the F equal to F current projection, and so F current is actually below F_{MSY} , and so removals after management is implemented in 2020 declined, since the fishing is less than F_{MSY} . The stock status stays above SSB, because we're fishing below the F_{MSY} rate. This is F equal to F_{MSY} , and so the stock moves toward the MSY benchmark when the management is implemented, and the spawning stock is driven down, because we're fishing at a higher rate than is currently the case, and it approaches the SSB at MSY level.

Landings are initially higher, because the stock is above BMSY during those first few years of the projection, but it also moves toward the MSY level towards the end of the projection period. Then 75 percent FMSY is just sort of a very similar version of the last figure, with the exception that the F and the landings and the spawning biomass don't quite approach those benchmarks, because we're fishing at a slightly lower rate than the FMSY level.

I just put this table in here, and this is Table 21 from the report, and this is for the F equals MSY projection, and Table 20 and 22 have the F current and the 75 percent, and then the highlighted areas here show the landings during the projection period, both from the base run of the assessment, as well as from the median of the ensemble modeling.

That's all I had, and I will just briefly review some of the research recommendations. Certainly this stock was fairly limited, in terms of age compositions, both from the fisheries as well as from the fishery-independent index, and so there were recommendations to increase the age sampling in both the recreational and commercial sector, particularly since we did see some indication of cohorts moving through, particularly in the commercial handline age comps, which isn't always the case, and then developing some methods to get at the selectivity of the video index, and I think this is an ongoing effort.

In past assessments, we would typically make the assumption that the trap catches are reflective of the sizes and ages observed in the video, but that can be a pretty strong assumption, in some cases, and, here, we have a lot of observations on the video and very few observations from the trap.

As several people had noted earlier, we do have some reasonably significant changes in some of the life history parameters. We were using mature female biomass as a measure of reproductive potential, and we saw differences in the maturity ogive from the prior to the current assessment. We have some limited information, I believe, on batch fecundity and spawning frequency, but not enough to really get at a better measure of reproductive potential, and so, if there are significant changes in reproductive output, based on size or age, we wouldn't necessarily capture those with just a female biomass.

Then the natural mortality played a big role in this assessment, as Fred mentioned earlier, and I think amberjack might be a species where some other approaches, either telemetry or conventional tagging, might be possible, to improve the estimates of natural mortality. Then greater amberjack are one of the species that they seem to be fairly mobile and migratory, although they do show up on these hard-bottom reefs and things like that, and so some better characterization of the migratory dynamics and the potential for distribution shifts were another research recommendation. That's what I had, and I can stop here and see if there's any additional questions about anything that I have shown so far.

DR. SEDBERRY: Thanks, Kevin. That was quite a lot of --

DR. CRAIG: Or what was in the report, for that matter, too.

DR. SEDBERRY: That was a very thorough presentation, and we appreciate you taking the time to present it us to us. Before I take any additional questions or discussion from the committee, I

would like to take public comment first. As is normal, we take public comment after the presentation, and so, at this point, if there is anyone from the public who would like to make a comment, please raise your hand. I do not see any raised hands. Are there any additional questions from the committee on this last section of Kevin's presentation or on the presentation as a whole?

DR. SHAROV: A couple of quick questions. One is on selectivity, and so where is that -- Where we did we decide, or where did you decide, that there was any advantage in fixing the steepness in between your, as you characterized, poorly-estimated steepness in the model and the unit, the one, to get it somewhere in the middle, versus just simply saying, well, there is no indication, there is no reliable indication, that there is a stock-recruitment relationship, and, therefore, we just assumed steepness being one and proceeding as has been done with many other assessments, and was there any advantage of doing what you've done with the steepness?

DR. CRAIG: Well, I think you're sort of describing the two alternatives, and I don't know that they're really alternatives. I think, by choosing a proxy -- One, you have to choose a proxy, which also implies a particular value of steepness, and so I think choosing steepness, fixing steepness, is more explicit, and it does allow you to evaluate the effects of the uncertainty in the steepness value that you chose, which isn't always possible when you use a proxy, and I think part of it was that we were trying to -- This is sort of what we had mentioned before, is that, with the standard assessments, there is always this question of how much you deviate from what was done in the previous assessment, and steepness was used in the previous assessment, although, at that time, there wasn't -- People weren't really using the proxies that often, but we did try to maintain some consistency with SEDAR 15, in that respect, and so those were the primary reasons.

Choosing a proxy implicitly implies a particular value of steepness, whereas fixing it allows you to be more explicit about what you're assuming and to evaluate the consequences of the uncertainty in that choice, and then the desire to try to maintain some consistency with the prior assessment that had been done for this stock.

DR. SHAROV: Thank you. The second one was just a clarification, and I don't recall, and maybe I missed it, but a similar question to what Fred Serchuk was asking earlier. The fishing mortality rate that you're referring to throughout the assessment, what is it? Is it the full F, because, when you're jumping from assessment to assessment, there is no standardization, and so I just wanted to make sure that I followed that.

DR. CRAIG: In everything I showed here, it's the full F, as opposed to apical F or something like that.

DR. SHAROV: Right, or some weighted average or selected for whatever number of groups, and I tried to go through the report, but I couldn't find a specific description of what exactly is in the report, but I just assumed, but I needed to confirm it.

DR. CRAIG: Yes, and so I think that's right. It's full F, and then, in the presentation, in the report, where we show the F by fleet, it's the contribution of those particular fleets to the full fishing mortality rate.

DR. SHAROV: All right. Thank you.

DR. SERCHUK: I just would like to make a comment, if I could, and I, as well as Anne, or the SSC members on this -- We participated in the process here, and it was a real pleasure to work with the people that were as accomplished, from a technical perspective, as Kevin and Erik, as well as the great data that were provided by all the people from the institutes that provided the information, and I thought there was great synergy in the process, and I thought that all the participants on the team worked in a very harmonious fashion, and the analysts were open to our questions and suggestions and follow-up, and I know, when you are asking for people to participate in assessments from the SSC, sometimes there is great reluctance, but, because of my experience, particularly here with Kevin, I think it's a great learning experience, and we were so blessed to have the people on this team contributing the way they did, and so my hat is off to all the others on the team that worked together with the technical people to produce this assessment. Thank you.

DR. CRAIG: Thanks, Fred. I appreciate those sentiments, and certainly having you and Anne and other members of the panel engaged and providing feedback and suggestions is helpful. I have had assessments where we've had very quiet panels and then very engaged panels, and it's always more fun to have an engaged panel, and it actually results in a better product, and so I appreciate your participation, as well as the others.

DR. SEDBERRY: Wilson, did you have a question?

DR. LANEY: Yes, Mr. Chairman, I did, and so, looking at the last research recommendation bullet, Kevin, it seems to me that amberjack may be another species, kind of like king mackerel, where habitat considerations don't enter into the life history of the stock tremendously, but, again, my perception is amberjack may be a bit more associated with reef structure than king mackerel, I suppose, and the question would be, to the extent that our co-management partners engage in additional artificial reef construction during the future, is that something we should keep an eye on? Is that a possible factor that can enter into increased stock productivity?

Then, also, the other thing that's going on is that there's a tremendous amount of bottom mapping occurring off the South Atlantic, and we're learning more and more about where coral reefs are, especially deep coral reefs, and I'm not sure whether or not amberjack are associated with any of those or not, but I would suggest that another thing we should keep an eye on is, as those maps become more and more accurate, and we understand more and more about habitat associations of these species, we might even get to the point where we can start talking about habitat-production relationships, and do you think that is something that's a possibility in the future?

DR. CRAIG: I mean, I don't know that I have a specific answer to that, and I do think -- When we started this assessment, I was actually a little surprised at how structure associated greater amberjack appear to be, because they are large fish, and they're kind of pelagic swimmers, and they're migratory, and a lot of the spawning is occurring at the southern end of the range, and so I was surprised that we were able to get a video index for greater amberjack on hard-bottom reefs at all, and so I think there is something structure-associated that might be driving them.

I think the larger point you alluded to is sort of the much larger debate about whether artificial reefs, or increasing habitat, is actually leading to higher productivity, or is it somehow aggregating the productivity that already exists in the system, and that's a -- I think that's a question a lot of people -- In the Gulf as well, where they're doing a lot more artificial reef deployment, and the

South Atlantic, and I know we're starting to do more here, and so that question is relevant here, too.

DR. LANEY: Well, that's a perpetual, ongoing debate that may never be settled, perhaps, but, still, given our increased understanding of the natural reef systems that are out there, it seems to me that is something that we might want to keep an eye on, and, also, to the extent that we're getting more and more video monitoring, and I don't know, and maybe Marcel can say whether or not there is any likelihood of amberjack being associated with any of those newly-mapped deeper reefs, and my sense is that they tend to be more associated with the shallower, more inshore reefs, but am I correct on that, Marcel?

DR. REICHERT: Well, we do see them on the short bottom longline, which we generally deploy in deeper water, say between like ninety or a hundred meters to about 350, and so we do catch them there. Off the top of my head, probably towards the somewhat shallower areas, but it's not unusual for us to catch them in somewhat deeper water, and so I think they have a range, but I would agree with you that I think we probably see them in higher densities in the mid to shelf-edge range, but that's just without looking at our data, but kind of off the top of my head.

DR. LANEY: Thank you.

DR. REICHERT: Because, Kevin, correct me if I'm wrong, but you guys did consider the short bottom longline for amberjack?

DR. CRAIG: Yes, we did, and I think that was one where the sample sizes were limited and the duration was fairly short and there wasn't really enough there to put together a good index.

DR. REICHERT: But we do catch them on there.

DR. SEDBERRY: Mel, I see you have your hand raised. Did you have something to say about the stock assessment?

MR. BELL: Wilson asked the question about were they on the deeper reefs, and, I mean, we have seen them on the videos that have come off the deep MPA reefs, out in 350 feet of water or so, and I think it was actually the first fish we actually saw, and so they are out there in deeper water, but Marcel is correct that they tend to be -- You see them a lot more in shallower.

DR. SEDBERRY: We have a couple more questions. I do want to point out that it's after five, and so, once again, we're probably going to have to go late today, and I would really like to see if we can finish this up today. Let's continue on with the questions and discussion, but, eventually, we need to get to the action items.

DR. BUCKEL: Thanks, Kevin, for a great presentation. Kevin, this came up in cobia, and that's the only reason that I noticed it. For the projections, you have the Y-axis labeled correctly as removals, but then, in the legend, where you talk about the -- It says "landings" for that 2018 and 2019, I think, which would be removals, correct, because it's not just the landings, but it's both the landed fish and the discards. The same goes for the tables. They are labeled as landings and not removals.

DR. CRAIG: Yes, that's right, and they should be removals. Thanks for pointing that out. I mean, I did go back through the report, and I found a couple other sort of minor editorial things, and so I will probably send over a revised version to Kathleen. Thanks for pointing that out. I will make sure to correct that.

DR. BUCKEL: No problem, and just a quick thing for Wilson on the habitat. It may be something for the Habitat Committee to think about, is the relationship between the juvenile amberjack and the sargassum, and there's been some indication that sargassum is on the increase, and so that may -- It's interesting to see that recruitment is on the increase for amberjack, and so there may be something to -- There may be ongoing research in that area already for that relationship, but, if not, that may be something to consider for a research recommendation.

DR. CRAIG: That's a good point. I hadn't heard the sargassum connection, but, yes, and it does seem like there is indication, from the assessment, of recruitment, since the mid-2000s or so, at least compared to the 1980s and 1990s, and so, yes, I mean, if sargassum has increased over that time, certainly that suggests a potential mechanism that might be associated with that.

DR. SHAROV: I thought I would wait until the discussion, but, since we have Kevin here for answering questions, I will torture him a little bit more and get back to the question that we raised several times over the day today on the effect of natural mortality. Kevin, I want to ask how confident you and the team were in the improvement of the natural mortality estimation using the Charnov methodology, and I'm sorry that I have not familiarized myself with that particular paper, over Lorenzen.

The difference is visually maybe not that substantial, but you essentially have a higher estimate of M at-age, which has a profound effect on the status of the stock, unfortunately, when comparing -- I also know that, compared to the king mackerel, that we talked about earlier today, this species has more ages, like seventeen, and king mackerel had twelve considered, yet you have -- Amberjack was supposed to have lower natural mortality, yet it has higher natural mortality, even though there are more ages within the assessment, even if you compare the -- If you just do Lorenzen's first, but I suspect that the differences are in details of calculations, and I noticed that you are calculating using the reduction in absolutely to like 1.2 percent, and you are scaling it up a little bit, et cetera, and all these minor details will make a difference, but the principal question is was the team confident in the new set of the natural mortality at-age, because that has a substantial effect on the conclusions about the status of the stock. Thank you.

DR. CRAIG: Well, I mean, I think we can say we were as confident as we've been in any of the other estimates of M . I mean, as you know, these M s always --

DR. SHAROV: At least that was honest.

DR. CRAIG: M is always difficult to estimate, and we do it with fairly coarse life-history-based correlates that are derived from other species, and I think that's one of the reasons that some of the tagging recommendations are pretty important, because they provide you an alternative methodology, with a different set of assumptions, to get estimates of M , and so -- I mean, I think, to the extent that -- In terms of Lorenzen versus Charnov, I mean, it's -- I am not sure that I can really evaluate or make some sort of conclusive statement about which is better than the other, other than to say that, you know, some of the studies that were included in Charnov and that were

also included in the Gislason paper that preceded Charnov were based on some perceived limitations of the Lorenzen method, in terms of the studies that were included, and so I think, as I mentioned, the more recent meta-analysis includes more species that are sort of similar in life history from more sort of southern latitudes, the South Atlantic and Gulf of Mexico systems, rather than strictly sort of more temperate, or more temperate, and freshwater systems, and so, to that extent, I would say that it was an improvement.

The one thing I can offer, and I think you can see my screen here, and this wasn't something that played a prominent role in the assessment, but this is a profile over an M vector, and so it's basically a multiplier on the M, and what it does suggest is that there's a minimum around 1.2, 1.25 or so, and so it's suggesting that M is actually a little bit higher than what we would assume, and so, in that sense, given that the Charnov estimate was higher than the Lorenzen estimate, this sort of is consistent with that, that this would suggest that, based on the other sources of information in the model, we see some minimum at maybe a higher M with a multiplier greater than one, and so I think that's what I can offer, that one is the suggestion from this particular assessment.

Based on some ancillary profiling, M may actually be higher than what we had previously thought, as well as just the progression from the Lorenzen to the Charnov paper, which are separated by eight to ten years, I believe, in terms of the studies that were included and how reflective those studies are, you know species that we deal with in the South Atlantic.

DR. SEDBERRY: Wilson, did you have a question?

DR. LANEY: Yes, Mr. Chairman. It's actually a comment to thank Jeff for pointing out the relationship between juvenile amberjack and sargassum, and, for those of you -- I can't remember if I sent the paper out to the SSC or not, but, yes, there has been a great increase in the amount of sargassum out there in recent years, and there was a recent paper in *Science* about it, and so that is a factor that we should definitely keep an eye on.

Then, relative to mortality and greater amberjack, and I will ask Marcel again, because, having seen some of those videos that show greater amberjack and white sharks swimming around the chevron traps, it causes me to perhaps wonder if there might be some adverse interaction between greater amberjack and white sharks, and I don't know. That's just a random thought that popped up there.

DR. SEDBERRY: Thanks, Wilson.

DR. LI: I have a comment or a question regarding natural mortality, and I agree with Alexei that, because the stock status determination is sensitive to the choice of M, based on the sensitivity analysis, I just feel that we need to be very careful to support, or justify, why we use this instead of the other one, and so I am thinking -- Do we have any other studies that are independent of this stock assessment, like reference papers, previous studies, that can support the use of this versus others, and that can ease our concern over the sensitivity of the stock status determination to natural mortality.

DR. SEDBERRY: Kevin, or other modelers, do we have a response for that?

DR. CRAIG: I mean, I guess I can take a crack at that, and I don't know that I can offer much more than I said before. I'm not really aware of any directed studies addressing M for greater amberjack, and I think I mentioned the potential for telemetry, or a tagging-based approach, as an alternative to get at M, but I don't believe that that has actually occurred.

In terms of the justification, I guess I would just go back to what I said previously. If you do believe that there is some progression in science, and we have the Charnov paper, which is a fairly recent paper, which builds on a paper by Gislason a few years earlier that expanded on the Lorenzen approach, which was a few years earlier, presumably, as those methodologies get considered and evaluated, and the number of studies that are included in those meta-analyses increase, then the likelihood of getting a better estimate for any particular species would seem to increase as well, but, beyond that, I don't know of specific studies addressing M in greater amberjack that could be used as support for one method over the other. I actually think it might be harder to justify going with the older method, given the advances that apparently have occurred.

DR. LI: Thank you, Kevin. I mean, here, I appreciate the methodology, and I think the methodology is great, but the question is, based on the methodology, the values that we have here, is it real? Is it really representing what's going on for the species? That's a question. I am not like debating between Lorenzen versus the Charnov. If the methodology is valid, then I believe both methodologies should have come out with similar outcomes, but, the values that we see here, is that real? Is that representative?

DR. CRAIG: I mean, I think that's a valid question. I think we don't have an objective means to answer that. I mean, all we can do is compare different approaches, or different estimates, of M. I don't think we have an objective way to evaluate whether one is better than the other, except to the extent that, as these methods develop and as additional studies get included and so on and so forth, the presumption would be that we would be getting a more informed representation of M for any given stock, particularly as the meta-analysis expand and include more species, and potentially more species similar to those that occur in this region.

MS. LANGE: A couple of things. One, Kevin, related to what Yan has been talking about, and Alexei, how many assessments -- Is the Charnov method the method of choice in recent years at the Center, for stocks with this type of data? Then, two, Yan and Alexei, is there something in the pattern of estimates of natural mortality in this assessment that concerns you? I mean, what is it that's making -- What's raising the question in your minds, I guess is my concern.

DR. CRAIG: Anne, to just the first part of that, I mean, I wouldn't want to say that we're prescriptive, in terms of the particular method, to estimate M, but certainly the trend has been, at least in this region, for the last few assessment cycles, to use Charnov, as opposed to the Gislason method, which occurred earlier, and Lorenzen, which was before that, and so I would say it's certainly pretty common for the assessments that I've done.

I believe for all of the assessments that are ongoing now, including red porgy that you will hear about tomorrow and scamp that is going on as a research track and for cobia that was finished last year, and I did vermilion a couple of years ago, and we used Charnov for that as well, and so I do think there is a gravitation towards the more recent methods, on the assumption that those are improved, but I think it's a valid point that natural mortality is very difficult to get a handle on, and it has incredibly large effects on assessments, and a lot of our decision-making is based on

some sort of assumption or presumption that certain methods, or perhaps more recent methods, are better, and maybe that's something that should actually be evaluated.

I think, fundamentally, if we're using these life-history-based sort of correlates, whether it's Lorenzen or Gislason or Charnov, they aren't going to give us precise estimates of M for any given species in any given system, and I think, if you want that kind of information, then there would have to be an investment in approaches, and the tagging is one approach, to actually generate empirical estimates for a particular species in a particular system, and, outside of that, we're kind of left with the life-history-based correlates, which I think get us in the ballpark, but certainly I don't think we should expect that they are necessarily a real or accurate reflection of the natural mortality that is experienced by particular species in a given system at any given time.

DR. SEDBERRY: Are there any more questions from the committee? I am going to ask for some advice here from the committee and staff. It's almost 5:30, and we still have --

DR. ERRIGO: George, Kai Lorenzen has his hand up.

DR. CRAIG: Yes, we should listen to Kai.

DR. SEDBERRY: Okay.

DR. ERRIGO: Kai, are you there? I am going to put your hand down, because I can't hear you, but, if you are there, you can type into the question box, if you can't talk.

DR. SEDBERRY: In the meanwhile, it's almost 5:30, and we still have all the action items to address for this agenda item, and they are the same action items that we just went over for king mackerel, and probably we'll have a lot of the same issues that we had with king mackerel, and I'm just wondering if we want to try and tackle that and see what we can get done before say six o'clock, or is there a suggestion that we recess and try and take this up in the morning and still fit in red porgy, which is going to be a difficult one, and several other agenda items, and I am asking for some advice here.

DR. REICHERT: I am not sure if I'm going to make myself popular, but I suggest that we try to get at least some of these action items done today, because I believe we will have -- I expect that we will have a lot of discussion for the red porgy assessment tomorrow, and so I think it would be good to get at least some of this done today.

DR. SEDBERRY: I agree with you, and maybe, since we've already discussed some of these items relative to king mackerel, they won't be as contentious for greater amberjack, but you never know until you start into it.

DR. ERRIGO: George, Kai said he is able to talk now, if you want to give it another go.

DR. SEDBERRY: Okay.

DR. LORENZEN: Thanks for allowing me to comment, and I'm actually here to learn about the amberjack, and I didn't tune in to talk about natural mortality, but I wanted to give you a little bit of a perspective, in that it was interesting for me -- So, because I don't usually deal with the South

Atlantic assessments, I hadn't seen the Charnov used, and so, in the Gulf, we're still -- Very much we're doing Lorenzen, and not just Lorenzen, but we're doing Lorenzen and Hoenig, really, and so the way it works is that original 1996 paper is really a paper about the scaling of mortality with size.

Then we are using that scaling, and, typically, we anchor that, in terms of total mortality, using an estimate for mortality at L infinity, or length at maturity, that is derived from Hoenig, and so from that relationship between longevity and natural mortality, and the Gislason and Charnov method really is based on a sort of combined regression of the mortality against length, length and L infinity, and K.

The other thing that has changed is that they are using quite a different set of data, and so they use a subset of the same data, but not very much, and then they use some new data, but the big difference between the dataset that I originally used and the one that they use is that theirs covers a much, much narrower range of length, and so it probably provides less good information on the scaling with length and age, but it does bring in more tropical species, and, in particular, it has a lot of tuna data from the Pacific, and so it's a different dataset that I think has different strengths and weaknesses, and I am not really sure. I think they are two different datasets and two different ways of analyzing them, and I am not really sure that one is necessarily better than the other.

I think we have a natural mortality workshop planned to look at issues like this that was postponed, and it was meant to have been held in Seattle in March of this, and it's been postponed to next year, but I think the bottom line is I would be a little bit careful with a wholesale shift from one to the other, because the Charnov is actually based on a relatively small dataset that involves a very limited length range compared to -- And a limited, actually, longevity range, compared to, length-wise, the Lorenzen, and, age-wise and longevity-wise, the Hoenig, and so I think I would treat it with some caution, but, at the same time, I think that's been quite well taken care of by the sensitivity analyses that have been run for this assessment, and so I'm not suggesting that there's a big problem, but I just think we need to be a bit careful just going from one to the other. Thanks.

DR. SEDBERRY: Thank you. Okay. Any additional comments or questions before we tackle at least part of the action items? I'm sure some of these issues are going to come up again when we do look at these. Okay. I don't see any hands raised. Mike is going to bring up the Google document, and so here is the action items.

As with the previous assessment, there are several action items for each of these, and we would like to have an SSC response to all of them, and so let's just start at the beginning, which the first bullet is reviewing the assessment, and we've had a review of the assessment, and so we need a statement from the SSC if we agree that the assessment addresses the terms of reference to the SSC's satisfaction. We can make that as simple as yes or no. I will note that, in the report, each term of reference is addressed, and there is an italicized response to each one, and so it seems to me that they've done a good job of addressing the terms of reference. Any comment from the committee on that?

MS. LANGE: I agree. They are listed clearly and addressed, I believe.

DR. SEDBERRY: They are, and it's a good way to do that. Any other comments? Great. Next, does the assessment represent the best scientific information available? Again, here's the loaded

phrase, and we can get into that whole term of reference discussion again, but I don't -- I would really rather not. We can either adopt the language we used in the previous one, which we never really came to consensus on, or we can just say, yes, we consider this BSIA or we don't consider it BSIA. What's the pleasure of the group?

DR. REICHERT: I think, since this was a standard, I think that is less -- Given the terms of reference, it was less of an issue, because there was more leeway given to making adjustments to the model, and so I am more comfortable just saying the SSC considers this the best scientific information available.

DR. SEDBERRY: That's a very good point, Marcel. Thank you. Any other committee members have a comment on this? Okay. Seeing none, let's move on. Does the assessment provide an adequate basis for determining stock status and support fishing level recommendations?

DR. SHAROV: I would agree that it does, but conditional on the treatment of the results, the assessment results, of uncertainty, specifically, with respect to the natural mortality range, as we discussed before, which I don't know yet what I would like to offer, but the fact is that the level of natural mortality selected within the base assessment model and results are very specific that the stock is not overfished, and overfishing is not occurring.

If we will accept the possibility that the level of uncertainty is such that it is the M estimates based on the Lorenzen method application, as in the previous assessment, are possible likely whatever - - We don't know what the likelihood is, and then there is a certain probability that the stock status is different. Obviously, the trends are the same, et cetera, but we are dealing here with a somewhat larger uncertainty than the assessment states right now, based on the base model results. I have no specific suggestions of how to formulate this, and I invite the SSC members to do so.

DR. SEDBERRY: Right now, we have the statement that the SSC considers the assessment an adequate basis for determining stock status and supporting fishing level recommendations, with the uncertainties outlined below, and would saying something like that -- Then, when we get down to the uncertainties section, expanding upon the uncertainties associated with natural mortality.

DR. ERRIGO: Uncertainties are -- That is part of what you're going to do, is describe the uncertainties and how they may affect the status of fishing level recommendations.

DR. SEDBERRY: Okay.

DR. ERRIGO: That's part of the next section, and is that what you mean, or do you mean you don't think that you can get fishing level recommendations, or do you think that you need multiple fishing level recommendations, based on each of the different kinds of natural mortality estimates? That affects this section.

DR. SEDBERRY: Fred, do you have some input here, or a question?

DR. SERCHUK: Maybe my input is not going to be well received. My input is no good deed goes unpunished, and here's an assessment where there were lots of sensitivity analyses done. In other assessments that we've had, we don't see the sensitivity analyses, and so we accept it carte

blanche as, okay, this is the estimate of M. M is one of the most uncertain parameters that we have.

This evaluation and assessment I think went above and beyond, in terms of sensitivity analyses, and now we're bringing into it a cause of uncertainty that we normally wouldn't do, and haven't done, for other assessments. Now, maybe we've opened up Pandora's Box here, and maybe that's good, but I think we ought to be consistent. Here, we have lots of information, and lots of sensitivity analyses, pointing out that M does affect the assessments. Others, where we've given a point estimate, or we've continued with the estimate that was done before, we don't say anything, but I want to be very careful about the language here.

I think -- One, I think the assessment does have an adequate basis for determining stock status and supporting fishing level recommendations, and I would leave it at that. I think the SSC needs to think very strategically about how we answer each and every one of these questions across-the-board, so that we don't come back to this de novo every time we have a stock in front of us.

In this case, I thought there was a tremendous amount of work that was done looking at the sensitivities, not only of M, but also of other parameters, and my feeling is, by using a technique that we haven't said boo about for other assessments that we've done, and accepted it for other assessments that we've done, and so I'm concerned about consistency, first and foremost, and I think the lesson that I would like us to learn, or take away, from this exercise is we should be very careful that we're consistent in terms of how we approach filling out these action items from stock to stock. Thank you, Chairman.

DR. SEDBERRY: Thank you, Fred, and that's a good point.

MS. LANGE: I completely agree with Fred. I agree with the statement that Mike has put in here about consider the assessment adequate for supporting fishing level recommendations. We have the uncertainty discussion that, I believe, goes under where Mike is putting it now, as an uncertainty. The assessment -- I mean, I think, as Fred has said, they did a wonderful job on the assessment, and they would respond in every possible way to questions that we on the panel had, and, again, as Fred said, all of the sensitivities are displayed completely in the assessment. I support leaving the comment that Mike has in here, that we support it.

DR. SEDBERRY: Thank you, Anne, and that's what I was trying to say, and I thought that's what Alexei was trying to say too, but let me call on him. He has his hand raised again.

DR. SHAROV: Thank you. I have a great respect for the work that has been done by the team, and by no means am I trying to diminish it. It's a great work, and I'm also very concerned, as Fred is, about the consistency, and I was thinking about that consistency right in between the discussion of the king mackerel earlier and this one, because there has to be consistency, and we don't have it yet in our approach.

This morning, we accepted the assessment where the selection of the natural mortality, in this case, was based on the application of the Lorenzen method, which has been a standard for quite a while, and it is very possible, or it was very likely, that this team could have used Lorenzen's method as well, and we know what the results would have been, and, with the same rigor, we would have defended and said that this is the best scientific information available, and the conclusions that

would be based then on that vector of the natural mortality were also valid, and we just simply would have to describe the level of uncertainty around it, but, yet, the conclusions about the status of the stock would have been really different.

With consistency, we cannot just go from one method in one assessment and say that this is the best approach, and then, two hours later, we say that, well, this method, in this case, is the best approach, and, therefore, we choose this result as well. That concerns me, too. I have no doubts about the quality or the work that has been done here, but the fact is that the -- As Kevin said many times, we really don't have a true ability here to make a judgment about which method is most appropriate for this particular species.

Therefore, we should be considering the uncertainty to a greater extent than just simply saying, well, there is some uncertainty around the eventual mortality estimates, which are addressed in the probability distribution of the B over $BMSY$, and so it's all related to the simulation, et cetera.

DR. SEDBERRY: Thank you, Alexei. I am not sure how to respond. You know, I'm not an assessment scientist, but I'm not bothered by different methods being used for different species, because different species are sometimes very different, but I don't know enough to really say anything.

DR. REICHERT: This is exactly why I was so adamant about adding the terms of reference in the previous assessment, because I kind of saw this coming, because there were two different methods, and I have realized that the Charnov method is now kind of common practice and considered the best scientific information available, and that was not done in the previous assessment, and why it was not done is because of those terms of reference.

I just wanted to mention that, and that's also why, in this assessment, I did not think the terms of reference remark in the above bullet point was essential, but I really appreciate the consistency that Fred and others have brought up, but, in this particular case, I think we are not necessarily inconsistent if we lay out where the uncertainties in both models, or in both assessments, are and that we indicate what the caveats are, and I will leave it at that, because I don't want to belabor the point too much.

DR. SEDBERRY: Thanks, Marcel. Anybody else that would like to weigh-in on this? I can see Alexei's concern about reading the two italicized sentences there, that we consider the assessment an adequate basis, but then the choice of mortality estimate has a large impact, and so have we chosen the right one?

DR. REICHERT: I tried to -- I was thinking how to help in resolving this, because that has a large impact we know, but then what the consequences are, and so is the committee comfortable saying that we consider using the Charnov an improvement over using Lorenzen, given the information that we've gotten earlier here for this assessment? At least then we address that, yes, there was a -- It has a large impact, but also that we consider that an improvement over Lorenzen.

DR. SEDBERRY: That would be a good way to express it if we all agree on that.

DR. REICHERT: I completely realize that it does not address the inherent uncertainty around any estimate of natural mortality, but I just want to throw that out there, and that gives us a little -- At least, as a committee, we are saying a little bit more than just saying that it had a large impact.

DR. SEDBERRY: Right.

DR. SHAROV: I think what I would suggest is a way out of this -- What I would suggest is -- Well, number one, I wanted to respond to what Marcel had just said. I am surprised, and we have just listened to Kai, who had explained as to what the differences are and limitations are between the datasets and approaches, and, based on his information, I would not accept that we have clear evidence of the Charnov application over the Lorenzen method, considering the limitations that he mentioned.

Nonetheless, I think we could address what I was concerned about in the next section and in describing the recent consequences of the assessment uncertainties with regard to status, and that's actually where my concern is, and so, in that section, I would suggest that we mention that the SSC notes that, if the previously-used Lorenzen method of estimating natural mortality was used, the estimated population size and fishing mortality would be substantially different and result in a different definition of the status of the stock, and so we just need to say that, if the previous method was used, and it's still possible that those values are more appropriate, then there is a risk that our interpretation is incorrect and the population status of the biomass is closer to BMSY, or below BMSY, depending on how far or close it is to the Lorenzen-method-based vector of M . If we would address it there, as an uncertainty, then I think we would then do a diligent job in characterizing where the risks are.

DR. SEDBERRY: Okay.

DR. SHAROV: In other words, the simple fact is that this assessment concludes the stock is not overfished, and overfishing is not occurring, and, actually, if you look at the SSB over SSB MSY, I think it only touched the overfishing threshold only once, and so, essentially, for the whole period, it was above the BMSY.

Yet, if you would allow for a moment a thought that the natural mortality estimate could have been the ones based on the Lorenzen method, then the trends of the stock and fishing mortality would be generally the same, but the stock status relative to BMSY would be different, and that is, most of the time, it would be overfished, or at least some of the time it would be overfished, and we just have to characterize it, that there is such a risk, that we're mischaracterizing the natural mortality, and, therefore, there is a risk, some risk, of us being overly optimistic, in whatever terms you want to describe it, but I think that's what needs to be done, but this boundary is more important to mention.

DR. SEDBERRY: Okay. Mike is working on some wording there, and we have several hands raised, and so let me call on Amy next.

DR. SCHUELLER: I was just hoping to see the figure of the -- If I'm not misremembering, I think a likelihood profile over M , and did you show that, Kevin? I don't see it in the report, and I don't see it in the PowerPoint, and we have -- So I assume it's perhaps an extra slide that you put up, and can you put that back up for me?

DR. ERRIGO: I might be able to get that.

DR. CRAIG: Hold on one second, Amy, and let me --

DR. SCHUELLER: I was just trying to remember the scale of that versus the scale of these that we're talking about, and it seemed to me that it was higher.

DR. ERRIGO: What were you looking for?

DR. SCHUELLER: It's a likelihood profile across M, I thought. There is variable or different M values along the X-axis, and it's like a U-shaped plot.

DR. CRAIG: This is actually something that has been ongoing with some folks in the assessment group, and it was done for greater amberjack recently, after the assessment was over, and what it's showing is a profile over a multiplier on M, and so, if you take the M that was used in the base run, which is that Charnov vector that you had seen before, and then you include a parameter in the model that scales that whole curve up or down by some amount, and so it increases it by 5 percent or 10 percent, or decreases it by 80 percent or 70 percent or what have you, if you profile over that multiplier, the minimum is at a value greater than one.

What that is suggesting is that, based on the other sources of information that we have in the amberjack model, they would suggest a value for M that is actually higher than what we assume using the Charnov, and Charnov is higher, based on the calculation, than Lorenzen, and so I think that would argue for a higher natural mortality, based on the other sources of information that are in the model.

If it had gone in the other direction, if the minimum was less than one, then that would suggest that we overestimated M in the model, but the fact that the minimum in the profile of the scaler is greater than one suggests that, even though we used Charnov, which is higher mortality than Lorenzen, it still may not have been high enough, based on the information content in the model.

DR. SCHUELLER: I asked you to put this back up because I thought perhaps this could be used to help add some weight of evidence in this discussion with respect to the fact that the base run for this has Charnov, which is higher, and so I don't -- It's not in the report, is it, and it sounds like it was done afterwards, and so maybe it can be added as part of the SSC report, or I don't know how to handle that exactly, but I think this is a worthwhile thing to put into the report.

DR. CRAIG: As far as I'm concerned, I don't see any reason why it couldn't be added. There is going to be a slightly revised version of the report that takes care of some of the editorial and the typos that have been mentioned. If it would be helpful to have this, I could include it as an addendum to what we have currently, as a way to kind of evaluate the M.

DR. SCHUELLER: I think it's the only thing we have right now, and so --

DR. SEDBERRY: I think maybe that addendum would be useful.

DR. CRAIG: Okay. Well, I can add an addendum that gives just a brief description of this for the file and what was done and how we're interpreting it and stuff.

DR. SEDBERRY: Okay. Thanks. Amy. Was that all you had? We have several additional questioners here.

DR. SCHUELLER: Yes, and I just wanted to put this back on the table for everybody to see and remember and for myself to remember, because I couldn't find it. Thank you.

DR. SEDBERRY: Thank you. Next on my list, I have Fred Serchuk.

DR. SERCHUK: Thank you, Chairman. A number of years ago, Amy Rosenberg, who used to be the Regional Director in the Northeast, published an article called "Fishing for Uncertainty", and the gist of the article was that scientists are very concerned about characterizing all the uncertainties, but it often gets lost in that they don't know what they're talking about, because there is so much uncertainty.

What we put here is going to be read very closely by not only the managers, but anyone else that picks up the report. From my perspective, if we say the assessment represents the best scientific information available, that means the estimates that we're getting in the assessment, or the parameters that were used in the assessment, is the best scientific information available.

When we have statements like, well, if it's not the -- If we didn't use the Charnov M, but we have the Lorenzen M, I am thinking what is a manager to do? You haven't indicated that the -- The take-away message from that is, well, you guys really don't know what you're talking about. On one hand, if you use the Lorenzen M, everything could be overfished and overfishing is going on, but, if you use the Charnov method, everything is copasetic.

I think we need to give the best information possible, and, if the best scientific information available is that the Charnov method was used, and we agree that it's appropriate, then we should base any information, in terms of the status of the stock, on that and not leave so much uncertainty that, when someone reads our script, they are going to say, well, what if it isn't, and how sure are they.

I know I'm giving a little bit of a harangue here, but I think it's important that we state very clearly to the people that are going to be using this information what our best understanding of the assessment is and what that translates into, in terms of the status of the stock and its relationship to the biological reference points. Thank you.

DR. SEDBERRY: Thank you, Fred, and, again, that's a very good point. It's very hard to take these reports to the council and to other people that read them and not have our consensus statements and our conclusions and our recommendations not be perfectly clear, and so we need to say -- We need to express it, I think, in the way that you have said.

DR. REICHERT: I was going to address that, because I was the one who suggested that language of the SSC considers the Charnov method an improvement over the Lorenzen method, and that was an issue with just saying that it would be helpful, in light of this discussion, to at least have the SSC mention whether a method that was used that we discussed relative to other methods was

an improvement, and I like that language that Fred used, something like, however, the SSC considers this method appropriate, and so I initially wanted to delete that for now, but maybe we can say that that's appropriate.

One more broad thing is that I think we all know that, every time, and especially if every time natural mortality is further explored, we seem to have the same discussion, and so I know we had several natural mortality workshops in the past, and same came out with clearer recommendations than others, but perhaps this is something that we as an SSC can discuss so that we don't have to have the same discussions in every stock assessment, although I realize that the effects on the stock status may be different between assessments, but I think we -- I certainly would benefit from a broader discussion about natural mortality itself, outside any particular stock assessment, but I just want to put that forward, and maybe we can consider that as a committee, if others are having similar thoughts. Thank you.

DR. SEDBERRY: Thank you, Marcel, and I think other people are having similar thoughts, and so that workshop, or whatever we need to address this, should be one of our recommendations, and not necessarily a recommendation from this stock assessment, but, from this meeting, an additional recommendation would be for us to have a workshop dealing with natural mortality and some of these other things that keep coming up that are common to all the assessments, but we keep having to discuss them every time.

DR. REICHERT: Yes, and whatever form that has, and perhaps, if we look at the discussions that were had within the various stock assessments themselves, that may provide enough information for us to come up with a more consistent approach, but a workshop or something else, white paper or something, that can help us to avoid having these discussions over and over, and that's irrespective of being consistent and all the other points that others, such as Fred and Amy and Alexei and others, brought up. Thank you.

DR. SEDBERRY: Thank you, Marcel.

DR. BUCKEL: This is déjà vu for me, because we dealt with the same issues with cobia in the review workshop last fall, and so the peer reviewers were very concerned with the switch from Lorenzen to Charnov, because it had similar effects on the outcome for the cobia stock assessment, a change in the status, and so the likelihood profiling that Katie did showed a similar result to what Kevin just showed, and that convinced the peer reviewers that the higher M had support, as well as the sensitivities that were done on M, and so I didn't -- When I presented that to the ASMFC management body, I didn't have any graphs other than that likelihood profile, and that was one of the ones that I did include, just to drive the point home that there was support from the model for the higher M.

If I recall correctly, Erik or Kyle, or maybe Katie, they would discuss that usually the model doesn't have information to where M should be, but, for cobia, there were strong year classes that you could track, and they felt like that maybe that was allowing a good estimate of Z, and then the landings are giving you F, and so that was maybe how the model was able to do that, and maybe something similar is happening here, and Kevin could comment on that, but, for amberjack, we're seeing a similar thing, where you can track these strong cohorts through time, and so, anyway, it's very similar to what happened for cobia, and we stuck with Charnov, for the reasons that I just described.

DR. SEDBERRY: Thanks, Jeff.

DR. SERCHUK: I just want to get a little bit to the language that we're using here. Most people that will read this report have no idea what a Charnov M is or what the Lorenzen M is. I wonder whether we could just begin by saying that natural mortality in this assessment was estimated using a different method than in the previous assessment, and then you can put Charnov M versus Lorenzen M, but those designations, Charnov M and Lorenzen M, mean a lot to us, but they mean nothing to the managers, and I'm just trying to --

DR. ERRIGO: Yes, that's true, and I have them there for you guys' benefit, but, when you report out, you should definitely make them more clear.

DR. SERCHUK: Okay. That was my only point. Thank you.

DR. SEDBERRY: Yes, and it's a very good point, Fred. The clearer we can make this to the council and other users, the better off we are.

MS. LANGE: On that same point, the language that Mike has here after that first sentence, that however the likelihood profile, it's not clear to me that that is what Kevin was saying. Does Kevin have access to this screen? Can he see the wording that's there?

DR. ERRIGO: Everyone should be able to see it now that's on the webinar.

MS. LANGE: Okay. Does that describe accurately what the likelihood profile was saying, Kevin?

DR. CRAIG: I'm just reading what's written here, and so --

MS. LANGE: I wouldn't have worded it this way, but -- It seems confusing the way it's written.

DR. CRAIG: I think that first statement is true, that using Charnov, because it's higher than Lorenzen, suggests that it's a higher productivity, and it might be an overly optimistic view of the stock status than if we had used Lorenzen. What the likelihood profile is saying is kind of what Jeff was just saying, that, given this particular model and the data that we have in the model, the profile is suggesting M may be even higher, by a bit, than Charnov, and so, if anything, the assessment model is potentially conservative with respect to M, and so I think it does provide support for using the higher Charnov-based M in the assessment, as opposed to the Lorenzen.

MS. LANGE: That's my understanding, but, the way it's worded here, it's not clear to me anyway that that's the stated conclusion.

DR. ERRIGO: It says here that the likelihood profile over the Charnov M scalar, which isn't it the scalar for the Charnov --

DR. CRAIG: That's right.

DR. ERRIGO: It suggests that natural mortality should be higher than the Lorenzen M that was done in the previous assessment and perhaps even higher than the Charnov M that was calculated for this assessment. Is it this that is confusing everyone? I can just take that out.

MS. LANGE: Maybe it's just me. It's just that "should be higher", and I'm just not sure how that would be read by people who haven't looked at the plot.

DR. ERRIGO: Well, I didn't want to say is higher.

DR. SEDBERRY: Is likely higher.

DR. ERRIGO: Is likely to be higher.

MS. LANGE: Yes, that's better. Okay.

DR. SEDBERRY: Okay. Are we ready to move on? I know it's getting really late, but we're getting really close to finishing this, or at least having a draft that we can think about overnight.

DR. SERCHUK: Sorry to continue this, but I think the first sentence of the use of Charnov M should be eliminated. We should just state that -- Because I don't think it's overly optimistic, quite frankly, and the second sentence says it's not overly optimistic, and so I would -- I think that we're having confusing statements here.

We say that the Charnov M is the best scientific information available, but then we say the likelihood profile, even of the Charnov M, suggests that M is likely to be even higher than what we have estimated, and is any of that correct?

DR. ERRIGO: Yes, that's correct. That first sentence is just saying -- Because someone pointed out that, if natural mortality is closer to the Lorenzen M, if you use the Charnov M, it will result in an overly optimistic view of the stock status.

DR. SERCHUK: I understand that, but that's like if the queen were king. We have basically agreed, I think, that the Charnov M is an improvement. I won't belabor this anymore, Chairman. Thank you.

DR. ERRIGO: I could take this out.

DR. SCHUELLER: I will just butt in, but I also agree that sentence should be deleted, and I feel like this second bullet should just start with "The likelihood profile over Charnov M", and, of course, we can add in the method change sentence that Fred suggested, but I also think this should be the first bullet. I wasn't sure, Mike, if you wanted us to like edit while you were --

DR. SEDBERRY: So delete the --

DR. SCHUELLER: Yes. Delete what you have highlighted.

DR. SEDBERRY: Delete what's highlighted and then move that bullet up to be the first bullet.

DR. SCHUELLER: Yes, and I think that first bullet can just be subsumed as a caveat into that bullet. Does that help, Fred?

DR. SERCHUK: I would like to see the -- I actually would like to see the SSC considers the Charnov method an appropriate method for use in this assessment, and I think that should go up as the first statement.

DR. SCHUELLER: That's fine with me, too.

DR. SEDBERRY: Yes, and it's a very strong statement. We're getting close there, and I think this draft says what we're trying to say, and it might need a little wordsmithing here and there. Can we move on to the next sub-bullet regarding the methods of addressing uncertainty being consistent?

DR. ERRIGO: I would say we move to that bullet and then jump right to the P*, because that last one lists in order from greatest contribution to most overall assessment uncertainty and comment on the effects of these assessment factors, and we might want to address that later, because that seems like will either take a long time or be trivial, and we can do it later.

DR. SEDBERRY: Okay, and so let's just get a quick consensus on are the methods of addressing uncertainty consistent with SSC expectations and the available information. Yes.

DR. REICHERT: It's definitely consistent with what we've seen in previous assessments, and we can wordsmith later, in terms of the methods that were used.

DR. SEDBERRY: Very good. I agree with Mike that, if we can get to the fishing level recommendations and the application of the ABC control rule, to fill out that table, and then we can come back to --

DR. ERRIGO: I took the liberty of looking up greater amberjack in the MRAG report, and so that's why that one is filled out already.

DR. SEDBERRY: Okay. So the assessment information.

DR. REICHERT: I would say, consistent with the previous assessments, it would be Number 2, because the steepness was fixed.

DR. SEDBERRY: Yes. any disagreement or discussion of that? Marcel has suggested that the assessment information would be rated two, which is two-and-a-half percent.

DR. SERCHUK: Just one comment, Chairman, if I could. In the proposed ABC control rules that we had before, I thought, the last time, it was given a score of one.

DR. ERRIGO: No, it was not. It was given a score of two, because king mackerel used SPR proxies.

DR. SERCHUK: I am talking about the greater amberjack. If you go to the table that you have there, that you sent out, it has a score of one for Tier 1. Am I reading it wrong?

DR. ERRIGO: I am sorry. Greater amberjack wasn't --

DR. SERCHUK: It's Table 2 in what you passed out, proposed South Atlantic Council ABC control rule, page 13.

DR. ERRIGO: The original control rule?

DR. SERCHUK: You provided us with a document.

DR. ERRIGO: I wasn't there when that was done, and I don't know why they determined it that way, and they may have actually not fixed steepness in the original assessment.

DR. SERCHUK: Okay, but I am right that it was given a score, in this particular case, of one, and that may not be appropriate any longer, and I understand that, Mike, but that's okay.

DR. ERRIGO: I didn't look at it. Which table was it? Was it 2?

DR. SERCHUK: It's Table 2 on page 13.

DR. ERRIGO: I can bring it up, actually. Here it is.

DR. SERCHUK: Yes, that's the one. There it is.

DR. ERRIGO: Yes, it was originally given a one, which means I think that it did estimate steepness.

DR. REICHERT: I believe that is correct.

DR. SEDBERRY: Alexei, did you have a question?

DR. SHAROV: Well, yes. I was going to say that I was convinced that it has to be one, and is the fact that the steepness was fixed the only reason for you not starting at a one?

DR. ERRIGO: Yes, and that pretty much means that you're using a proxy, if you're not estimating steepness. That's been how the SSC has been handling this in the past.

DR. REICHERT: Yes, I would agree with that. That's consistent with how we have done it so far.

DR. SHAROV: But don't we have the estimates? The estimates of BMSY here are not a proxy.

DR. ERRIGO: They kind of are, because you're not allowing steepness to vary and be estimated freely, and so you are kind of setting where it's going to be, and so it's -- MSY is correlated with steepness, and so, if you fix steepness, you are basically using the SPR value.

DR. SHAROV: Well, right, but we saw that the effect of steepness variability was in the range of 0.74 to 1.0 is negligible on the assessment results, and so, anyway, I am okay with two, but I thought it was pretty much one, but, to be consistent with the history, then two is fine.

DR. SERCHUK: I agree.

DR. SEDBERRY: Genny, did you have your hand raised?

DR. NESSLAGE: I did, but, as long as we're settling on two, I will put it back down.

DR. SEDBERRY: Okay. I think we're settling on two. Can we move to Tier 2? Okay. The uncertainty characterization. Any thoughts here? Feel free to throw something out to begin the discussion.

DR. REICHERT: I am happy to throw something out. If we are consistent with previous assessments, this would be high, because the environmental conditions were not included in the assessment, other than potentially in some of the standardized methods for the index, where temperature may be included, but the environmental conditions are not included, and that is, again, consistent with how we have approached it in previous assessments.

DR. SEDBERRY: Thank you, Marcel. It's good to have your historical memory for some of these things.

DR. REICHERT: It's rapidly fading.

DR. SEDBERRY: Yes, that's the problem. The more history you have, the worse your memory gets. Are there any disagreements with setting this at Number 2, high? Okay. I don't see any hands raised. Moving on to Tier 3, stock status.

DR. ERRIGO: That one was Number 1, according to the presentation and according to what the status estimates are.

DR. SEDBERRY: Right. It's neither overfished nor overfishing, and it may have approached benchmarks in the past, but --

DR. ERRIGO: Yes, and it has high biomass and low exploitation.

DR. SEDBERRY: Right. Is everybody happy with that, or at least not unhappy? Okay. I don't see any hands raised, and we can scroll on down, Mike.

DR. ERRIGO: I looked this up in the MRAG report, and it came out to be a low-risk species.

DR. REICHERT: Mike, are you sure? I thought it was medium.

DR. ERRIGO: Yes.

DR. REICHERT: Okay. Well, if we missed something, we can easily look it up.

DR. ERRIGO: Now you've got me --

DR. COLLIER: It should be in that last report that you just had up, Mike.

DR. ERRIGO: Yes. It would be here. Oh, it's a two. That's weird. I just looked it up here. All right. I guess it's a two. Two it is.

DR. REICHERT: Thanks.

DR. SEDBERRY: Okay. Yes, that makes sense.

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

DR. SEDBERRY: Yes, Fred. Go ahead.

DR. SERCHUK: It was two in the last one, because it was based on the previous assessment. Now that we have an assessment that has changed the M value, and we've seen the stock has been above BMSY, and it has been overfished maybe for one year in the time series, we get a different perspective on it than what we had from the last assessment, using the same rationale that we used this morning.

DR. ERRIGO: Okay. I am in the MRAG report, and greater amberjack is definitely low. This is greater amberjack. I don't know how they got a two the last time. If I go up here, to where they have it all written out, here it is, greater amberjack.

DR. SCHUELLER: Mike, wasn't there a method used prior to MRAG for the PSA scores?

DR. ERRIGO: Maybe they used the NMFS PSA analysis?

DR. SCHUELLER: Perhaps.

DR. ERRIGO: I don't know, but I know that the MRAG was used for the ABC control rule, and so I thought that it was MRAG.

DR. SCHUELLER: Well, it seems like MRAG has low, and so that seems like probably what we should go with, because that's what we've been using, correct?

DR. ERRIGO: Yes. We've been using this report.

DR. SCHUELLER: Okay.

DR. ERRIGO: That leaves a P* of 45 percent.

DR. SEDBERRY: So how does the committee feel about that? Is everybody good with this? Any heartburn or objections or won't be able to sleep tonight?

DR. REICHERT: I am good with that, and I need to look at that PSA, because I'm looking at the report that is South Atlantic PSA results, and it says greater amberjack is a medium, and I'm very

comfortable with a low, but it may be good -- I will get in touch with Mike to see where we are using different reports for that designation.

DR. ERRIGO: Yes, and we will come to a consensus and talk with the SSC about it. We'll see if we can figure that out.

DR. REICHERT: Okay. Sounds good, because I am -- I will forward you that report that I am looking at, which is the South Atlantic PSA results. Okay. Thanks.

DR. ERRIGO: Yes.

DR. SEDBERRY: Genny, I thought I saw your hand raised for a second, but maybe you changed your mind.

DR. NESSLAGE: I was just going to say that I will be awaiting the results of the PSA thing, and I'm happy with this, and this sounds great.

DR. SEDBERRY: Okay. Can we address these last two items before we turn into pumpkins?

DR. ERRIGO: Well, rebuilding is -- We don't have to address that.

DR. SEDBERRY: Right. I meant the two major bullets there, the provide advice on monitoring and -- We had several research recommendations in the report and the presentation, and we've mentioned a few things that I hope are in the notes as we've gone along, and I can't recall them right at the moment though, but does anybody have anything they would like to add right now to the monitoring or research recommendations? I don't see any hands raised. That could be a function of the fact that it's 6:35.

DR. ERRIGO: Maybe we can get these offline or tomorrow.

DR. SEDBERRY: We can be thinking about this overnight, and let's look at these last couple of things, last couple of action items, again, and there was one above that we needed to revisit too, first thing in the morning, when we have a fresh brain, or at least a fresher brain, and then we can jump into the red porgy assessment review right after that, and so Nikolai might be able to sleep in another fifteen minutes, or not. Is there anything else we need to go over now, before we recess for the day? Again, I know we need to revisit these action items first thing in the morning, which we will do. Does anybody have anything they would like to say?

DR. ERRIGO: I don't have anything. I was going to say I'm good.

DR. SEDBERRY: Okay. Well, once again, I appreciate everybody's efforts here. This has been a really long and dense and intense day, and I appreciate you hanging in there and all of your input into these assessment reviews, and you will be rewarded by getting to do another one tomorrow, and so yay. I think that's it for today. If you have notes, please send them to Mike and copy me, and we will take this up again tomorrow morning at 8:30 with another stock assessment, the red porgy one. First, we'll finish up these little bits that we have on this one, we'll take up red porgy, and then the other agenda items, SSC business kind of items, that we'll take up tomorrow as well. With that, we're recessed. Thanks, everybody.

MS. LANGE: Thank you very much, Kevin, and everybody else.

DR. SEDBERRY: Kevin, I forgot to mention that. Thanks to all the presenters. These have been really very good presentations. Thank you.

(Whereupon, the meeting recessed on April 29, 2020.)

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APRIL 30, 2020

THURSDAY MORNING SESSION

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The Scientific and Statistical Committee of the South Atlantic Fishery Management Council reconvened via webinar on April 30, 2019 and was called to order by Chairman George Sedberry.

DR. SEDBERRY: Good morning, everybody. I just wanted to again -- First, welcome you back to the third day of our epic SSC meeting and again thank everybody for their -- I am not sure what the word is, durability maybe, but for hanging in yesterday and participating in those discussions, and so, this morning, there's a couple of things. Wilson and Genny have sent out, and I believe they copied the entire SSC, and I'm not sure, but some research recommendations and some other things that they would like to see inserted into the greater amberjack and king mackerel notes, and we can do that, but I don't want to go backwards. I don't want to go back to those and revisit that right now.

We will just insert them, and, if that is something that the SSC feels like we didn't all agree on, we will try to look at it this afternoon, or maybe electronically, and I know that we talked about the things that they would like inserted, but I don't know that we ever came to any agreement about them, and so look at those emails, and, if you have any problems with those, let me know, but, like I said, we will insert them into the report, and then we'll have a chance to edit that later, and I don't want to go back to those two species at this point.

DR. ERRIGO: George, before we're done completely with greater amberjack, I just wanted to go back to the P* and resolve the issue with the MRAG PSA between Marcel and myself.

DR. SEDBERRY: That's right, and I meant to say that I didn't want to go back to king mackerel. We still have to finish up some stuff with amberjack, and so go ahead, Mike.

DR. ERRIGO: All right. That's fine.

DR. SEDBERRY: While you're bringing that up, I will note that there were several research recommendations that are in the report and that were presented yesterday afternoon, and I don't think the SSC disagreed with any of those, and we can just say that we concur with the research recommendations that are in the report and that were presented and enumerate them, if we need to, for our report. If there's any additional ones, we can list those here as well.

DR. ERRIGO: All right, and so what I found out was the report that I had -- I had taken that from old discussions, when the ABC control rule was first being looked at, and it happened to be the draft MRAG report, and so Marcel was looking at the finalized report, and so his was correct, and greater amberjack was a medium, and so the P* actually comes out to be 40 percent, and so, in Tier 4 -- The PSA analysis was a two, which is a 5 percent adjustment.

DR. SEDBERRY: Thanks, Mike. Any discussion of that?

DR. REICHERT: Mike, just to make, Fred was the one who suggested a low, and we discussed that, and so I wanted to make sure that everyone is comfortable with this, because this was based on the report, but Fred and others made some comments to justify the low, and he was correct in saying that we deviated in the king mackerel, and so it would just be good to make sure that everyone is comfortable with this.

DR. ERRIGO: If we do deviate, we will need the justification for that here.

DR. REICHERT: Exactly, and that's why I was mentioning this.

DR. SEDBERRY: So we're talking about Tier 4, and is that correct?

DR. ERRIGO: Yes, that's correct, for the PSA.

DR. SEDBERRY: Right.

DR. SERCHUK: If it's the will of the committee, of course, I will go along with the consensus, but we're now looking at a new assessment that wasn't available for MRAG, and so we have the opportunity again to re-examine each of these tiers in light of the updated information and the updated assessment, and I didn't see any reason, based on the updated information, to do anything other than consider this as a Tier 4, as a one.

I don't know why we're compelled to go to the MRAG report, necessarily, if we have updated information, but I don't want to spend a lot of time on this, because there's little difference for Tier 4 between one and two, but I think we ought to think about whether we have the opportunity, based on an updated assessment, or a new assessment, to deviate from the MRAG. That's all, Chairman. Thank you.

DR. ERRIGO: You always have the opportunity to deviate, but the PSA analysis doesn't necessarily use -- The productivity is most life history information, but, if there is different life history information, you certainly can change it. The susceptibility is mostly fishery information, and so, if there's different discard mortality information and things like that, you most certainly could deviate, based on that, but they use certain criteria, and that's what it was. If those criteria are the same, if you think those criteria are the same, then it would stay the same. If you think those criteria have changed, then you can make a case for it.

DR. SEDBERRY: Fred, do you have a response?

DR. SERCHUK: I do. I mean, we have a new natural mortality, that evaluation, and so that affects the life history, doesn't it, Chair?

DR. SEDBERRY: Yes. I mean, that's one of the life history parameters.

DR. SERCHUK: I don't want to belabor this. I will accept the MRAG. Thank you.

DR. SEDBERRY: Again, we're not just accepting the MRAG. We've looked at the MRAG and all of the other considerations and deciding that we're using the same value, right? We're not basing our decisions solely on the MRAG. I don't see any other hands raised, Mike. I see Jeff.

DR. BUCKEL: Thanks, George. I'm trying to remember, and I was trying to go back through the presentations, but was it amberjack or king mackerel where there was new age at maturity data, and so the age at maturity was younger, and I think it was amberjack, and I think Kevin described the additional small fish samples that decreased that age at maturity, which would also argue for Fred's point of where these -- You've got two -- That would be two life history parameters that have pushed it towards the higher productivity.

DR. ERRIGO: You're correct. That was amberjack.

DR. SEDBERRY: So I guess does that higher productivity warrant us changing this rating to a one, which is low?

MS. LANGE: I concur with Fred. I was thinking the opposite at first, because the PSA is not supposed to be associated directly with the assessment, and that's taken care of in another tier, but, the fact that the natural mortality has changed and the productivity increased, I think one is appropriate.

DR. SEDBERRY: Okay. Mike is making a note of those possible justifications.

DR. REICHERT: I am there with Fred, in terms of I don't want to belabor this, but, if we change those in the scoring, I would be very surprised if amberjack actually makes it into the low score, and so there's a scoring that's going in, and, if we change one or two of these, then it may not move into the low, just as a clarification, but I'm comfortable with either medium or low.

DR. SEDBERRY: Thanks, Marcel.

DR. REICHERT: The current score is an overall score of 3.07, and I think you need to go below a two, I believe, for a low, but I may be wrong with that.

DR. SEDBERRY: Let me see if I can pull that up. Are you looking at the -- I see it.

DR. REICHERT: Anyway, I don't want to drag this out.

DR. SEDBERRY: Okay. Can we -- It seems to me that there are more members that have looked at this that are leaning towards low. Is there anyone that objects to that? All right, Mike. We're sneaking up towards fifty. Mike has made the adjustments to the notes in the Google Doc. Is

everybody good with that? Any objection to that? Very good, Mike. Okay. Are we ready to scroll down?

I think the last two items are the -- We don't need to talk about rebuilding, but the monitoring and research, and, again, there are several research recommendations in the report and in the presentation by Kevin yesterday, and I don't -- I think that the SSC can concur that those are good research recommendations. Wilson sent out some additional habitat-related recommendations in an email to the committee, and, if there's no objections to what he has suggested, we can add those in as well. Is there anything that needs to be done in terms of monitoring? I don't see any hands raised.

DR. REICHERT: I want to make the same remark as I did for the previous one. Usually, the SSC recommends the potential timing and type of the next assessment, and we can add that here, and I would say, given the uncertainty, maybe within the next three to five years, get an update, but that's just to throw something out there, and then we can detail that while we are writing the report, but that is something that we usually provide the council.

DR. SEDBERRY: So that would be an operational assessment in three to five years, or do we call them --

DR. REICHERT: Yes, because this was a standard, and, given the amount of data, I am not sure if we need a research track, but other members may have a different opinion.

DR. LANEY: I was just going to say that, in terms of monitoring, it's rather important to keep SEAMAP alive, healthy, and well, especially since the juvenile bycatch data are derived from the SEAMAP program, and I don't know whether that's something that we traditionally would stick in here, but I think that's something we should think about, especially in view of the continued funding issues that we have with SEAMAP, and we may want to say something about NEAMAP as well, because, to the extent that greater amberjack could be influenced and shift its distribution as a result of increasing temperature, they may start picking it up in the NEAMAP survey, also.

DR. SEDBERRY: I think we captured that under king mackerel, where the SEAMAP trawl survey is used as an index for king mackerel, and I don't know that there is any amberjack data in SEAMAP, but everything is starting to run together.

DR. LANEY: That's a good point, and I was thinking king mackerel and not amberjack, but maybe Marcel can say. Does SEAMAP catch any amberjack, and certainly, if not, the SEAMAP trawl program, certainly the Southeast fishery-independent survey, because of the chevron video data, for sure, because, I think, if I remember right, didn't Kevin say that they looked at those data, but that time series wasn't long enough yet, and so, maybe by the next assessment, they would have a long enough time series for those data to be useful.

DR. REICHERT: Yes, you're right. SEAMAP also supports -- MARMAP and SEAMAP both fund the reef fish survey, and that's where the fishery-independent information for greater amberjack is coming from, and so I would agree with that, and I think that is captured in the research recommendations that was in the report, but we can strengthen that by saying that, obviously, we need to strengthen the fishery-independent information to strengthen the future assessments, and that is both true for the short bottom longline survey and the video survey, and I

think the determining length in the video survey is a high priority, and I think that was captured in the report.

DR. SEDBERRY: The SERFS survey is mentioned specifically in the report, but it doesn't hurt for the SSC to weigh-in on that as well, and so thanks. Okay. Anything else with greater amberjack? Mike will fill in the table.

DR. LANEY: You already said it, Mr. Chairman, but I will just put it in there with my voice for the record, that I did send those additional habitat-related recommendations out to the committee, and so, if anybody has any suggested refinements or comments on those, I guess the way to proceed, since they're already in writing and sent to Mike, that, if anyone has any suggestions for change, they can just go ahead and send them in as we review the draft report.

DR. SEDBERRY: That's a good idea, and Mike is updating the draft report now in the evenings, and all of us have access to that Google Doc and can make suggestions directly on that Google Doc, or you can send the suggestions to me and Mike.

DR. BUCKEL: I just wanted to mention that these research recommendations are even more important. If folks don't know, there is a competition between Sea Grant agencies right now to host an amberjack research program, and that's going to be -- I think it's anticipated that there's going to be \$10 million available just for greater amberjack research in the Gulf and South Atlantic, and so I know there are -- Let me read it. The goal of the research program is to develop additional data sources, assessment approaches, and knowledge to improve agency and agency-independent estimates of the abundance of greater amberjack throughout the range. These research recommendations will probably be looked at by folks putting in proposals to that pot of money, and so we should definitely be thorough here, given that that funding is going to come down the pipe soon.

DR. SEDBERRY: Thanks for that information, Jeff, and so that makes me think it would be worthwhile to just copy and paste the recommendations from the report, since we concur with those, and Kevin added those in his presentation, and I think they're the same, but we can look, and then, of course, the recommendations that we're making right now, and so, yes, thanks for that information. Anything else? I see where your cursor is flashing there, Mike, and is there a recommended trigger level for these metrics?

DR. ERRIGO: It's just there, and you don't have to -- I am not saying that we need to specifically address all these right this second, because you didn't quite put in indicators or metrics that the council should monitor and the SSC could use, like age composition, monitor the age comps, and then you would say what's the trigger level, if you see that the age comps are starting to truncate, or if you see this or if you see that.

DR. SEDBERRY: I am looking at the research and monitoring recommendations that are in the report, and they have that kind of age monitoring as a recommendation, along with reproductive parameters, maturity, fecundity, and then some specific research recommendations as well. Again, I think what we can do is copy those and put them here, and a couple of them were monitoring suggestions, and most of them are research recommendations, and we can just state that the SSC concurred with the recommendations from the stock assessment report. Does that sound good to everybody? No hands up. Okay.

DR. LANEY: Yes, it sounds good to me.

DR. SEDBERRY: Thanks, Wilson. Anything else leftover in greater amberjack that we need to address before moving on? Mike, I don't see any hands raised, and so I believe we are ready to move on to red porgy.

DR. ERRIGO: Wilson's hand just went up.

DR. SEDBERRY: Go ahead, Wilson.

DR. LANEY: Thank you, Mr. Chairman. Just one question occurred to me for the group, and that is, in reading the greater amberjack account in FishBase early this morning, it did mention that there are some spawning aggregations, and I know that the council has been very interested in establishing SMZs for aggregations for other species, and it indicated, in the FishBase information, that there isn't anything known about such aggregations in U.S. waters, but maybe that's something that we'll come up with a research recommendation to add to our list relative to trying to identify any locations of spawning aggregations and then consider whether or not the council might want to think about SMZs for those.

DR. SEDBERRY: Thanks, Wilson. I think that's a good recommendation. You know, there's Riley's Hump and some other places that have been thought of as being aggregating sites for greater amberjack, and it seems like they seem to be aggregated in south Florida during the spawning season, and so there might be something to that. I think you're right, and I think that is an additional research recommendation, because the spawning special management zone might affect the status of the stock. Okay. Now I see no more hands raised, and so let's tackle red porgy.

Red porgy, and this is Agenda Item Number 7, SEDAR 60, Red Porgy Assessment Review, and the notetakers on this are Wilson, Yan, and Fred Scharf. The documents are 12, 13 and 14, and 12 was in your briefing book, which is the assessment report, and Attachment 13 is the presentation, which we're going to hear shortly and which Mike sent out this morning, and then Attachment 14 is some MARMAP supporting documentation, and I'm not sure where that stands. I can't remember if that's been sent out or not yet.

DR. REICHERT: George, if I may, we were asked to provide some age comps for the two years post-terminal year, to help the Science Center with the projections and to help the SSC with their deliberations about the projections, and so that's what that supplemental document is about.

DR. SEDBERRY: Okay. Thanks. We're asked to review the red porgy standard assessment prepared through SEDAR 60 and provide fishing level recommendations, and red porgy was last assessed in 2012 as an update to SEDAR 1, where the stock was found to be overfished and undergoing overfishing.

There has apparently been very little recovery in the stock, due to what's thought to be a recruitment failure, and this is a standard assessment because of the length of time that has passed between the last update and now, and so there's been a lot of data collected and changes in the assessment methods as well since the last time red porgy was looked at. I think we're going to

have a presentation by Nikolai, who is going to present the assessment report, and, if he is ready, I believe we are ready.

SEDAR 60 RED PORGY ASSESSMENT REVIEW

DR. KLIBANSKY: I will note that I sent a version of this over twenty-four hours ago, but then I sent the slightly modified version to Mike Errigo this morning, because I made some slight modifications and added a couple of things, and so, if you're following along, it will be the version that I sent you this morning, if you're following along on a separate copy.

At any rate, I'm going to be presenting the SEDAR 60 South Atlantic red porgy assessment. Briefly, this is a table of contents, and so I will go through a brief introduction, and we'll go through the assessment history, summary of previous assessments, and a very brief management history, and we'll define "stock unit".

We'll go through the data that were supplied and ultimately used in the assessment, a brief summary of the data being fit, indices of abundance, removals, discard mortality rates, age and length comps, life history information, and then I will go through what changed between the data from the 2012 update to the current assessment.

Then I will go through the stock assessment model, which most of you are pretty familiar with. We'll go through the base model configuration, selectivity, and then we'll talk about the comparison to the previous assessment. The way this presentation is arranged, it's largely organized with methods and results together by section of the presentation, and so I'll go through the stock assessment model, method, and then right into the results, the fits to indices, age compositions, and length compositions. We'll look at estimated selectivities, spawner-recruit curve, landings and discards, and then benchmark time series and management quantities.

DR. SEDBERRY: Nikolai, sorry to interrupt you, but it looks like you have some natural breaks here in your presentation, so that we can take questions between the different sections.

DR. KLIBANSKY: Thanks for pointing that out, and I wasn't sure how you would normally do this, and so we can do it that way, take a break after each section. We probably don't need to do it after the introduction, but --

DR. SEDBERRY: Right, and we're not going to have a long discussion, but just clarifying questions, so that we can understand the presentation. The long discussion will come later.

DR. KLIBANSKY: Okay. Great. Just a note on long discussion, and so I know that I was scheduled until noon. I can go until one, and then, after that, it gets tricky, because, given the current state of the world, I am home with two kids, and my wife has something that she needs to do at one, and so, after one, I won't be there.

DR. SEDBERRY: Thanks for letting us know that we can just alter our lunch plans around that, and so that will work.

DR. KLIBANSKY: All right. Then we'll go through the Monte Carlo bootstrap uncertainty ensemble methods and results and the sensitivity analyses, retrospective analyses, projections, and then we'll go through the summary and conclusions. Then I have a couple of slides showing a post-assessment analysis that we did looking at chevron trap age compositions from 2018 to 2019, and Marcel mentioned that a minute ago, and they provided us with those age comps, and we looked at those.

As was mentioned before I think, the first assessment was in 2001 and 2002, and it was a benchmark assessment, with a 2006 update and a 2012 update, and that brings us now to the 2020 standard assessment. If you have this presentation, if you're looking at on your screen, these are the assessment reports on the SEDAR website. These are links. I didn't put a link to the current assessment report, but there's a few different hyperlinks like that throughout the presentation, and that can help you get to some of the reference materials quickly if you need to.

Prior to SEDAR 1, a rebuilding plan was put into effect for red porgy, and there was a rebuilding timeframe of ten years, beginning in 1991. In the benchmark in 2002, red porgy was found not to be rebuilt, and it was not undergoing overfishing. Subsequent assessments have also found it that was not rebuilt, but overfishing was not occurring, and it actually contrasts with the last assessment, with what George just said, but, from my read, that's been the case through the past assessments. The F over F_{MSY} was below one, and so was SSB over SSB_{MSY} .

This is a brief management history. The detailed management history is provided in the assessment report, in Section I, 2.2, and it's actually linked to that page, and so, if you clicked on that, it will take you to that section. I am just trying to paraphrase what was written in that section, and so correct me if something jumps out at you as being inaccurate.

The commercial fleet's seasonal closures were enacted in 1999 to 2017, and the quotas were enacted in 2006 to 2011, and they switched to annual catch limits and 2012 to 2017. There was a fifty-pound retention limit from 2000 to 2006, and there was a 120-fish retention limit from 2006 to 2017, and I didn't get down to the month of the year when those things occurred, but this retention limit extended over a little bit. A minimum size limit of twelve inches was in place from 1992 to 1999. It went to fourteen inches from 1999 to 2017.

A lot of the recreational regulations are similar, but the seasonal closures were from 1999 to 2000, and quotas were in place from 2010 to 2012, and then ACLs from 2012 to 2017. There were retention limits of five fish from 1999 to 2000, one fish from 2000 to 2006, three fish from 2006 to 2017, and then minimum size limits were at the same times and same sizes as the commercial fleet.

The stock unit here in the assessment extends from the southern boundary of the Gulf of Mexico Fishery Management Council/South Atlantic Council boundary to the South Atlantic/Mid-Atlantic Fishery Council boundary in the north, and these rough boundaries are drawn here in blue. This goes back to the SEDAR 1 in 2002 report.

Now we're getting into the data section, and this slide just shows the summary of data that we're fitting to in the assessment, and I'm just going to show you the data that are actually going into the assessment, and I'm not going to be showing all the data that was submitted. These different data

types are color-coded by fleet, and they are arranged by data type, and so these first few are the landings and discards, age comps, length comps, and then indices.

The top line is commercial handline landings, and that extended from 1972 to 2017, and the trawl landings occurred only at the beginning of the model, from 1972 to 1988, and headboat landings and MRIP landings are available for the entire time series. Then we've got discards, and discards occur later in the time series, or were available later in the time series, and then commercial handline discards were from 1999 to 2017, headboat from 2001 to 2017, MRIP from 1981 to 2017.

For the age comps, commercial handline age comps are available in most years, including 1997, and the headboat age comps were a little more spotty. It started in 1979, and then you can see we're sort of missing years, but then it's pretty consistent, and the sample sizes are higher toward the end of the time series. Chevron trap age comps in 1990 to 2017, and we'll talk about it again later, but we mostly excluded the length comps from this assessment model, but we did use length comps for the commercial trawl, and, when we looked at the numbers of trips per year, they were pretty small, and so we pooled those to a single year, to help us give selectivity to the trawl, and so those are pooled to 1977. This model includes two indices of abundance, headboat from 1973 to 1998 and then chevron trap, chevron trap video index, which I will talk about more, which goes from 1990 to 2017.

DR. SEDBERRY: Nikolai, I'm sorry to interrupt, but, before you go on, we have a hand raised. Alexei, did you have a clarifying question?

DR. SHAROV: Good morning, everyone, and thank you. Nikolai, just to confirm, the size regulations, you indicate sort of the final year is 2017, and I assume it's up to current, right, and it's just because the assessment runs through 2017.

DR. KLIBANSKY: Yes, right, and so Alexei is just referring back to what I mentioned for the size regulations, and, yes, and I just refer to years through the end of the assessment, the terminal year being 2017.

DR. SHAROV: So there hasn't been many changes since 2017. Okay. On the recreational, and I'm sorry that I didn't get thoroughly through the report, and so how come you have the recreational landings starting with 1972, while MRIP actually was available only since 1981? Was there some regional program or something? If you go back to your table, your landings for recreational headboat and landings for recreational MRIP, you show that they're available since 1972, and I was surprised.

DR. KLIBANSKY: I am just plotting what was available, but I think that the MRIP landings prior to that -- I will have to go back to the data, to where the data were provided, but they were sort of extrapolated back in time, to my recollection.

DR. ERRIGO: I think they might have extrapolated based on -- Headboat goes back that far.

DR. KLIBANSKY: Headboat does, yes.

DR. ERRIGO: I think it's MRIP based on headboat.

DR. KLIBANSKY: You can see, when I show the actual landings series, that it's a flat line prior to 1981, and it's clearly not exactly as observed, but --

DR. SHAROV: All right. Thanks.

DR. SEDBERRY: Fred Serchuk, did you have a clarifying question?

DR. SERCHUK: Just to say that, every five years, there was a recreational survey that was conducted prior to the MRIP program, and I know that, in some cases, the extrapolations made from those five-year surveys were often used in assessments, and so there are some data that go back that far. Thank you.

DR. SEDBERRY: Thanks, Fred. Okay. I think we can move forward, Nikolai.

DR. KLIBANSKY: All right, and so now we'll look at the actual indices of abundance, and so there are two indices of abundance, and this first one that I will talk about is the SERFS chevron trap video index that is shown here in red, and so this is a combination of the chevron trap index, which is described in detail in this Working Paper 1 and the newly-developed SERFS video index, which is described in Working Paper 7, and there has been a number of recent SEDAR assessments where we have combined them with the Conn method. The chevron trap index part of that extended from 1990 to 2017, and the video index is more recent, from 2011 to 2017, and so they are combined in this red index, and then the associated uncertainty that's fit to the index in the model is shown in the light red area.

We also did the headboat index, which extends from 1973 to 1998, which, again, is just before all those regulations begin in 1990. I will note that the addition of the video index is new for this assessment, and it's actually one of the items listed in the TORs, to consider using that, and the headboat index is identical to what was used in the previous assessment.

Now we're looking at removals. For commercial landings, we have two fleets, commercial handline, which is largely hook-and-line gears, handline and longline, but it also includes some unreported gear, when we didn't have a gear type available, and we had the trap there, which is a difference between this assessment and previous assessments. Trap was treated as a separate fleet, and so a small proportion of the total landings. Then diving and spear gears are also added into that handline category, and so that's indicated by this red line, and the solid line is indicated as the observed South Atlantic, and then the error bands are drawn out to two standard errors, as indicated in this light red shaded area. Commercial trawl is indicated here as the blue line, and the light blue shaded area is indicating the error bands around that.

This is recreational landings by fleet, and headboat is here as the solid red line, with the associated uncertainty, and here you can see that, based on Alexei's question, that, prior to 1981, we just have a constant value, and I'm forgetting exactly how that was calculated, but that's how the data was provided to me.

Just a note on something that you may notice, and we certainly noticed early on, and others noticed before we even were working on this assessment, but we have this really large spike in 2016, and the MRIP methods have been updated recently, and there are some general differences as well, if you look at the MRIP landings for this assessment and past assessments.

Regarding that peak, we noticed it, and we contacted the data providers, to just double-check that there was nothing out of the ordinary with that and that, as far as we were able to determine, that was the best available data, and there was a pretty extensive discussion about this at the workshop that we had last December, and Mike Errigo actually wrote a working paper, or a reference document, Reference Document 18, and that characterizes the discussion that occurred about that point. Nonetheless, we also did a sensitivity run to evaluate the effect of this point in this assessment.

Here we're looking at discards, at three different discard series, commercial hook-and-line, commercial handline, and I apologize for kind of going back and forth between handline and hook-and-line, and it means the same thing. That is indicated here in the red line, with the associated uncertainty, and the headboat is here in green, and the MRIP discards, and these are all in thousands of fish, and MRIP is in blue, and, again, there's a spike in 2016 in discards, and these are also dealt with in our sensitivity run that we'll get to later. As you can see, there is a fair amount of uncertainty around those values.

I will just note that, like other BAM assessments, other Beaufort assessments, the uncertainty that I'm showing here is not going to fitting the landings in the model, but it does get factored into the uncertainty analysis, the Monte Carlo bootstrap analysis, that we did, and so, with fitting, we're fitting pretty tightly to the landings and discards, basically matching them, but these uncertainty values do go into the uncertainty analysis.

Those are looking at total discards, and we apply discard mortality rates to dead discards, and we have separate rates for the commercial and recreational fleets, and these are newly developed for SEDAR 60. We revisited what had been done all the way back in 2002 for SEDAR 1, and Pulver wrote a working paper, Working Paper 4, on how we characterized discard mortality for the commercial fleet, and then we had a lot of discussion at the workshop, where we came up with three values for the recreational fleet.

While we're talking about it, this came up in a previous assessment, and they actually did the same discard mortality for handline and headboat and then a separate value for MRIP, but, in our discussions, we thought that it seemed more appropriate to use separate values for commercial and recreational, and so those are shown here as 0.53 for commercial handline and 0.41 for the headboat and MRIP discards, and the gray bars that extend above and below those large red bars indicate the error that we incorporated into our MCB analysis, and so we incorporated uncertainty in discard mortality in that analysis, as shown there as those gray bars.

I am not going to show the full age and length comps right here, and I will show them later, when I'm showing fits, but we did fit these three series of age composition data, as I mentioned before, and it's a subset of that figure that I showed before, and, in this assessment, we switched to a preferred method of using calendar ages instead of increment counts, as was used in previous assessments, and that's documented in Working Paper 5 that is linked here to the SEDAR website.

To get to calendar age, you need the increment counts or annual live counts, plus the knowledge of the edge type of the otolith and the time of year that the fish was caught, and so, for some of the chevron trap ages, we actually had to use a predicted edge type to determine a calendar age, and that was a process as well, and we had a lot of discussions about it, and multiple webinars, and

Wally Bublely did a lot of work on putting that together, and he vetted it pretty thoroughly, and he documented it in this Working Paper 9. The only length composition that we ended up using is for the trawl, and, for trawl, we have ten-millimeter total length bins, and they range from 120 to 720 millimeters.

DR. SEDBERRY: Nikolai, sorry to interrupt before we move on, but Fred Serchuk has a clarifying question.

DR. SERCHUK: Thank you. With respect to the landings data in the recreational fishery that were shown in Number 11 and Number 12, is my understanding that, in 2016, there were essentially the same amount of fish landed in the recreational fishery as were discarded in the recreational fishery, and I know that you say, in this graph, that 278,000 fish, approximately 500,000 pounds, were landed, and, if you go to the next figure, which is the discards, you say that, in 2016, in terms of the discards, they were essentially 274,000 fish and almost 500,000 pounds, and so is my understanding correct that, in that year, as many fish were discarded of exactly the same weight as were landed?

DR. KLIBANSKY: I should note the weight is just a number that I pulled out.

DR. SERCHUK: Okay, but the same number of fish were essentially --

DR. KLIBANSKY: The number, that's the data that were provided to us, and that's correct.

DR. SERCHUK: Okay. Thank you for that clarification.

DR. KLIBANSKY: Sure, and I should note that I made that conversion with 1.8 pounds, just to have a conversion to put into pounds, but we don't have actual weights for those fish.

DR. SERCHUK: Okay, and is there any reason why you would expect discarding to be the same amount as landed fish?

DR. KLIBANSKY: I guess if they're fishing for -- I'm not exactly sure, but, if they're fishing in a spot for a longer period of time, then they're meeting their bag limit for something, and maybe they're not necessarily targeting red porgy, and they're fishing for other things, and they could still be discarding a lot.

DR. SERCHUK: Okay. It's just unusual to see the same number of fish, essentially the same number of fish, discarded as were landed. Thank you.

DR. KLIBANSKY: Sure, and I would just note that I'm not sure that that is the actual condition, but that's just what came out of the curve. Can I move on to life history?

DR. SEDBERRY: Yes, go ahead.

DR. CROSSON: I didn't raise my hand, but hopefully I can ask. I didn't quite catch what you said, Nikolai. I understood that Mike has a report on that outlier, but did you also say that you guys ran a sensitivity analysis?

DR. KLIBANSKY: Yes, we did.

DR. CROSSON: What was the result of that?

DR. KLIBANSKY: Well, I will show you later. I will be showing that to you. I will go through the life history information, and so the length-weight conversion equation is unchanged from the previous assessment, and it actually goes back even before that. The von Bertalanffy growth equation was updated with additional ages and also the calendar ages, as shown here, and uncertainty is this thinner line, and the parameters are given here, and so this is for males and females.

Proportion male was updated for SEDAR 60, using additional data and using calendar age, and the relationship is shown here, and these are, of course, protogynous hermaphrodites, and so they start life as female and transition to male, and you can see that, at age-one, they're somewhat under 20 percent male, and then, by nine or ten, they are 100 percent male.

Proportion of males mature is unchanged in the 2012 update, and they were considered all to be mature, and we had some discussion about this at the SEDAR 60 workshop, and we agreed that that was a good approach, and we just don't see many immature males at all.

Proportion of females mature was updated for this assessment, again using calendar age, and we also switched from using time-blocked maturity to a single maturity vector for the entire assessment, and the calculations are documented in Working Paper 2. We also ran a sensitivity to making that decision to go from time-blocked maturity to a single maturity relationship.

Natural mortality was modeled as age-variant in previous assessments, and so this is just a single M value for all ages, and Beaufort stock assessments have been transitioning, and maybe at this point they're all transitioned to age-varying natural mortality, using the Charnov approach, which has been scaled by a constant M of 0.22, which is very close to the previous M of 0.225 from past red porgy assessments, but we did actually go through the process of refiguring that value based on current information about maximum age, and we used a couple of different calculations of M from maximum age, and we actually generated about six different values, and we took the average and then used the upper and lower estimates on that set of estimates to be our endpoints for rescaling M in the Monte Carlo bootstrap analysis, and so the lower value is 0.14, and the upper value is 0.32. The vector of M that is used in the assessment model is the solid black line and the black dots, and then the upper and lower values are shown bounding the shaded region, and that goes into the Monte Carlo bootstrap analysis.

Some of this now is kind of a review, based on things that I've been saying, but just to be explicit about the differences in data from this assessment to the last, and there are several differences in life history. There are updated estimates of constant natural mortality, based on new estimates, and maximum age. We included age-varying natural mortality, following current SEDAR standards, and we updated most estimates of other life history parameters, including more recent data.

We treated the female maturity at-age as constant over time, instead of different time blocks. One thing I didn't mention before was, in the previous assessment, time of spawning was sort of

effectively a default of January 1, and we changed that to a value of February 1, based on empirical data, and that's what is done with a lot of Beaufort assessments now.

Just a note of something that might be kind of hard to notice otherwise is the amount of uncertainty that was incorporated in M into the uncertainty analysis is a fair bit higher than what was included in the 2012 update, and I mentioned the scaling between 0.14 and 0.32, whereas, previously, the range was only 0.2 to 0.25.

DR. SEDBERRY: Nikolai, before moving on, Wilson Laney has a question.

DR. LANEY: Good morning, Nikolai. What is the new estimate of maximum age for the species?

DR. KLIBANSKY: There was discussion on that, and I believe that the maximum in the maximum age was twenty-five, but the -- I think the maximum age was twenty-five, and I can pull up the values that were used, and it will take me a minute, but we had some discussion about the potential for how maximum age is affected by sampling and so on, and so we actually had three estimates of maximum age, and, of course, one of those is the highest, but we had three estimates of maximum age and two equations that determine M from maximum age, and so we generated six values of M and took an average of those.

One of the tricky things with maximum age is that the chance -- Obviously we have the best readers reading ages that we can, but there's a chance that the fish that you get that has a maximum age - - That there's something funny about that otolith, or there's an issue with age reading, and so we decided to have a range of maximum ages, but the M is ultimately different, based on just a single fish.

DR. SCHARF: Nikolai, just to add to what you were saying, to answer Wilson's question, the bounding we used during the SEDAR was to use twenty, twenty-five, and thirty for Tmax, and so twenty-five was the estimate that we had, and then, based on some of the reasons you just talked about, we used twenty and thirty as the bounds, and then we used different equations, the original Hoenig estimator using Tmax to estimate natural mortality, as well as the modified version that was published by Fenn in 2015.

DR. KLIBANSKY: Thanks for clarifying that, Fred. I appreciate that.

DR. SEDBERRY: Thanks, Fred. I don't see any other hands raised, and so I think we're ready to move on, Nikolai.

DR. KLIBANSKY: Okay. Thanks. Those are the differences in the life history data, and other aspects of the data that changed -- Obviously, there was an additional six years of data in 2012 to 2017, and the previous terminal year was 2011 in the last assessment. A minor thing that had very little effect on the model, but simplified the model itself, was that there is a zero value in landings, in trawl landings, in 1974, which was replaced with the smallest non-zero value in the time series, and that didn't have an effect on the model.

We combined, as I mentioned before, this relatively small amount of commercial trap landings into the commercial handline landings. We excluded most length comp information. In some cases, it seemed to conflict with the age composition data. We pooled the commercial trawl length

composition data into a single comp. In the previous assessment, it was modeled as separate years, and there was an index that was used in the previous assessment, the MARMAP Florida snapper trap index, which was excluded, including the corresponding age and length composition data. There is a sensitivity that will basically show you that it had little effect on the model. That's the end of what I'm going to say about the data, and I'm going to get into the model.

DR. ERRIGO: Real quick, before we jump to the model, I have an answer for Alexei about how the MRIP data was projected, hindcast, and I went back to the SEDAR 1 assessment report, and what they did was they took the average landings from 1981 to 1990 and just used that value for backwards in time. That's all.

DR. SHAROV: Thank you.

DR. KLIBANSKY: Excellent. Okay, and so the model used is coded in ADMB software, and the catch-at-age model is used in most of the recent SEDARs, and so we started with the most recent version of the Beaufort Assessment Model and modified the structure to be similar to the previous assessment, and then, of course, we expanded the model from there to get to the SEDAR 60 assessment model. The timeline for SEDAR 60 begins at the same start year as the previous assessment, 1972, and it goes up through the current terminal year of 2017.

A lot of these inputs that I mentioned, the age-structured life history, they are derived by the age-length relationship, the growth model, age-dependent sex-ratio, age-dependent male maturity, age-dependent female maturity, and natural mortality. In the assessment model, we physically match the landings and discards time series, and they are fit, but they are fit with a very tight CV, and the discards time series, of course, incorporates the estimates of discard mortality rate, and that includes dead discards.

We fit indices of abundance, as I showed you, and age compositions, length compositions, and we're estimating recruitment deviations, in this case for most of the time series, and we estimate initial numbers-at-age deviations, to fit that initial age structure. We estimate fleet-specific fishing mortalities for landings and discards. We estimate a Beverton-Hold stock-recruit function and the selectivity functions, and then, finally, we calculate biological reference points and the stock status estimates.

Just a brief slide on the selectivities, and so, the way the selectivities were structured, for the commercial handline landings, it's the logistic selectivity and two time blocks, and the time blocks in this assessment for selectivity is associated with the timing of the size limits, going from that twelve-inch size limit in 1972 to 1998 and the fourteen-inch size limit from 1999 through the end of the assessment.

For the commercial handline discards, it's a logistic selectivity, and it's just one time block, and it's equal to the commercial handline landings selectivity from the first block. Commercial trawl landings is logistic, and it's also a single time block, and the recreational headboat landings are logistic, and there are three time blocks here, again associated with the size limits, and the reason that there wasn't a third time block for commercial handline was there was just no information in the earliest time block, prior to 1991, or prior to 1992, I should say, to characterize selectivity, and so we just have a selectivity for that block from 1972 to 1998 for commercial handline.

Headboat discards, the selectivity is similar to what was done for handline, which is one block that is equal to the selectivity from the first block of headboat, and the MRIP comps were pretty sparse, and so we're using them in the assessment, and so the selectivity was set equal to the recreational headboat selectivity. The chevron trap survey was also modeled with a logistic selectivity and a single time block.

Other just differences between this assessment and the previous assessment, and I'm not going to get into too much detail on the model, and just realize that it's been documented well. Anyway, the comparison to the previous assessment, here, we're modeling the youngest age at age-one, and the previous assessment treated the age of recruitment as age-zero, and there were actually very few age-zero fish in any of the age composition data, and so, after lots of discussion, we removed that from the model, and so we're modeling the age-one up to age-fourteen-plus.

We changed the way that we initialize numbers at age at 1972, and that was done using the method of what was used in SEDAR 53 for red grouper, where the equilibrium age structure is computed and the deviations at-age are estimated. The growth model in this assessment is fixed, rather than being estimated in the model. It was estimated in the previous assessment. Here, we have almost no length comps, and so there's not a lot of information in the model to determine those growth parameters.

Selectivity of commercial handline, as I mentioned recently, only included two blocks, since there was no age comp data to inform selectivity in that early block, prior to that twelve-inch size limit. We did deviate the form of the chevron trap selectivity, and so, in the previous assessment, it had been dome shaped, and we switched to a flat-top logistic form, and there was a lot of discussion about that, both at the panel at the workshop and from data providers, and we just didn't have any -- We kind of revisited that, but there was no indication that there was any doming going in the traps, and there was no suggestion that large red porgy are being excluded from the trap.

Length at-age comps were fit with Dirichlet multinomial likelihoods, moved to multinomial likelihoods, as was used in the previous assessment, which is a change that we're basically going to for all of our assessments, and data sources that were fitted were not reweighted with user-supplied weights, and so there is lots of discussion at a lot of assessments about what sort of subjective weights to apply to different data sources, and a lot of that reweighting had been done in the previous assessment, and, in this case, the data are, in a sense, weighted by the data themselves, but no additional data weights are applied to the data sources, and so all they did for the age comps and so is just have the same user-supplied weight.

DR. SEDBERRY: Nikolai, before moving on to the next slide, Yan Li has a question.

DR. LI: Thank you, George, and thank you, Nikolai. My question is, because you mentioned the weighting of the data, my question is, when you fit the catch data, do you -- Did you give a CV value or did you just use the value estimated from the data?

DR. KLIBANSKY: When we fit the landings, and I think this answers your question, but, when we fit the landings, the CVs that were used for each year of the landings is actually 0.05, and that's across all fleets and all years. I am going to scroll back for a minute. The CVs that I am showing, or the uncertainty error bars that I'm showing you in these plots around landings, are supplied to us by the data providers, and we use them for our uncertainty analysis, that I will get to a little bit

later, when we generate a bootstrapped landings series, but then, even those runs, when we're fitting, we're fitting them very tightly to the landings, and so we're basically treating the landings as being correct, with a little bit of wiggle room.

DR. LI: Okay, because, looking at the figure here, the variation of the landing data over the years it seems is quite different. The earlier years has very wide standard error, and then, when you go down to the recent years, it's quite narrow, and so I'm just a little bit concerned about this is what the data shows, and then we try to force the model to behave in a certain way, try to fit the catch data well, and we just manipulate the CV that is not reflected in the data, and that's my concern. Thank you.

DR. KLIBANSKY: I guess one thing I will point out about these plots is, because they are just kind of a statistical thing, because they are plotted to the standard errors and they're not showing the CVs, these lower values are -- Their error bands are partly smaller because the value of landings is smaller, whereas these higher values convert CV to standard error, and you get these larger bands, and so it's partly inflated by just the calculation of standard error, but, to your point, I guess I'm going to pass the buck a little bit and just say that what I've been advised with these model fits, from senior stock assessment scientists in our group, is that the model -- There end up being a lot of problems with the model when you try to -- If we don't fit landings fairly tightly, and so that's done in most of our assessments, and I don't know if Erik or Kyle, if they're on the call, want to clarify that decision as something that certainly precedes my time here.

DR. SEDBERRY: Erik Williams has his hand raised.

DR. WILLIAMS: Thank you, George. This seems to be a constant point of confusion with catch-at-age models. Catch-at-age models assume that landings are known perfectly. We fit them in the model as more of a numerical convenience, and this applies to SS, and this applies to BAM, that those landings are assumed to be known in a catch-at-age model, and that's just one of the basic assumptions that goes into these integrated assessment models.

I would say that the step that we do that SS doesn't is we actually incorporate the uncertainty in the landings in our ensemble MCB process, and that's a step that SS does not do, and so, therefore, we're actually accounting for uncertainty in landings in a much more complete way, compared to an SS model, where they just assume the landings are known, and they make no attempt to try and account for the uncertainty in the landings.

DR. SEDBERRY: Thanks, Erik. Okay, Nikolai. I don't see any other hands raised, and I think you can move on to the fit to indices.

DR. KLIBANSKY: All right. Here we are, and so this is the first slide of results of the base model, and this is looking at the SERFS chevron trap video index, and the fits are generally pretty good. This is the recreational headboat index, and I guess I should mention the solid line and the solid points are the fit, and the open circles are showing the observed values of the index, and the error bars are plotted as these purple bars.

The headboat index fit is generally good, and there's a long run of negative residuals toward the end here, and we talked a lot about different ways we could try and remedy that, but, ultimately, I think that this end of the time series overlaps with the chevron trap index, and so I think it's harder

to tell that the -- It thinks that the fit is being sort of pulled down relative to the chevron trap points, and pulled up a bit, but headboats are just sort of the compromise, I think, the tradeoff going on between the headboat and the chevron trap fits, but it's just more obvious in the headboat.

This is the fit to age compositions, and there's a number of slides in here that I have purely for reference, and I won't talk too much about them, and I will move through them kind of quickly, but this shows the commercial handline fits from 1997 to 2011, and they go down in columns in time.

This is the commercial handline fits for age comps from 2012 to 2017, and, again, the solid lines are the estimates from the model, and the open circles are the observed values. This also shows the headboat from 1979 to 1992, and, as I showed you before, there's a bunch of years that the age comps are missing, and so, as you're looking at each panel, note that the year is indicated in the top-right corner, but, basically, they're consecutive.

These are headboat age comps from 1998 to 2017, and then these are the chevron trap age comps from 1990 to 2003, and these are from 2004 to 2017. This is a lonely length comp, the pooled commercial trawl length comps.

I will just go very quickly through the selectivities. You will notice that selectivity by block generally shift from -- They should double their ages over time, as you would expect, given increasing minimum size limits, and this is commercial handline landings on the left and discards on the right. This is commercial trawl landings selectivity, commercial trap catches in numbers of fish. This is headboat landings on the left for those three time blocks, and the beginning of the time block is indicated in the legend, and I will note that the year that is printed on there isn't always the first year that those data were available, and so it just indicates the beginning of the time block.

As I mentioned before, you can see, as I slip back and forth between these, that MRIP selectivities were set equal to the headboat selectivities, and then this is the selectivity for the chevron trap survey, which is just that one block for the whole time series. This is the estimated spawner-recruit curve, and there's a steepness of 0.38, R_0 is 3.43 million, and there's a standard deviation residuals of 0.45.

If you look at this plot, you can see that these higher values, stock size and recruitment, occurred earlier in the time series, and that's sort of hard to tell from here, but the stock size is above 4,000 metric tons, and the metric tons occurred at or before 1980, and so that's slightly off, and this is from 1977, and so these values are before 1980. Then, here, to 2,000 metric tons, is 1981 to 1984, and below 2,000 metric tons is occurring after 1985 or so.

These are the -- We wanted to look at the landings kind of in sequence with some of these other plots at the end, just to kind of look at sort of what is the current landings, and so the landings are increasing in the late 1970s, to a peak in 1982, and then decreasing to kind of a smaller peak in 1990 and increasing to about 2000 and then pretty low throughout the end of the time series. Discards here on the right are a little bit of a different pattern, and there's a scale on this plot that goes up to 200,000 pounds, and this is both in thousand pounds, and this kind of shrinks down to about here on the landings plot. With discards, you get an increase after regulations begin and

then sort of variable peaks in 2002 and 2016, although the 2002 peak is mostly due to the commercial handline, because the 2016 peak is likely due to those MRIP discards.

DR. SEDBERRY: Nikolai, Marcel has a question.

DR. REICHERT: Just quick, the moratorium in 1999 and 2000, the fact that there were landings is because that moratorium kind of overlapped those two years, and so there was a little bit of landings in that year?

DR. KLIBANSKY: I mean, I'm not exactly sure why they occur despite that.

DR. REICHERT: Okay.

DR. KLIBANSKY: But we can look into it more, and I'm just not sure.

DR. REICHERT: Just, off the top of my head, I forgot when the moratorium started and when it ended, and I think it has to do with it kind of not being exactly a calendar year, and so there was a little bit of overlap, but I will look that up. Thanks.

DR. KLIBANSKY: Okay. I got a chat back from Kyle saying yes.

DR. REICHERT: Okay. Thanks.

DR. KLIBANSKY: If you look at landings, the way that -- Landings basically have a peak in 1982, whereas, if you look at the time series of F, F is increasing, but it really doesn't peak until 1990, and it's just sort of where that second landings peak occurred, and then it's decreasing to 2001, to where landings decreased to a low of 2000, and then, after 2000, it's variable, and it's relatively low through the end of the time series, with a peak in 2016, despite low landings, and you have a lot of these Fs in this recent period were still above the -- This is showing the FMSY line, and it's lower, but we still have a number of years where we dip below FMSY.

This is the time series of spawning stock biomass, and SSB is pretty high early on, in the 1970s, and then it drops pretty rapidly between 1980 and 1985 and then kind of more slowly down to about 2000, and then it's increasing gradually after that, and, at about 2012, it starts to drop again pretty quickly, and so the relative low SSB matches the low landings.

This is a slide looking at the age structure, and these are in proportions of numbers of biomass at age, and the colors are indicating the different ages, and the legend is shown on the right, and so the start of the rainbow is the youngest age classes, and the end of the rainbow is the oldest age classes, the fourteen-plus group. The age structure is fairly stable in the 1970s, and then, of course, older fish kind of grew quickly from 1980 to 1985, and it stabilized, and then it begins to expand again around 1991, with kind of a peak expanding through 2017, and so that peak is around 1990.

Recruitment has been pretty high, and it was high over the time series, with a peak in 1977, and it's been relatively stable, with a slight decline to 1990, and, at that point, F is peaking, and SSB is already pretty low, and so, after that, we see a steady decline throughout the time series, with especially low values in the very most recent years. This is the table from the report with the benchmarks and management quantities. This is where I was going to end the section on the base

model. It would be a good time to take questions on the stock assessment model, before getting into uncertainty.

DR. SEDBERRY: Any questions from the committee, clarifying questions? I don't see any hands raised, Nikolai, and so I think you can proceed.

DR. KLIBANSKY: Okay. We have methods on how we factor uncertainty analysis, and so this Monte Carlo bootstrap uncertainty ensemble is a process of randomizing the data inputs and some of the fixed parameters that go into the model, and we developed 4,000 different sets of these randomized inputs, and then we run the base model, and it's the same base model that I was just talking about, but we choose different inputs, and then the outputs from each of those 4,000 runs are summarized, with a little bit of filtering for values that didn't converge or were the extreme.

The values that we do randomize I'm going to go through now, and so landings and discards were resampled from log-normal distributions, which are defined by values that are provided by the data providers, and so that's what I was talking about before when I was saying that we're physically matching the landings and discards in the model, and we actually are incorporating the uncertainty that the data providers are providing us by basically resampling those landings and discards time series for each one of these bootstrap runs and generating new series that are then matched in the fitting process within each Monte Carlo bootstrap run.

Anyway, there's a note that the CVs that were provided to us for the MRIP data were based on a new method that has been developed by the NMFS staff in Miami, and those CVs are pretty similar to the PSEs that you might see for the MRIP landings on the MRIP website and were shown in the plots that I showed before. Just to note, since we talked about that 2016 point, there is a fair amount of uncertainty in those 2016 discard and landings points for MRIP, and that's incorporated into the bootstrap analysis, and so it's basically summarized in the outputs for the model.

Indices are resampled from the log-normal distributions provided by data providers, the same way that the landings and discards are, and the only difference is that we're also using the CVs for the indices to fit the indices. The length and age composition data was resampled with replacement, and so those are all the data inputs that are randomized, and then, for the parameters that we randomized, natural mortality is usually included in this process, and it's an important one, and so the natural mortality scaling value, which in the base model is 0.22, was resampled from truncated normal distribution, which is defined by a mean of 0.22 at the standard deviation, determined by the deviations of the six values that were determined at the workshop, as mentioned before, and that distribution is truncated to a range of 0.14 to 0.32, and so M outside of that range is being used. Also, Ms in the middle of the range, around 0.22, are more likely to be sampled than Ms at the extremes. Discard mortalities were sampled from uniform distributions to ranges determined at the workshop, which for commercial handline is 0.45 to 0.64, and recreational is 0.27 to 0.53. Those are all the sources of uncertainty, according to the model. I just have a couple of plots to look at showing these results, and these are the time series on the left.

DR. ERRIGO: Nikolai, I'm sorry to interrupt, but I just wanted to ask -- The MSST in your presentation, is that 75 percent of SSB MSY?

DR. KLIBANSKY: It was noted to me, shortly before -- A couple of days ago, that the value that we had used -- So the quick answer is no, it's one minus M, which is actually quite close, and so

that would be 78 percent of SSB MSY, and so the history of it is that, initially, I had it at 0.75 times this BMSY, and, at the workshop, and I don't remember now, and I don't remember what the source of it was, but someone said that it should actually be one minus M, and so let's change it to one minus M, and so I guess we'll go to 0.75.

DR. ERRIGO: All right, but they're very similar, and that's fine.

DR. KLIBANSKY: They're very similar, and, unfortunately, neither of them looks great.

DR. SEDBERRY: Nikolai, Yen has a question, also.

DR. LI: Thank you, George, and thank you, Nikolai. My question is do you mind going back to one more slide, where you showed the bootstrap, and I can understand where you bootstrap natural mortality and discard mortality, so it can incorporate uncertainty, related to these two parameters, and then the length and age composition data is resampled from the original data, right?

DR. KLIBANSKY: Right.

DR. LI: But, for the landings and indices, it is sampled from a distribution predetermined by the data provider, and so I am thinking the rationale of doing this is that we sample from a predetermined distribution for those two -- For those two types of data sources that are input into the model, and so I am thinking -- In this way, I am just thinking that we want to explore and fully consider incorporating the variation in the original data. However, by doing this, like you sample from a predetermined distribution, I feel it's not exploring the variation in the original data. Instead, we are kind of fixing, forcing, the data to vary in a certain way. That's my concern. Thank you.

DR. KLIBANSKY: Sure, and I guess I'm not sure I would characterize what we're doing with the landings and discards -- I mean, we're resampling values for each year of landings and discards, assuming that the distribution around that observed value would be log normal. We don't have any of the -- Like, in contrast to length and age composition data, where we have a full distribution of ages in a year, and we can just draw M values from that distribution with replacement for each run, for the landings and discards, we only have one observed value in each year, and so we have to make some assumption about the distribution around landings in that year, which we do by -- It's described by the observed value in the CVs provided by the data providers. I guess, in theory, if you're saying about making some assumption about the distribution, but I don't think we have any more information about the distribution in that year.

DR. LI: Thank you, Nikolai. Okay. I still don't quite understand why we do this, and also the bootstrap itself is not going to generate any new information, because it's way too generous in variation for your output, and, to me, the variation is already there in the data, and so we don't need to generate the variation for output. The bootstrap original -- The original data from the distribution is not going to generate any new -- Obtain any new information from the data itself if we resample the data, and so I still don't understand why we need this. I can understand why we need this for the natural mortality and discard mortality, but not for the data itself, and that's just my comment. Thank you.

DR. KLIBANSKY: Thanks for that. I'm going to just flip back for a minute to a plot that we were looking at before, and so, like for this plot that we're looking at, this is commercial landings over time, and there are -- As you had mentioned before, the uncertainty in landings is greater in the earlier part of the time series than the latter part of the time series, and so, in the process of bootstrapping these values, like we would say this value right here, and this is 1985 maybe, the observed value is right here, but it could be anywhere let's say within this range, and so we draw vectors of landings and discards during the bootstrap process, and we're incorporating this uncertainty, and so some of those vectors are going to go way up and then kind of come down, and then most of them are going to be pretty close to the end. I think, to your point before, doing that is incorporating uncertainty that was not actually in any way incorporated into the assessment model, because the assessment model was just matching those Ms.

DR. LI: Thank you for that. When you bootstrap the landings data, you bootstrap for each year, based on its own distribution?

DR. KLIBANSKY: Right.

DR. LI: Okay. That makes more sense to me now, and then I still think that, instead of doing the bootstrapping thing, like when you fit the data, can you direct input the CV for each year's data point? If you input a CV, a very small CV, for the same value for across years, for all the data points, I am saying can you input the actual CV that is calculated based on the data and then input that to the data point? Like, for example, the 1980 year landings, I input a CV, for example, of like 2 percent, and then, for 2000, I input 0.05 percent, or something like that.

DR. KLIBANSKY: Sure. I mean, we can, and, in fact, we input the CVs as a vector with a value for each year, and I haven't tried to do that. We do it in some assessments, actually, and sometimes the CVs vary from 0.05, but my understanding, based on what Erik was saying before, is the model basically needs to be fit to the landings pretty precisely.

DR. LI: Okay. I just feel that way is more natural, because it comes right from the observation, instead of, later on, it was brought from a predetermined distribution, but that's just my comment. Thank you. That makes sense if you bootstrap for each year individually from its own distribution, instead of one distribution for all years.

DR. KLIBANSKY: I mean, it's not like we just pool all the landings and then just randomly draw each one and we get these crazy time series that would be like -- It's not like you could --

DR. LI: That makes more sense to me, but still I feel like the CV from the observation is better than the bootstrap later, but that's just my thought. Thank you. That makes sense now. Thank you, Nikolai.

DR. KLIBANSKY: Okay.

DR. SEDBERRY: Erik Williams has some input for this, I believe. Go ahead, Erik.

DR. WILLIAMS: Thanks, George. Again, I think our group is going to need to put out a working paper that will be part of SEDAR explaining this, because it seems to be a constant point of confusion. You cannot put in landings with a large amount of error into these models. The models

assume the landings are known. If you try to put in a stream of landings with a lot of uncertainty, the model has no way of knowing what landings should have been, and it is basic input into the model.

There is no information to tell it whether the landings should have been higher or lower. The only way you can put landings in that way is if you have like an effort time series or some other ancillary data, and so landings have to be assumed to be known in these models. We only fit to them with a CV of 0.05 as a numerical convenience. It's just it speeds up the optimization process, rather than -- We could actually put code in there that would fix those landings and just spit out an F automatically, but that causes optimization error sometimes, and so it's just a numerical convenience, but landings in these models are treated as known, with no error, and so that -- Again, apologies for the confusion on everybody's part on that, and that's a failure on our communication, and I think we'll try to put together a SEDAR working paper to make this point a little more clear in the future. Thanks.

DR. SEDBERRY: Thank you, Erik. Fred Serchuk, did you have a clarifying question?

DR. SERCHUK: I did, Chairman. I want to go back, if I could, to the Slide 42, if I could, and I'm looking at the F full time series, and it seems to me that, since 2000, Fs have been about -- Full has been about -- The lowest is maybe 0.17, or 0.18, or maybe 0.13. and my concern about this is, when I try to compare this with results that were given in Slide 5, and I know that's going way back, but Slide 5 gives some information on the three previous assessments, the relationship to F to MSY, and, in those cases, the Fs, the current Fs, in those three years were 0.08 and 0.09, and I don't see anything below 0.1 here. Is this because the recreational data has inflated the Fs? Do you follow what I'm talking about?

If you go back to Slide 5 for a second, and could we do that? I just want to look at sort of the parsimony, in terms of what the new assessment is showing. Okay. So you see that F to FMSY, let's say in 2012, was half of FMSY. That means it was about 0.08, when the previous time it was two-thirds of it, and so that would be about 0.07, and then, if you go back to the previous one, it may have been about 0.09, and those are very low values, and I don't see a value that low on any of the figure, any of the things in the current figure, of the assessment results, which is Figure 42, and I'm just wondering why the scaling has changed. Do you follow?

DR. KLIBANSKY: Right, and so the recreational landings, the MRIP recreational landings have to be adjusted. One thing I will note is that I wrote in the report, and, if you look at where the relationship of F to FMSY historically -- If you look at that from this assessment and compare it to the previous assessments, they also don't totally agree, and so the status in these terminal years from previous assessments are not necessarily the same in the current assessment, and that may be as a function of that as well.

DR. SERCHUK: I understand, but I'm just saying the scaling looks a lot different now, with the most recent data, and I presumed it was because of the recreational landings. Thank you.

DR. KLIBANSKY: Yes. correct. We looked at the assessment model, and we talked about the methods for the Monte Carlo bootstrap process, and here we are looking at uncertainties of SSB to MSST, which is slightly different than the MSST of 75 percent, and it's more 78 percent, but

it's pretty close. Anyhow, I think all I wanted to say about this is basically that the uncertainty of the inputs does manifest a fair amount of uncertainty in the resulting time series.

When we look at the solid lines, the solid lines are the values in the base model, and the dashed lines are the medians from the Monte Carlo bootstrap procedure, and they are very similar, and so it seems to be -- The gray area seems to be fairly well characterizing uncertainty in the base model results.

Here we are looking at a plot of the terminal F status estimates from the Monte Carlo bootstrap analysis, and the stock status is on the Y, and it's mostly in this lower-right quadrant, and there is very little uncertainty in the terminal status for either status of the stock or the fishery. This is showing the stock is overfished and undergoing overfishing.

These are just density plots of the same values of SSB over MSST in the top and the terminal status for F on the bottom, and just to note that, for the current F estimate, for estimating fisheries status, we use the geometric means of the last two years of the assessments, and so this is the geometric mean F from 2015 to 2017. That the is the end of what I was going to say about the uncertainty analysis before moving on to sensitivities.

We ran a number of sensitivities with different values. We have some more standard runs looking at incorporating low and high values in natural mortality, a couple of runs where we used the low and high values of steepness, which in those runs were fixed, in the rest of the assessment model, and, in the bootstrap runs, the Monte Carlo bootstrap analysis, steepness is estimated. We looked at a low fixed value of R_0 to fit the stock-recruit model. We looked at including the MARMAP Florida snapper trap index and the associated age composition data. We looked at including female maturity at-age at time-varying vectors, and so using those female maturities in different time blocks over the assessment.

I mentioned before the headboat index, and this is a little bit of -- We looked at upweighting the headboat index. In the base model, it's given a weight of one, just like the other dataset, and then, as I mentioned, we looked at replacing the 2016 MRIP landings and discard with average values on either side, 2015 and 2017, and so that's one run where we had averaged those values for both landings and discards, and so, basically, those values are just changed, and the models were run as before.

This is looking at results, and this is low and high values of natural mortality, and so the next series of slides is looking at F over FMSY in the top-left, and SSB over MSST is the top-right. Biomass is the bottom-left, and recruitment is in the bottom-right. In all cases, the base model is shown as the solid black line and the solid black dots. The model is fairly sensitive to M, especially a higher M, similarly to steepness, and this is looking at high and low steepness, and it's fairly sensitive to those values of high and low steepness.

Also, the base is fairly sensitive to the low value of R_0 , and you can see the status plots on top, and not so much the biomass and recruitment. It really was not sensitive to the Florida snapper trap index and the age comp data, and you can see there's a little more deviation here, where the index actually occurred.

This is looking at female maturity at-age, and female maturity at-age is a time-varying vector, and so different time blocks, and it doesn't show too much of a difference from the base model. There is none at all in biomass and recruitment. Then we upweighted the headboat index, and there's not too much of a deviation from the base model when it's given a weight of two, but it's fairly different given a weight of three. This run is looking at replacing that 2016 MRIP value with averages from either side, and certainly the F value in 2016 goes down, but, otherwise, there was very little effects.

Here we look at status estimates with F status on the X-axis and the stock status on the Y-axis, and we plot all of these different status estimates from all the different runs, and the model is most sensitive to R0 and low steepness values, and it's moderately sensitive to upweighting the headboat index by a factor of three, added steepness, and low M, and so this group, and these values are very close to this black dot, which represents the base model, or it represents runs that the model isn't very sensitive to, and so clearly upweighting the headboat index by a factor of two, including that time-varying female maturity, including the Florida snapper trap data, and smoothing that 2016.

DR. SEDBERRY: Nikolai, Marcel has a question.

DR. REICHERT: It's more an observation than a question, because I'm still trying to wrap my head about that, but if you could move to one previous slide, I believe. I was a little surprised of the relatively small effect of replacing that 2016 point, and I was wondering if you guys had thought about that and whether you had expected that to be this small or -- For some reason, I had expected that to be a lot bigger influence in the outcome, and so I would just like -- As I said, I'm still trying to wrap my head around this, and maybe you can comment.

DR. KLIBANSKY: I mean, it does have -- It certainly has an effect right here on the F for 2016, but, honestly, I didn't know quite what to expect.

DR. REICHERT: Especially since it looks like the terminal year value didn't change at all.

DR. KLIBANSKY: That may be because of the calculation that is based on the geometric mean, and so I'm sure, if it was just an arithmetic mean, it would be affected more, but the geometric mean is less sensitive to outliers, or extreme values, I should say.

DR. REICHERT: Okay. Thanks. I'm still thinking about this, but I just wanted to mention that and see if you guys had any thoughts. Thanks.

DR. KLIBANSKY: Sure.

DR. SEDBERRY: Alexei has a question as well.

DR. SHAROV: Thank you. I know it's not time for discussion yet, but just a suggestion. The answer to Marcel's question is likely that this high data point in 2016 is next to the terminal year, and so, if it happened much earlier, you would have seen an effect, but, here, it just didn't have time to show effects through the changes in age structure.

DR. KLIBANSKY: Yes, given that it's toward the end.

DR. REICHERT: Okay. That makes sense. It didn't have time enough to affect the biomass yet, because it happened just the year before, and that's kind of the translation of that. Okay. Thanks.

DR. KLIBANSKY: It could affect the forecast projections.

DR. REICHERT: Thanks.

DR. SEDBERRY: Okay, Nikolai. I think we're ready to move on.

DR. KLIBANSKY: Okay. Just a couple of slides on the retrospective analysis, and so, for the retrospective analysis, the process is very similar to the sensitivity analysis, and so, if we just truncate the data in the assessment model to the terminal years, ranging from 2011 to 2016, and then the base model is re-run with the truncated data, and we plot those to look for patterns in the terminal year values.

This plot is very similar in layout to what I showed for the sensitivity analyses, and so the terminal years apply to the large colored dots for 2016, 2015, and 2014, and so to 2011. You can see patterns of substantial over and underestimation, in terms of your estimates of FMSY, here and here, but the terminal values of SSB, MSST, and biomass are consistently underestimated to analogous values from the base run. Some of that may be a function of changes in recruitment over time.

DR. SEDBERRY: Nikolai, I am going to suggest that, before you get into the projections, that we take a break. Maybe you're not through with the retrospective.

DR. KLIBANSKY: No, that was the last -- That's okay.

DR. SEDBERRY: I think, if that's okay with you, let's go ahead and take a break. We've been going at this for over two hours, and then we can start back up at five minutes to eleven.

DR. KLIBANSKY: Yes, that's fine with me.

DR. SEDBERRY: Okay. Great. So we're recessed until 10:55.

(Whereupon, a recess was taken.)

DR. SEDBERRY: We're back. Mike and Nikolai, if you all are ready, we can go forward to finish up the red porgy assessment report, and I think we left off at projections.

DR. KLIBANSKY: We plotted the methods for the projections, and we put together projections as specified in the terms of reference, and the projections were actually made out to 2026, or 2032, which is beyond what was asked for in the TORs, but it was necessary. We projected fishing level changes beginning in 2021, which deviates a little bit from what was in the TORs, but yet it seemed necessary.

Fishing mortality for 2018 to 2020 was set at the F current value, and the projections were fixed F then from 2018 out to -- This is actually out to 2026 or 2032, and so, in the TORs, it specified to determine the OFL to project F based on a P* of 50 percent and then also F equals FMSY, which

is basically the same projection, but we show both of them, and then, to evaluate the existing rebuilding plan, it asks for projections at 75 percent FMSY, and, unfortunately, none of those are particularly rosy, and so we ran an additional projection with F equals zero from 2018 on, and so, again, this slide just says what was done for what years, F equals F current for all projections from 2018 to 2020, and then for the other projections. We set the new F from 2021 to 2026, and then, for the F equals zero projection, we extended it out to 2032.

This slide is here really for your reference, and it just indicates -- This is kind of a legend for the plots on the next few pages. For all the projections, I have a plot that looks like this, but the spawning stock is on the top-left, the top-middle is recruits, the top-right is the F per year, and that ends on the bottom-left, the discards in the bottom-middle, and then the bottom-right is the probability that SSB exceeds SSB MSY, with some reference lines drawn here at a 0.5 and 0.7.

In this first projection, you can see that, the assessment projection is predicting the spawning stock going up, recruits going up, but, ultimately, the probability that SSB exceeds SSB MSY through 2026 is basically nil, and this bottom-right is just one line that we have the uncertainty balance, and I should mention that the gray region -- The uncertainty actually is a function of the uncertainty in the Monte Carlo bootstrap run projections, and we run a deterministic projection, and then we run projections based on the outputs of those Monte Carlo bootstrap runs, which are, again, randomly drawn to get the uncertainty projections.

This was F at $F^* 50$, and you can see that F equals FMSY, and the projections are almost the same. This is projecting F at 75 percent FMSY, and it's a little bit better, and you can see, from the plot, that probability is picking up a little bit toward the end of the range projected here, but not much, and so, with F set at zero from 2021 to 2032, the probability that SSB exceeds SSB MSY goes above 50 percent, just in this last year of 2032.

I included the tables from the report that are associated with each of those projections, and not to get into the specific values, but they're just here for reference and not for discussion, and so this is a table associated with a P^* of 50 FMSY, and this is 75 percent FMSY, and, finally, this is the projection out to 2032, and that's F equals zero.

DR. SEDBERRY: Nikolai, before we move on to your conclusions, we have a couple of questions. Fred Serchuk.

DR. SERCHUK: Thank you, Chair. One of the inputs that will certainly affect the projections is what the recruitment is that's being used, and I presume, but Nikolai can inform me, that -- Are you using a stock-recruitment curve for the recruitment for the out years? If so, are the recruitments that are being estimated then higher than the most recent record low recruitment that we've seen? I look at the recruitment series that you put on in Slide 45, and it seems that we're now at the lowest recruitment values that we've seen in the time series, the last several, and are the recruitments that are going into the projections higher than those?

DR. KLIBANSKY: Right. They are, and so that's something that we expected to talk about, and it's something that we noticed certainly as well, and we figured that we would talk about it afterward. I will put up the slide you're talking about. We're looking at --

DR. SERCHUK: So you can see we're at the lowest recruitment, and another possible projection could be that saying we're going to take the most recent two or three recruitments and presume that recruitment will remain as low as it has been, and I'm just wondering how much that might affect the scenarios for stock rebuilding.

DR. KLIBANSKY: It might be worth kind of holding that thought, if you would, and we can talk about it.

DR. SERCHUK: Okay. Thank you.

DR. KLIBANSKY: I only have a couple more slides, and I think that's going to be something that we definitely need to talk about. Is that okay?

DR. SEDBERRY: That sounds good to me, and Marcel also had a question.

DR. REICHERT: It was the exact same question, and so I will hold off until we get to that point, and so thanks.

DR. SEDBERRY: Okay. Thank you, Marcel.

DR. KLIBANSKY: I just have a couple of brief conclusions. The assessment indicates that red porgy are overfished and experiencing overfishing, and there seems to be little uncertainty in the status of the stock or the fishery from the MCB analysis, and the trends we talked about in F compared to the 1980s and 1990s have been relatively low since 2000, but overfishing is still occurring in recent years.

Landings have been low since 2000, and so has abundance of F compared to the high. A majority of estimated landings from the 1980s and 1990s were from commercial, but, since the early 2000s, larger proportions of red porgy landings have come from the recreational sector in many years, and it is undoubtedly affected, to some degree, by the recalculation of the MRIP landings and discards.

Recruitment has been generally declining throughout the time series, and it's been especially low in recent years, and it's been below average since 2007. The probability of being rebuilt only exceeds 50 percent in the projections at I think it was zero in 2032, and so we just have this slide of a basic summary and conclusions, but then this couple of slides is talking about additional analysis that will be relevant to what gets decided for projections.

We had also noticed not only that recruitments were low toward the end of the time series, but that that last recruitment estimate in 2017 is especially low, and it's the lowest recruitment in the time series, and, fortunately, folks at South Carolina DNR have already done the ageing for red porgy for the 2018 and 2019 data, and so, even though the assessment ended -- The terminal year was 2017, but we had that additional data, and so they supplied us with those data, and we were able to kind of look at a couple of things in that age composition data.

They are pretty large sample sizes. For 2018, it was 361 trips, and we call them trips, but deployments is probably more appropriate for the chevron trap, and it was 353 in 2019, and it was 1,837 fish from 2018 and 1,559 fish in 2019, and so there is age comps for those two years.

We took a look at -- We kind of followed this 2017 age class, to see if that low recruitment signal continued into 2018 and 2019, and it also allowed us to evaluate whether the projected and observed age compositions, particularly the recruits, were -- If the projections were looking good for those recruits or if there maybe there was something else that we needed to do, to project recruitment a different way.

This slide is showing a similar plot to the age comp plots that I showed before. It's four years of 2016, 2017, 2018, and 2019, and, in each panel, predicted values are shown as the dashed lines of the predicted, and the observed data are shown with solid lines and points. Colors, which not only look pretty, but they indicate the year classes, and so, if you were to follow this red dot in 2016, it's the same with the 2016 year class, but it's just then shown in 2017, 2018, and 2019. I should also mention that the predicted values for 2016 and 2017 are coming from the assessment model, whereas, in 2018 and 2019, these dashed lines here are from the projections, and the projections for those years were F current itself, and I think it's from the FMSY projection.

What I got from this is that low recruitment in the 2017 year class, and so here, is also low numbers at age-two and age-three, age-two in 2018 and then age-three in 2019, and so it's seeing that that signal seems to be real. Of course, it could be real versus being like a sampling artifact or something, that for some reason we just weren't getting age-ones in 2017, but it seems like it's carrying through those MARMAP age comps in those years.

It also seems that a projected recruitment in 2018 and 2019, and so these values, is also fairly high compared to what we see in the actual observed MARMAP chevron trap age comps, and you also see actually the projected value for age-twos in 2019, which is a function of this 2018 recruitment, and M is also very high, and so, to Fred's point before, it seems like there is probably a way to evaluate how we project recruitment. That's all I have to say about those comps, and that's the last slide for my presentation.

DR. SEDBERRY: Thank you, Nikolai. That was a very comprehensive, if not depressing, presentation.

DR. KLIBANSKY: I thought we needed more bad news these days.

DR. SEDBERRY: Yes, and I'm sure that we have lots of questions and then general discussion, but, before we do that, I wanted to remind everybody that you're only here until one o'clock, and so we need to hit you up for all that we need from you in the next couple of hours, and, before we do that, I need to take public comment, and so we would like to hear any public comment. As we normally do after the presentation, we take public comment, and so, if there are any members of the public who have any questions or would like to make a comment on the red porgy assessment, please raised your hand.

DR. KLIBANSKY: Can I say one more thing before public comment?

DR. SEDBERRY: Yes, sure.

DR. KLIBANSKY: I just wanted to emphasize that, given that this whole presentation showed you projections and then showed you why some of those projections might not be the best, I just

wanted to emphasize that we're very fortunate to see these age comps, and this stuff is pretty hot off the press, after the projections were done and everything, and so we just didn't get there yet, and so it's not that we just were -- Anyway, this is all pretty new stuff, and so hopefully we'll be able to resolve this today.

DR. SEDBERRY: Yes. Great. Thanks. The only hand I see raised is Marcel's, and he's not public. Okay. I do not see any public commenters with their hand raised, but I do see that Marcel and Erik have theirs raised, and so let's take Marcel first and then Erik.

DR. REICHERT: I just have a brief comment. Especially the 2019 ages, I want to thank Joe Evans and Stephen Long. They have worked really hard to make sure that we could provide those, and Wally, Tracy, and Dawn helped with the analysis, and so I just wanted to, on the record, make sure that I recognized those guys for getting this to Nikolai and his colleagues in such a short time, and so that's all I wanted to say.

DR. SEDBERRY: Thank you, Marcel.

DR. KLIBANSKY: Certainly thanks a lot for doing that. It was a help.

DR. WILLIAMS: I was going to say something similar, and thanks to MARMAP and Marcel and his crew for providing those ages. That's something they don't normally do, and they were glad to accommodate, and I think it does help us understand what's going on in the recent years. The other thing I wanted to mention is, although Nikolai probably has to cut out at one o'clock, Kyle and I are both on this webinar as well, and we're fairly well-versed on this assessment, and so I don't want you to feel too pressured to curtail the conversation too much just because of Nikolai's one o'clock deadline, and so I just wanted to throw that out there.

DR. SEDBERRY: Okay. That's great to know, and I'm glad that you all will be able to stay on and help us. I really appreciate it, and so great. We can just kind of proceed with our usual plan, which is to, again, any final clarifying questions or questions regarding the presentation, and then we will take up the action items. Any questions on the presentation?

DR. SCHARF: Nikolai, can I revisit something in the model output? If you go back, if you're able to go back, to Slide 41, which is the assessment model output of the landings and the discards, and so, if you go back to the question that Fred Serchuk had asked very early in the presentation about the landings and the discards in the recreational sector, particularly that 2016 datapoint, one of the things that you look at, when you look at the MRIP inputs, is that, prior to 2000, and this is back on Slides 10 and 11, if people wanted to look at those, but, prior to 2000, you see that the landings, the recreational landings, estimated by MRIP between like 1981 and 2000 generally exceed the discard estimates by a little bit, and that aligns with the regulatory changes, because you didn't really see -- You saw a bag limit change that dropped in 2000 to one fish, for a good part of that decade, and then it came back up to three fish, but, for a while, it was one fish, and you also have a drop in the size limit, or an increase in the minimum size limit, from twelve to fourteen inches, and so what you see after those changes in 2000 from the MRIP inputs is that the discards are predicted to match the magnitude of the landings, and not just in that one year of 2016, but throughout most of that time period.

Yet, the model seems to predict from 2000, and so the model seems to capture what is happening in the MRIP inputs prior to 2000, where the landings are a little bit higher than the discards, but, if you look after 2000, the pink bars for the discards are more than twice the landings, and sometimes triple the landings, and so I remember talking about this a little bit in the SEDAR workshop, but I just wondered if you could refresh -- Maybe refresh my memory as to what might be causing this and whether you think it was impactful, in terms of the stock status, that the recreational discards are so much higher than the landings in the last two decades, given that the recreational sector is a big source of F in the later part of the time period.

DR. KLIBANSKY: I don't remember the discussion that we had about that, but, just to be sure that we're talking about the same thing, the scales on these plots are really different, and so what we were talking about --

DR. SCHARF: So the scales on these plots aren't in numbers of fish, but they're in landings in weight and then dead discards?

DR. KLIBANSKY: Right, and this is the computed -- In the model, it's the computed discards and weights, and so the units are the same, but the Y-axis here is a tenth of what you see on the left side, and so, really, this is 200,000 pounds, whereas this is two-million pounds, and so 200,000 pounds is only going to -- I am just trying to compare, and it would be nice if they had them all in one plot, but --

DR. SCHARF: No, I see, and so never mind.

DR. KLIBANSKY: There's a table, and I am just thinking -- The best thing is to probably look at the table in the assessment report, if it has both in the same units.

DR. SCHARF: Yes, I remember that. I just wanted to make sure that the MRIP stream was being interpreted correctly.

DR. KLIBANSKY: Yes, and I just note, again, that this is -- The plots that I showed at the beginning of the presentation and the data inputs are live and dead discards, and now we're just looking at the dead discards, which were scaled back by 0.41.

DR. SCHARF: Right, and these dead discards are using the low estimate. In the recreational sector, it's mostly using the sort of 8 percent discard mortality estimate, right?

DR. KLIBANSKY: In this model, it's 0.41 percent, because recall -- Well, anyway, it was 8 percent in the previous assessment, but, in the current assessment, we revisited discard mortality rates and decided on 53 percent for commercial handline and 41 percent for both headboat and MRIP, for recreational. The previous assessment had made judgment that MRIP landings were occurring at much shallower depths, and so they had it separate, but we revisited that, and we didn't think that was appropriate.

DR. SCHARF: Okay. Thank you.

DR. SEDBERRY: Thanks, Fred.

DR. LANEY: I will just remind everybody that I am probably the least qualified person on this whole SSC from a quantitative perspective, and so that precedes my question, and my question is I heard the term “recruitment failure” mentioned earlier, I guess, in this presentation and our discussions, but, from the results of the analysis in the projections, would it be fair to say that the stock is pretty much in total collapse at this point, with little prospect of recovery for the near term anyway? I am trying to think of what sort of public reaction is going to come from this assessment.

DR. KLIBANSKY: I was just going to say that I wouldn't use those exact words, but certainly you can see, from the assessment model, that, at the end of the time series, spawning stock biomass is very low, and it seems that the recruitments have been going down, and some of the lowest recruitment in the time series is in 2017, and that is definitely problematic for the stock in the future.

DR. GRIMES: It's my turn to ask a question that nobody probably knows the answer to, but is it possible that -- I mean, I think back to the times in the early 1970s, when we were first working on reef fish out of the Beaufort Lab, and red porgy were by far the most abundant thing we would see, but is their poor performance, in terms of rebuilding, and specifically with recruitment, somehow related to hermaphroditism? I mean, oftentimes, protogynous species were -- The largest and oldest is males, and they have some complex behavioral system associated with reproduction or something like that, and, I mean, is it possible that we have just -- That the harvesting has disrupted their normal reproductive system? I guess that's kind of a crazy question, but, anyway, I don't know who would -- George, you might be willing to pontificate about that too, and I don't know.

DR. SEDBERRY: Well, I will arm-wrestle Wilson for the least quantitative person on this panel, and certainly the protogyny, as you know, makes assessment difficult and management difficult, and that life history strategy makes them more vulnerable to fishing pressure, and I think that's been shown for many protogynous species, and so that's probably part of this, but you know that we have other protogynous species that seem to react pretty well to management, or at least react some, and red porgy hasn't -- The dial hasn't moved. If it has moved at all, it's moved in the wrong direction, and I am baffled by this.

I can remember, back -- I am going to tell an anecdote, which I really hate, but I'm going to do it anyway, but, back in the late 1990s, I was doing all that work in the eastern Atlantic islands, in the Azores and Madeira Islands, and, back then, the fleet there was building boats just to fish for red porgy, because they were so abundant and so huge, and they wanted to know if the recruitment was coming from over here, and I told them I didn't think that was likely, but there was something weird going on over there, and it was kind of the opposite of what was going on over here. I don't really know what to make of that either, and it's a peculiar fish, and so I don't have an answer to your question, but we have a few hands raised, and so maybe someone else will.

DR. REICHERT: A couple of things. I think the somewhat higher recruitment may be an optimistic sign. The other thing, and we have recently started looking at that, but one of the things that's happening with red porgy is that the life history parameters seem to be rather plastic. There is a lot of plasticity, and we've seen some changes in the life history parameters, in particular transition, maturity, and the growth rates.

Of course, that gets a little complicated. If the growth rate changes, then you also see changes in the life history parameters, but I may go out on a limb to say, and maybe others can chime in here too, but I think what we've seen is a change in the life history parameters to a younger age at maturity and a younger age at transition, and we may have seen kind of a slight bouncing back to some of the values that we've seen earlier in the time change, and I'm not sure how much that affects recruitment, but that may also come into play potentially in like a delay in recruitment recovery, because these fish may start maturity, slowly start maturing, at a larger size or an older age, but, in particular, the recruitment -- Hopefully that's not like a one-time blip, but that may be some positive news, and I'm not sure how much that would ultimately result in the rebuilding or how that would affect the timeline of the rebuilding, but, yes, and, I mean, we've seen, in the fishery-independent index, a gradual decline, especially since 2012, from 2012 to the current year, a gradual decline in our trap CPUE. Of course, that's not a -- There's a lot more going into what's happening in the population, but, anyway, I just wanted to mention those aspects of red porgy life history and population trends.

DR. SEDBERRY: Thanks, Marcel, and one thing that occurs to me that makes this protogynous species different from the other protogynous species that we manage is that it feeds much lower in the food chain. It's not a piscivorous grouper, and its ecology is different than the other protogynous species too, and maybe that's what makes it more vulnerable, because it does feed pretty low on the food chain relative to a grouper, but I really am not sure what's going on.

DR. SERCHUK: I'm going to respond a little bit to Wilson. It's often impolite in interacting, in talking about fisheries, to use the "C word", collapse, but, if we don't want to use that word, we can use "depleted", and, commenting from a background in New England, I am not completely unfamiliar with collapsed and depleted stocks, and the signs that we have here, with spawning stock biomass being at a record low, and recruitment being at a record low, and having an F equals zero scenario where you can barely get back to the spawning stock biomass, SSB MSY, after ten years, and that's under an optimistic recruitment input scenario, by the way, which is the reason that I asked that question earlier, I think this stock is depleted.

I think we -- I think it would be useful for us to think about running a projection, or having a projection run, with the lowest two or three years of recruitment averaged and running that forward, to see whether we could even get back to SSB MSY within ten or eleven years, and I doubt it, and so I think that, if one is concerned about trying to restore this fishery, I think that it's important to point out that, given all the information, fishing mortality -- If I were to make a comment, but, just to throw it out, fishing mortality should be maintained at the lowest possible levels, and perhaps restricted to incidental bycatch. That would be sort of the way forward that I would see for a depleted stock. Thank you, Chair.

DR. SEDBERRY: Thank you, Fred.

DR. NESSLAGE: Nikolai, in the assessment report, it talks a bit about the decisions that were made to change the maturity ogives from having multiple periods to just one static maturity ogive across the time period, and am I characterizing that correctly, and can you talk a little bit about that, because I wonder if there really have been shifts in maturity at-age and they are large over time, at least between the early and the late period, and that can have an impact on how big this decline really is, right, or am I not thinking correctly?

DR. KLIBANSKY: Well, one thing I will add, just to put up the slide that shows the sensitivity analysis that we did including those time-varying female maturity vectors, and so it had actually been developed for -- I should say, for this assessment, initially, we were looking at using those time-varying female maturities as they were in the previous assessment and then adding a new block, a new time block.

That's how we started out, but we had discussion -- Well, and, since I have the slide up, I will note that the choice to include or exclude time-varying maturity didn't have a lot of effect on the assessment output, and we had a discussion in December, at the workshop, about it, and there were different points that were raised, and one was some concern that maybe the changes that were seen in maturity -- Were they blocked in the best way, and were changes that were evident explainable by some -- Was there a biological explanation that we could sort out? It was that they sort of change and then change back, and it wasn't clear that we had, let's say, an obvious relationship between, let's say, fishing pressure, because, if fishing pressure goes up, the size at maturity goes down, or something like that, and it wasn't that clear, because it sort of changed and then changed back over time.

I think that the main reason that we ultimately decided to go with a single vector was a concern that none of our other life history inputs were being varied over time, and so the size of sex change or growth or natural mortality, and so we decided that it was probably better to just use the single vector and deal with the time-varying aspect in sensitivity and then make a research recommendation that it would be something that would definitely be worth looking into for looking at size-varying life history, but not just limited to time-varying maturity of females.

DR. NESSLAGE: Thanks for that. I guess that section in the report really stuck out, and it stuck out to me, because it seems that, if these animals are as plastic as Marcel was saying -- Well, I appreciate that one sensitivity run, and that's probably what you had here, and you're saying that that's not really reflecting what might be going on, if growth and maturity and multiple things, multiple life history aspects, are changing at the same time, and I just worry about the magnitude of -- Our ability to estimate the magnitude of the change between the early and the late period, but I'm not sure at this point, and it sounds like you guys have enough information to go on that this might be the best you've got, and so thanks for your answer.

DR. KLIBANSKY: Sure. I will note that the -- I believe, in Dave Wyanski's working paper, the different maturity functions by time block are available, and so we have that information for female maturity over time, but not for some of those other things.

DR. SEDBERRY: Okay. Is there additional discussion?

DR. LANEY: I would certainly support Fred's suggestion that it would certainly be interesting to see an additional run with an average recruitment value for those last three years or so, and I apologize to everyone for using the "C word", as opposed to the "D word", and it certainly -- I think nobody would deny this stock is definitely depleted, and, of course, most of you know that most of my background is ASMFC related, and so I won't throw out the "M word" at this point in time, but I would certainly agree with Fred that it sure seems that F is going to have to be maintained at an extremely low level, to even have any hope of seeing any sort of rebound at all.

DR. SEDBERRY: Thanks, Wilson.

DR. REICHERT: I agree with Wilson and with Fred, and we -- Also, I wanted to say that Genny was right. I mean, the complicating factor is that, if your growth rates are changing, then that analysis gets a little complicated, but we are currently looking into that, and, obviously, that is something that is important for future assessments, and so we are definitely working on those life history parameters and changes over time, but, yes, I agree with both Fred and Wilson that, looking at the projections, that's probably the situation we are in, in terms of our potential recommendations as to what to do with this stock.

DR. SEDBERRY: Thanks, Marcel.

DR. GRIMES: I know that, in the Gulf of Mexico, they're just generally not as abundant as they were in the Atlantic, or at least that's my impression, but how has this stock performed, this fishery performed, in the Gulf of Mexico, for comparison purposes, and does anybody know the answer to that?

DR. SEDBERRY: That's an interesting question. I don't know the answer, but there might be someone on the line here that does.

DR. LANEY: I wanted to go back to a comment that you made earlier, noting that red porgy feeds a good bit lower on the food chain than some of the other hermaphroditic species, and ask you if there had been, or ask Marcel too, because I guess they do sample benthic species in their SEAMAP program, but has there been any significant alteration in the prey base for red porgy? I know you guys do a lot of diet work, Marcel, and so have you seen any sort of a shift in the diet, or have you see any sort of a shift in the invertebrate composition of the samples that would indicate that there might be a prey issue going on here?

DR. REICHERT: To answer that question, Wilson, we do not sample the benthic community. The only indication we have, but that's very, very coarse, and I don't think that includes a lot of prey items for red porgy, is looking at the videos and the percent cover of like sponges and the like, but that's a very coarse identification, and so I don't think we can say anything about that.

The SEAMAP may collect some more information, but there is virtually no overlap between where we catch red porgy and the SEAMAP trawl survey. Red porgy is generally found way further offshore, and, in terms of diets, we recently have not included red porgy in our diet studies. Also, as you know, diet studies are a very time and cost -- They cost a lot of time and money, and those have been, unfortunately, one of the first things that we had to drop, and so we have not done a recent red porgy diet study, and so, right now, we do not have the data to say anything to address that point in any way, shape, or form.

DR. SEDBERRY: As I'm sure everybody is aware, Chuck Manooch studied the diet of this species back in the 1970s, and then I looked at it again in the early 1980s, and what I found was similar to what Manooch had found, and I did compare the stomach contents with benthic samples, but I can't remember, off the top of my head, what the selectivity was, but, when I said that they feed lower in the food chain, they do, but they also still take some fish and some cephalopods, but, compared to the groupers, they are eating a lot of crustaceans, crabs and shrimp and things that are abundant out there, but lower in the food chain, and, again, I just don't know if that makes any difference to their life history strategy or their ability to take fishing pressure or not.

DR. REICHERT: Does anyone know if red porgy was assessed in the Gulf of Mexico?

DR. SEDBERRY: We never got an answer to that, did we, and I don't know anything about the assessment in the Gulf of Mexico.

DR. COLLIER: Mike Travis has indicated that it's not managed under the Gulf Reef Fish FMP.

DR. REICHERT: Thank you.

DR. SEDBERRY: Interesting.

DR. SCHARF: I guess one observation, and maybe a little bit of sort of a silver lining, maybe, is that, in the stock assessment model output on Slide 44, it does show that, or at least the predictions from the model show, that the age structure is starting to -- It's been slowly starting to rebuild, and it really started back in the early 2000s, when the regulations took place, and so the age structure was really truncated, starting in the mid-1980s, and it continued through most of the 1990s.

Just coming back to Church's point about the protogynous life history, and if we -- If, in response to that age truncation, we saw shifts in sort of the size at sex transition and female reproductive output, and even changes in sex ratio where maybe we were seeing limitations in the number of males, the rebuilding, the slow rebuilding, of the age structure would provide some hope, at least down the road, that, for some of those, the functionality of that life history could maybe be restored, and even Nikolai's sensitivity analysis, looking at the sensitivity to R_0 , which is lifetime female reproductive output, suggests that there is some reproductive impairment, and, if that can be restored, there may be some hope.

I think, back to Fred's point about this being depleted, I don't even know, looking at our current position, in terms of estimates of spawning stock biomass, if -- I question whether it's even realistic that we can even hope to get back to spawning stock biomass levels that we saw in the 1970s and 1980s. Even to get to reasonable SSB levels, such as SSB MSY, or even to achieve the MSST thresholds, it's probably going to take a decade of very, very low F s to help continue to rebuild this age structure, and hopefully restore some reproductive functionality, and so I think just making clear that our position is that the fishery needs to be sort of prepared for a long road on this one. Thanks.

DR. SEDBERRY: Thanks, Fred. Any additional discussion?

DR. LANEY: So I will go ahead and ask the question, and so at what point would, or would it at all, would the SSC consider that a -- Ultimately, it's the council's decision, but we talk about maintaining F at a low level, and what -- How do you achieve that? Is a moratorium recommendation a more effective recommendation?

I mean, again, my history is a lot to do with striped bass, and it worked for that species, but it has a very different life history than this one, and I'm not so sure that -- I can't remember moratoriums having been shown effective in marine systems, and I don't know. I will throw that question out there. If you're down to low F s, versus a moratorium, is one better than the other? Obviously, it would be, from an enforcement perspective -- A moratorium is a more -- Well, some would argue

it's easier to enforce, and others would argue that it's harder to enforce, and I guess you could argue that one both ways, but I would be interested to hear what others think about that question.

DR. SEDBERRY: Well, those are really kind of council things, rather than SSC things.

DR. LANEY: Yes, you're right, George, that they are, but the SSC, or at least my perception of the SSC, and this gets back to some of the points that Fred was making earlier, is we're supposed to give the council the best advice we can, and so I am guessing that SSC members may have an opinion with respect to whether what's best for the stock would be trying to keep F low, knowing that you're still going to have a lot of recreational effort and headboat effort out there, versus just imposing a moratorium, which, in some respects, would be a lot easier to enforce.

DR. SERCHUK: I am going to defer to Shep on this. The stock was considered to be in an overfished condition, but overfishing wasn't occurring, and it didn't rebuild during the first rebuilding plan, and now things have gotten worse, and I don't know whether there was an F rebuild, to try to bring the stock up to SSB MSY, but, if there was one in effect, it hasn't worked, and so I'm just wondering what obligation now the council has, because, even under an F equals zero, with an optimistic recruitment scenario, I think, at least for the immediate future, there is no probability above 50 percent that the stock could be rebuilt in nine years, and I think it only gets up to above 50 percent in the terminal year, and so it seems to me that some measures are going to legally be required of the council.

This is the reason why I am thinking that, and Wilson has indicated an agreement, that we ought to have another run with a low-recruitment input, because I think that will show that, even with an F of zero, the stock cannot be rebuilt to SSB MSY within a ten-year period, and that suggest to me that a moratorium, or a no-landings restriction, or a no-fish retention, would be about the strategy that you would have to maintain to have some probability of seeing some significant recovery within the next decade. Thank you.

DR. SEDBERRY: Thanks, Fred. Let's hear from Shepherd to this point.

MR. GRIMES: Thank you, Mr. Chairman. I was just going to say that it sounds to me a lot like some sort of management action is needed. I am not familiar enough with the current management status of red porgy, but, if it's under a rebuilding plan, then, much like we recently did with red grouper, if we cannot achieve our rebuilding targets, we're going to have to do something.

The guidelines, just like I believe we did with red grouper, allow for fishing at 75 percent of FMSY throughout the rebuilding period, but it seems like that might even sound too high for this stock, but I would also encourage you, and it seems to me that it's a little premature to be talking about management strategy for it, but we need to decide -- The SSC needs to decide is the assessment good enough and give the ABC recommendation and give the whole picture of where we stand in terms of rebuilding, and then we'll figure out where we have to go in terms of management. Thank you.

DR. SEDBERRY: Very good point, Shepherd. Thank you for that.

DR. REICHERT: Remarks have been made, and so I don't have anything to add, and I think Shep addressed some of that, and so I was going to suggest to take a look at the action points and then take it from there. That's basically what was already discussed.

DR. SEDBERRY: Right. I think we need to evaluate this assessment, by going through the action items, and then take it from there, as you just said. We're kind of at a good stopping place for lunch, if we would like to take up the action items after lunch, and that puts us a little bit behind schedule, but I think we'll be able to finish on time still. Scott, did you have something?

DR. CROSSON: Yes, and just a reminder that we only have Nikolai until one o'clock, right, because of family obligations?

DR. SEDBERRY: Right, but then Erik indicated that he and Kyle are available, and they're very familiar with the assessment and the report, and they would be able to --

DR. CROSSON: My apologies. I had to step out of the room. Okay.

DR. SEDBERRY: Okay.

DR. KLIBANSKY: I can deputize Kyle and Erik.

DR. REICHERT: Originally, we were supposed to come back at 1:30, and it's a little before twelve, and, again, I'm not sure if I'm going to make myself very popular, but would it be possible maybe to come back at one o'clock? That gives us a little extra time.

DR. SEDBERRY: I think so too, and we don't need to travel anywhere for lunch, and so that's a very reasonable request, and so that's what I would like to suggest too, that we start back up right at one and finish up, or at least do the action items associated with this assessment and then take on the rest of the agenda items this afternoon, but, before we recess, we have a couple of hands raised.

DR. SERCHUK: I know that Erik is willing to step in, but I wondered if we could just go to 12:30, Chairman. This way, we would still be able to have Nikolai here, and I don't think that's -- Because I think we can make a lot of progress on the points while he's still here. Thank you.

DR. SEDBERRY: That's a good suggestion, I think. If Nikolai can do that, we'll go until 12:30 and then break from 12:30 to 1:30.

DR. REICHERT: I'm okay with that.

DR. SEDBERRY: Okay.

DR. SCHARF: Two things. I actually have to participate in a virtual department graduation from about 12:00 to 12:30, and so I won't be able to stay on for this next thirty-minute discussion, which is okay. I mean, I'm okay with the group proceeding without me, but I was going to say, if we were going to break now, I just wanted to reiterate just sort of a point that Fred made yesterday just regarding the SEDAR process, and so I was one of the SSC reps on this SEDAR, along with George.

I just wanted to say that it was a really good experience, and I wanted to thank Nikolai for all the work that he put into this, as well as Erik and Kyle and their participation in it. We had a lot of really good collaborative, productive discussion. Folks from all the states were there, and Mike and Kathleen from the council were there, and so, even though it was a depressing and challenging outcome, the SEDAR process was really productive and collaborative, and I just wanted to make that note.

DR. SEDBERRY: I agree with you that there's been a tremendous amount of work put into this, and under the close deadlines and everything else happening in the world, and so I appreciate everyone's -- Nikolai and Kyle and everyone's efforts on this assessment. Let's look at the action items and see how many we can get through before 12:30. Again, we have several action items similar, if not identical, to the action items that we had on the previous two assessments, and the committee needs to address each one.

The first one is review the assessment, which we had an excellent presentation from Nikolai on, and the first question associated with that review is does the assessment address the terms of reference to the SSC's satisfaction? The terms of reference are included in the report, and there are statements addressing each TOR, again in italics, in the report, and it seems to me that, yes, the assessment does address the terms of reference. Is there anyone on the committee that disagrees or has a comment?

DR. LANEY: I concur, and the answer is yes, but where do we put Fred's suggestion for doing at least one more run with that average of the most recent three years of recruitment plugged in there? Is that something we put in further down, or should that go as a qualification here?

DR. ERRIGO: That, I think, will go further down, and, specially, there is talk of the fishing level recommendations and rebuilding and all that, and you can put it down there.

DR. LANEY: Okay. Thanks, Mike.

DR. SEDBERRY: Mike has typed in our kind of standard response there to the terms of reference, and the next item is does the assessment represent the best scientific information available? Discussion?

DR. SERCHUK: Yes.

DR. SEDBERRY: Very good.

DR. NESSLAGE: I concur with Fred.

DR. GRIMES: I agree, too.

DR. SEDBERRY: The only thing I had in my notes here is that there's a Table 18 in the report that is never referred to in the text, and I can't remember what my confusion was about that, but I will look into that later. Does anyone not agree that the assessment represents the best scientific information available? Okay. Does the assessment provide an adequate basis for determining stock status and supporting fishing level recommendations? Discussion?

DR. REICHERT: I think this would possibly be a place to refer to a potential additional projection run.

DR. SEDBERRY: Right, because this -- That additional run is in reference to fishing level recommendations with --

DR. ERRIGO: If you're going to use the assessment to provide you with your fishing level recommendations, to support the fishing level recommendations, then I would say, yes, it will provide an adequate basis. I was thinking, down here, where you actually provide fishing level recommendations, your recommendation would be to use a different recruitment. I mean, that was my recommendation, and you don't have to.

DR. REICHERT: No, that makes perfect sense. Thanks.

DR. SEDBERRY: I am good with that too, and any objection? Okay. So we agree that the assessment provides an adequate basis for determining stock status and supporting the fishing level recommendations. Okay. The next bullet deals with the uncertainties, and so the first sub-bullet is review, summarize, and discuss the factors of this assessment that affect the reliability of estimates of stock status and fishing level recommendations. Discussion?

DR. LANEY: Do we want to say something about recruitment here?

DR. SEDBERRY: Right, and it was the average recruitment, or the time period chosen for recruitment, and what was your wording on that, Wilson?

DR. LANEY: I think Fred had suggested that we maybe look at an average of the last three years, or maybe a longer period, and I'm not sure. Fred, do you want to weigh-in on that, Fred Serchuk?

DR. SERCHUK: I will. Typically, the average of the last three years of recruitment is often used in stock size projections, particularly when the pattern of recruitment suggests that recruitment has been very low, and it's simply because the stock-recruitment curve then often tends to overestimate recruitment relative to what we have most recently seen, and so my feeling for asking for that low recruitment was it may impact whether the stock has any chance whatsoever of being rebuilt within the ten-year period.

In the case that we have, the recruitment scenario that's provided says that, by the tenth year, there is a greater than 50 percent chance in that tenth year, and my feeling is, if the projections were re-run, there would be no possibility whatsoever that you would attain SSB MSY after a ten-year period of no catches, no harvest. That's why I asked for that scenario to be done. Thank you.

DR. SEDBERRY: Okay, and so the factor that affects the reliability of the estimates is the recruitment values that were chosen and that the additional scenarios need to be run with different recruitment values. What Mike has written there is good, but it's not a factor, and I'm trying to describe what the factor is.

DR. SERCHUK: Can I make a suggestion, Chairman? It was suggested that an additional projection run be performed with input recruitment being equal to the average of the most recent three years, because those are the lowest on record.

DR. ERRIGO: I will put that in. I was going to put that in here. Is that okay?

DR. SERCHUK: It's okay with me.

DR. ERRIGO: Is adequate rebuilding progress being made, and comment on reasons why progress projections --

DR. SEDBERRY: That seems good.

DR. ERRIGO: It's under where we're applying the control rule. That's where it talks about rebuilding, and I will put that in here down here, and so this is just asking what the uncertainties are.

DR. SEDBERRY: Right.

DR. ERRIGO: The uncertainty here is on the choice of the recruitment pattern used in the projections.

DR. SEDBERRY: Exactly.

DR. ERRIGO: It affects projected fishing levels and the rebuilding status.

DR. SEDBERRY: That's correct. Okay.

DR. ERRIGO: But I will -- We will put that in when we get to fishing level recommendations, and I just want to make sure that I keep everything nice and neat.

DR. SERCHUK: If I could just make one editorial thing. It's just the recruitment pattern used in the projections.

DR. SEDBERRY: Yes.

DR. LANEY: I was going to ask, on another point, if, per Churchill's question, we should indicate that there is some uncertainty here with regard to whether or not fishing pressure may have altered the structure of the stock, male-female ratio, earlier maturation dates, so forth and so on, and whether that has an effect on the ability of the stock to rebuild, and it seems to me that might be another source of uncertainty, because I think that was a good question, or a good observation, that Churchill made, and Churchill may want to weigh-in on this one.

DR. GRIMES: No, I agree with you, and thanks. I don't know how you get at that, but I do think it's an issue in the uncertainty, but I don't know how you --

DR. LANEY: I don't know how you would either, but I think it's certainly a source of conceptual uncertainty anyway.

DR. SEDBERRY: Yes.

DR. ERRIGO: I know that Marcel mentioned that they had data showing the affected age and size at maturity, but was there anything else that you guys actually have data on that you can show other life history parameters that have changed?

DR. REICHERT: No, except for the growth rate.

DR. ERRIGO: And growth rate.

DR. REICHERT: Those are the three that we are currently looking at in a little bit different way, and, as I mentioned before, the interaction between growth rate and size and age at maturity and transition makes that analysis a little complicated, but hopefully we will have some further analysis I would say in the next six months or so.

DR. KLIBANSKY: I just wanted to add to the discussion that there seems to be a lot of uncertainty in the dynamics of reproduction in protogynous fish in general, and one thing that is a concern is that normally we have a sense of what happens to fertilization rates if sex ratios get skewed in a protogynous population, and there's also uncertainty in terms of -- We're looking at these things on a very broad spatial scale, but, with red porgy in particular, they are basically spawning where they live, and they're on these fairly small patch reefs, and so we could have different dynamics going on at different patch reefs, and so these kinds of things can be operating in a different way in a small spatial scale, when you think of the scale of the entire Southeast region, and I think that's valuable for future research.

DR. SEDBERRY: So that is interesting, and I wonder where we put something like that.

DR. ERRIGO: I was just going to say, are those part of the research recommendations at all, because they will get in here if they are.

DR. REICHERT: I think they would be part of the research recommendation analyses that we should do to elucidate that.

DR. SERCHUK: Can I make an editorial suggestion for the last part of the sentence? Rather than "which can have an effect on the ability of the stock", it might be better to say "which can impair or constrain the ability of the stock to rebuild", because that's the effect that we think will happen, either which can impair, of, if you don't like "impair", or "constrain".

DR. SEDBERRY: Okay.

DR. SERCHUK: Thank you. Any other factors?

DR. SCHUELLER: Do we really think that there's enough evidence to suggest that fishing pressure has been shown to affect age and size at maturity and growth rate? I mean, I feel like that's a pretty bold statement, like we've done a bunch of research and we have good samples and we did a big analysis, and, yes, age and size at maturity and growth rate are affected by fishing pressure, and I'm not sure we're there yet.

DR. ERRIGO: You're right. I instead should put something like "fishery-independent data has shown".

DR. SCHUELLER: Sure. That can work, but some factual statement where we can back that up would be preferable, because I think that could be misleading otherwise.

DR. SEDBERRY: Yes.

DR. REICHERT: I was going to suggest something similar, and I actually thought about wordsmithing that a little bit later, but you're absolutely right, Amy, and so I would say this is good, that it shown changes in size-at-age and maturity and transition, et cetera, and so we can wordsmith it later, but I agree with Amy.

DR. SEDBERRY: Okay. Any other --

DR. REICHERT: I can look into that a little bit more, but I think it may even be fishery-dependent and fishery-independent data, because we have some growth information on fishery-dependent data also, because we have age and lengths.

DR. SEDBERRY: Okay.

DR. ERRIGO: Would you rather me put independent and dependent?

DR. REICHERT: Yes, or "data".

DR. ERRIGO: That's what I have now, just fishery data.

DR. REICHERT: Yes, I think that's fine.

DR. SEDBERRY: Yes. Okay. Are we ready to describe the risks and consequences of the assessment uncertainties with regard to status and fishing level recommendations?

DR. COLLIER: There are two more questions, or one more.

DR. SEDBERRY: Okay. Sorry.

DR. LANEY: Nikolai noted, a short while ago, that this species spawns where it lives, and that prompts me to ask a clarifying question of Nikolai. Are these things territorial? I mean, are the males actually maintaining a territory on the patch reefs, and, if so, then perhaps -- This probably would be more appropriate for a research recommendation, but maybe there is -- Again, looking at the changes in age and size at maturity and growth rate and transition state and all those complexities Marcel pointed out, if you throw territorial behavior in there with it, if that is in fact the case, that makes it all the more complex to try and understand the factors that are affecting stock rebuilding, and so I was just wondering if it does show any -- Apparently, if it's spawning where it lives, and if it's territorial to boot, then that really puts us in a position where, the more we understand about the habitat that's on the bottom out there, the better job we might be able to do in terms of predicting future production.

DR. KLIBANSKY: I don't know how much we know that they have like a territory, per se, but, in a sense, the theory of why protogyny should really exist is that there is some advantage to being male, larger sizes at older ages, and advantages to being female at smaller sizes, that somehow the larger, older males are able to monopolize mating, and so, in a sense, if you think of the territory as being the female mating opportunities, then, yes, it's thought that, in a given area, that the larger males are able to monopolize mating, and so they're able to sort of have better territory, in terms of mating opportunity, than smaller males, but that can occur at the scale of whatever they are -- I don't want to say home range, but like a home range would be on whatever reef they are occupying, but I think those dynamics are really, for probably most protogynous fishes, are not that well understood. A lot of that research is on really small tropical grasses and things like that.

DR. SERCHUK: Mr. Chairman, I wanted to discuss this particular point, and, in this regard, I hope people read the article that I sent out last night on fishing for uncertainty, and my thing, in terms of describing the risks and consequences of the assessment uncertainties, is, although the assessment has uncertainties, and these are well documented, the status of the stock, with respect to its record low spawning stock size and record low recruitment, are obvious, or something else like that.

I mean, I think here's a case where we can all agree that this is a depleted stock, and I just want to say that, yes, the assessment has uncertainties, and all assessments have uncertainties, but it's clear that the stock size is at a record low, and recruitment is at a record low, and overfishing -- The resource is being overfished, it's in an overfished condition, and overfishing is going on. Something like that, Chairman. Is that helpful?

DR. SEDBERRY: Let's take Marcel's question, and then Anne's and then I will say what I was going to say.

DR. REICHERT: I think maybe -- I think the term that's used in the report is that those findings are robust, and I think that's the -- Relative to the earlier point about behavior, I think what we know, from the videos, is that the red porgy, in terms of territorial behavior, doesn't show similar things, such as gray triggerfish and hogfish, but we really don't know much about the dynamics, but I just wanted to say that, from the videos, we haven't seen that clear territorial behavior that we see in some other species from red porgy.

That's just as an aside, but that doesn't mean that it's not there, but it's not -- It hasn't been very obvious in what we've seen thus far, but I do agree that they live, eat, and -- They spawn and eat where they live, and another obvious evolutionary advantage of the protogyny is that you have a lot of younger females that contribute to egg production, and that, in some instances itself, has shown an evolutionary advantage, because you increase your output.

DR. SEDBERRY: Thanks, Marcel.

MS. LANGE: I just wanted to concur with Fred, and I'm not exactly sure which bullet he wanted to put that under, whether it's the one that we've been just working on or the next one that describes risk and consequences, but I think a very concise statement that this assessment is clear in its findings should be included here.

DR. SEDBERRY: Yes, and I think it's the next bullet.

DR. SERCHUK: Yes, the next one, Anne.

MS. LANGE: Okay. Thanks, Fred. Sorry.

DR. SEDBERRY: I don't want to mess up Mike's typing, but it's where my cursor is now. I guess Fred's point was, yes, there are uncertainties, but its overwhelming findings are that the stock is depleted, and so there is no uncertainty about that. Does the wording there, Fred, get at what you were trying to say?

DR. SERCHUK: It does, but I would just -- Because it's clear that the recruitment is at its lowest level, and stock size is at its lowest level, the status of those two components of the stock are robust to any of the uncertainties in the assessment.

DR. SEDBERRY: Okay. Then we have the question about are the methods of addressing uncertainty --

DR. SERCHUK: Could we also add the status of fishing mortality, or the estimate of fishing mortality, because now we think the stock is being overfished.

DR. ERRIGO: I don't think we need to worry about these so much. I already said what their effect is on, and we can flesh that out when we're writing the report. Like I said, that recruitment pattern used in the projections is an uncertainty, but I said that its effect is on projected fishing levels and rebuilding status, and so we can just flesh out exactly what that means later. The same here with the ability of the stock to rebuild is the consequence of this uncertainty, and so I think we already have those in there. We can just flesh them out when we write the report.

DR. SEDBERRY: Okay. Are we ready to take up the fishing level recommendations when we come back from lunch?

DR. SERCHUK: Can I make another comment, Chair?

DR. SEDBERRY: Yes. Go ahead, Fred.

DR. SERCHUK: I wonder whether, to be a little bit more explicit in the term of reference that we just did, we could add the status of the terminal recruitment (lowest on record), the terminal SSB (lowest on record), and the current F, and I don't know whether the current F is above -- Whether it's the highest on record or if it's FMSY.

DR. SEDBERRY: But describe it, whatever it is.

DR. SERCHUK: Exactly. I think that would be helpful to the layperson reader. Thank you.

DR. SEDBERRY: It is, and it's helpful to the council, and it's helpful to remember what we were thinking.

MS. LANGE: I concur completely.

DR. ERRIGO: I will look up the F time series. I don't think it's the highest on record, but I will look it up, just to make sure.

DR. REICHERT: No, you're right, Mike. It's not.

DR. ERRIGO: Okay.

DR. SEDBERRY: Okay. Are we ready to break? When we come back, we'll take up the fishing level recommendations for this and the last few items in the action items.

DR. ERRIGO: Would you guys say that the methods of addressing the uncertainty are consistent with your expectations and the available information? I mean, that's like a yes or no.

MS. LANGE: Yes.

DR. REICHERT: I would say yes.

DR. SERCHUK: Yes.

DR. ERRIGO: Okay. I will take care of the language over lunch. That's all I needed.

DR. SEDBERRY: Okay. All right. If there are no more immediate questions or comments, let's recess for an hour and meet back here at 1:30 to finish off this assessment and then take up the remaining agenda items. If there are no objections, and I don't see any, and I don't see any hands raised, we are recessed. Thanks, everybody. That was a good morning.

(Whereupon, a recess was taken.)

DR. SEDBERRY: Good afternoon, everybody. Welcome back to the final session of our spring SSC meeting, brought to you by webinar. We left off, before lunch, and we were finishing up the review of the SEDAR 60 assessment for red porgy, and we were -- We still have to look at the fishing level recommendations and then provide advice on the research and monitoring, and I believe that's where we left off.

DR. ERRIGO: Yes, that's correct, and I put in a statement here that we were talking about earlier, if you guys want to take a look at it, about running that additional projection using a recruitment level equal to the average recruitment from the last three years of the assessment.

DR. SEDBERRY: That looks good to me.

DR. ERRIGO: We may want to be a little more specific about which scenarios we would like re-run. Would we like all the scenarios re-run, or just like 75 percent of FMSY or F equals zero or I can put --

DR. SERCHUK: Can I make a comment, Chair? The only scenario I think is worth running is F equals zero, because, even under F equals zero under the projections we have, the stock can only be -- It can only meet the rebuilding criteria at the end of the ten-year rebuilding period, and so my

point in asking for a different recruitment run is to indicate that, if we use recent recruitment, the stock cannot be rebuilt in that period at F equals zero.

DR. SEDBERRY: Right.

DR. COLLIER: I do want to point out that, in the scenario that Nikolai had projected, it took twelve years to meet that 50 percent rebuilding timeline.

DR. SERCHUK: I still think we should do it at F equals zero, to see how much more extended it will be.

MS. LANGE: Should we say something in there of what the consequences of these runs will be? I mean, what's the action after this, if the run is made?

DR. ERRIGO: If you would like to suggest something, go right ahead, and I will put it in here.

MS. LANGE: I don't have a suggestion, but it just really seems like it's leaving it hanging in there, to do this run and then what? I mean, I agree with doing a run, but I am just wondering -- I think we should say something to what's going to happen with it.

DR. REICHERT: This may be for Shep, but, under the law, there is a rebuilding of ten years, and what happens if the modeling indicates that that's not possible, or is there a -- If there is then a different timeframe, or I'm not quite sure, and I know this may not be relevant for how we are phrasing this, but I was just curious.

MR. GRIMES: Would you like me to respond to that, Mr. Chairman?

DR. SEDBERRY: Yes, please.

MR. GRIMES: I was thinking about that a lot since this first came up, and I think it's complicated. I mean, I want to get all the facts together, but we're not talking about a species that is just now determined to be overfished and put under a rebuilding plan, right, and it's already under a rebuilding plan. If we were looking at it in the first instance, then we would say T_{min} is over ten years, and so we get to pick -- The council would get to pick a rebuilding time period based on the alternatives in the guidelines now, and they could be based on generation time, or I forget exactly what the other options are, but that's not what we're doing.

I think we're partway through a rebuilding plan, and determining that there is no way we can reach -- Much like we were in the case of red grouper recently, we cannot rebuild the stock within the current rebuilding period, and so we'll have to figure out what to do. Off the top of my head, I don't really know how I would advise on that, and I'm not sure that I have all the facts together at this point, and so I guess the answer is that it's complicated, and the council will have to figure it out.

DR. SEDBERRY: I think this thing that we're requesting here would be informative to the council, because they're going to have to -- It would make them look at, I guess, the fact that there is no way to rebuild this, but I don't know what this is -- I don't know -- I am kind of where Anne is.

DR. COLLIER: George, Roy has his hand up, and he might be able to talk about this.

DR. CRABTREE: One of the concerns that I think we're going to have to look at here is, if you don't use the stock-recruitment curve, and you use the recruitment from the last three years, then I suspect the stock never rebuilds, and I suspect the stock won't rebuild to the biomass at MSY unless recruitment goes back up to those higher levels, and so that's going to be a real complication of how to do the projections for rebuilding, and it seems to me that we've been in this situation before in the Gulf of Mexico, maybe with triggerfish, and I don't remember exactly what we did, but I suspect, at the low recruitment levels, that rebuilding is just not possible.

DR. SEDBERRY: Thanks, and I agree with you, and so I'm beginning to wonder, what do we do with this additional projection?

DR. CRABTREE: Well, I think, at some point, you have to consider if there's been a regime change, is recruitment ever likely to go back up, do we assume lower recruitment for some period of time and then start following the stock-recruitment curve? I don't know what the answers to any of those are, and I'm not sure there is a really good way to do it, but my suspicion is that, if you project out with the low recruitment levels, then you never rebuild, and so, if that's what you think is going to happen, you really have to redefine what MSY is, and so I think there's a lot of complexities here that will have to be considered. Thanks, George.

DR. SEDBERRY: Thank you, Roy.

DR. SERCHUK: The projection that we have now for F equals zero, first of all, it's not a ten-year projection, and that's correct. Since it cannot rebuild to SSB MSY in ten years, even given the stock-recruitments that are coming out, and that is not possible until 2032, and that was given in Slide 69. All the projections that we have actually tables for only go through -- We do have one table that goes through 2032, and that's on one of the latter slides.

I mean, clearly it cannot be rebuilt in ten years, and so maybe it's moot to talk about having a recruitment scenario that would use the most recent low recruitments, but the fact is all I'm saying is the recruitment scenario that we have in there may be optimistic, and, even then, you cannot rebuild until twelve years later. Thank you.

DR. SEDBERRY: Okay. Well, why don't we just say that then?

DR. SERCHUK: That would be fine.

DR. SEDBERRY: Okay, and so the SSC --

DR. ERRIGO: I just wanted to point out that, for red grouper, you also ran a low-recruitment scenario, and you used that to get your fishing level recommendations, in order to not have overly high landings, or ABC, when you were trying -- For this stock, if you thought that the near future would have low recruitment. Then the stock would be re-assessed, to see what state it was in, in the next few years, three to five years, let's say. If recruitment has come back up, then you can change your fishing level recommendations based on projections from that assessment, but that's what you did for red grouper, and I'm just saying there are other ways you can use projections like this that you have in the past, that you have used in the past.

DR. SEDBERRY: Thank you, Mike.

DR. REICHERT: I have two comments. The first one, or actually three, and the first one was, with my question to Shep, I did not mean to imply that I did not support the request for this additional projection, and I think it would be very illustrative to have that, and so I still support that request.

The other one was what Mike just brought up, and so that point has been made, that we've done that in the past, and red grouper is a good example, and the other one is unrelated, and I sent a paper to Mike, Wilson, and George, and I am embarrassed to say that I forgot that our lab did a diet study on red porgy that was published in a paper in 2016, and so, if that's relevant, Mike can send it to the rest of the committee, but I just wanted to make that comment, that I forgot about that study and that paper, because, earlier, I said we did not have any information.

DR. SEDBERRY: Okay. Thanks, Marcel. It sounds like we can do something with this request for the additional projections, that it would be informative to the council and to the SSC, and so we're requesting it. Then what do we do with it, to answer Anne's question of, okay, we requested this, and now what.

DR. SHAROV: What we do with this is this provides us with an idea of what would be the trajectory of the stock if the driving factors of the lower recruitment are consistent and we cannot uncover -- We don't know what they are, and we have not identified them, and we have some speculations on some elements, but, nonetheless, recruitment continues to be low, and you can demonstrate that, even in the absence of fishing, this stock is not going to recover to the target levels of biomass that were established based on the whole time series of -- The analysis, and, therefore, that leads us to the answer for the next question to comment on any difficulties -- On the adequate rebuilding process.

As has been just said recently, it is likely, if this continues in the future, in the next several years, we probably will have to re-evaluate the level of productivity of the stock, defining it as either being a standard or whatever it is, but it probably would be the next step that would have to be done, and so we will have to reevaluate the biomass reference points, because, if you have a stock that something has changed, whether it's productivity -- Of course, we're limited in our projections, and we are not -- We don't know, truly, what is going to happen in the absence of fishing, and we have only a simplistic model, and we're not accounting for other biological effects that might occur, but, if that continues, then, based on this projection, we should say that, if that's the case, then the stock productivity is likely to have changed, and we need a reevaluation of biological reference points.

DR. SEDBERRY: Okay. Thank you, Alexei. That makes sense to me.

DR. NESSLAGE: If I'm following the conversation and what Alexei is trying to say, I guess I'm -- Is it just the reference points that we would be reevaluating, or would we be rerunning the assessment with a shorter time period? Maybe that's a question for Nikolai or Erik or Kyle.

DR. SHAROV: Frankly, why would we want to shorten the time series for the model?

DR. NESSLAGE: Well, if there's been a regime change, you wouldn't want to include the information from the previously-high period in your -- It's going to affect your estimates of pretty much everything coming out of the model that you would compare with whatever your reference points are, and I guess these are MSY-based, and that's going to fit into your stock-recruitment curve, right, and so, if you're including the previously-high period of biomass, when it was highly productive, wouldn't that affect your biological reference points?

DR. SHAROV: Yes, that's right, and it's going to be really hard to determine where to draw the line.

DR. NESSLAGE: I agree completely, and I'm just looking at the landings dataset, when that big drop occurred in the biomass estimates, and you don't have much of a time period to do an assessment across, all of a sudden, and so I don't know how realistic it is that we would get a -- But I don't want to get people's hopes up that we might get a good number to work off of, of the same caliber assessment as this is, and do you know what I'm saying?

DR. WILLIAMS: The question, Genny, is, if we look at sort of other assessments that have dealt with regime shifts, one of the ones that comes right to the surface is Pacific halibut. The way they dealt with it is they still had the full time series model, but what they did was had two stock-recruit curves, and so what you do is just estimate a new stock-recruit curve at the point at which you think that the regime shift occurred.

Now, in the Pacific, they had a very clear sort of line they could draw, and they had lots of corroborating evidence of this regime shift, and so on and so forth, and so it was pretty clean-cut, and the effect you see is that there was just a reduction in recruitment at the same stock size. We are maybe seeing the beginnings of that with red porgy, but it's unlikely that we're going to be able to get a new stock-recruit curve out of basically what is really only about five to seven years of what looks like the beginnings of a regime shift, and I think the problem is we just don't have enough years under our belt, so to speak, to declare it a regime shift, and so that's one complicating factor.

DR. SEDBERRY: Thanks, Erik.

DR. SHERTZER: I was going to say similar comments to what Erik just said, but, as far as regime shifts go, it's really a short time period at the end of the assessment that we're seeing recruitment residuals that are less than zero, and Nikolai showed that in one of the plots that's in the report, but I think it's only the last six years or so where recruitment is lower than expected, than the spawner-recruit curve, and so it's less than a generation time, and so, similar to the case with red grouper, it seems a bit premature to consider a regime shift.

DR. SEDBERRY: Thanks, Kyle.

DR. LANEY: The term "regime shift" has been introduced by Dr. Crabtree, and followed up on by Erik and Kyle, and is the sort of -- Is it possible, and I say this based on some offline conversation with Mr. Bell here, but is it possible that we're getting the same level of recruitment, in terms of eggs and larval production, but we're just not seeing those fish reach any older ages, because of predation by a couple of other red species that have become quite abundant during this same time interval, those being red snapper and red lionfish, and is that a possibility? I know,

Marcel, there is some diet information on red lionfish out there, and I'm suspecting, and I know there's some on red snapper too, but is that the sort of ecosystem alteration that would constitute a regime shift, Erik and Kyle?

DR. WILLIAMS: I will jump in. I mean, it could. Again, and I think Kyle talked about this as well, but you really need more time to sort of verify that, because you want to be careful not to assume a regime shift when it hasn't occurred. At the same time, we clearly are seeing low recruitment. I mean, we're all kind of guessing at how long this low recruitment is going to continue to last into the future, and that's the game we're playing.

In some sense, we can almost -- I hate to kick this to the managers, but there is a whole risk scenario here to play out. Do you want to assume that it's going to be low recruitment and assume that the productivity can't get higher than that, or do you want to assume that the productivity could get higher and we're just in this period of low recruitment? Your guess is as good as mine, and regime shifts usually mean more of a permanent shift, and that's why I say that, really, you need time, more time, to sort of declare that, or have enough evidence to say for sure.

DR. SEDBERRY: Thanks, Erik.

DR. SHAROV: I think those were very valid comments by Erik and Kyle, and I agree with them that it's probably true to say right now that there is some sort of regime shift or change in productivity, but this is why I was saying here, actually, under the bullet point, that, if this trend in recruitment continues, we will need to evaluate the productivity of the stock and the benchmark reference points, and so it's if. It's conditional.

Certainly consider this. I mean, this particular projection that you're requesting, this is sort of the worst-case scenario, but, of course, there probably couldn't be worse than this worst-case scenario, but hopefully this is, at this point, the worst-case scenario, which essentially says that, no matter how much we can practically cut the fishing mortality, we're not going to rebuild in the current situation. That's all that it says.

Therefore, whatever the rebuilding requirements currently are, we need to be continuing it, and we're essentially in the same place or getting to where some other species are -- Like others where there is little to no removals and no recovery, and so we probably are in the right place, and it's just a matter of how do we -- Do we need to define that much what that specific projection means to us? I mean, I think I see some useful value in it, but it's not like a super-critical one, but an overall judgment.

DR. SEDBERRY: Thanks, Alexei. Erik, did you have your hand up?

DR. WILLIAMS: Just one more point to add to this whole discussion, and I don't know if this helps or confuses it even further, but I am big fan of taking advantage of every bit of information we have on the table, and so, if we do look at Nikolai's last slide, and we look at that MARMAP data that we have in hand, we have evidence that at least the two years after the end of the assessment are also potentially low recruitment, and so we do have some short-term evidence that recruitment has stayed low, at least in the two years after the end of this assessment.

DR. SEDBERRY: So does that give us an indication that the average recruitment from the last three assessment years would be a reasonable thing to assume would happen in the near future?

DR. WILLIAMS: Yes. I mean, I think that's what that is telling us, is that this low-recruitment pattern has at least continued into 2018 and 2019.

DR. SEDBERRY: So running this scenario with using the average recruitment from the last three years could be useful to tell us what to expect for the next couple of years.

DR. SHAROV: Well, that does mean that the situation will not change under any certain circumstances, right?

DR. SEDBERRY: Right.

DR. SHAROV: Yes, it's useful. However, we could say that -- I don't have to say anything more on that. Thank you.

DR. ERRIGO: I just wanted to remind you guys that you have to make an ABC recommendation using something.

DR. SEDBERRY: Right.

DR. ERRIGO: F equals zero probably won't do it, unless you want to recommend closing the fishery.

DR. BUCKEL: I would just get at the point that Mike just brought up, and so I think it's useful, for us and for the council, to see the kind of optimistic and pessimistic recruitment, and so continue to have the projection that Nikolai has with the average recruitment and the deviations off of what was in the model, and then the more pessimistic recruitment that Fred has proposed, but then the other thing that's going to affect the projections, as Nikolai showed, is the F, and so the F equals zero for the pessimistic recruitment and optimistic recruitment -- The F equals zero is the optimistic F, because, as we know, this is part of a multispecies fishery, and F equals zero is not going to be possible unless, as Mike just said, you shut down all of the hook-and-line, which won't happen probably, and so I wonder if we need a pessimistic F, to kind of add that other -- Nikolai has some of those higher Fs, but maybe another thing to consider is trying to estimate from the data what the number of discards would be if there was a closed fishery for red porgy, and what's the F likely to be, given what we know about the behavior, and my guess is a lot of the harvest and discards that you see now are -- That that would continue, and I don't -- At least in the recreational fishery, and I don't know how many folks target red porgy, if they're targeting vermilion and then they get the red porgy, or they might be targeting something else and get red porgy, and so I just wanted to add that other -- That would be the other pessimistic side, relative to the optimistic F equals zero.

DR. SEDBERRY: Thanks, Jeff.

DR. SERCHUK: There's a couple of things that we need to consider, Chairman. First of all, as opposed to the previous status, overfishing is now going on, and, as I recall the requirements, and Shep can correct me, but I think the council has a year in which to reduce the fishing mortality, so that overfishing is not occurring, and we have a run that does that, and that's the run of 75 percent

FMSY, when it means a current F of 0.3 to be below the FMSY of 0.18, and we have a run of 0.14 that begins in 2021, and that will take care of the overfishing part of it, if the council is responsible for doing that, going from overfishing going on to not overfishing going on, but it won't rebuild the stock.

That also presumes the results that we have there is that the recruitments that are going into that projection don't take account of the most recent -- If there's a continuation of the most recent poor recruitment year classes coming in, and so the council is faced with two issues now, overfishing, which they have to ensure that overfishing doesn't occur, and I think they have a year to do it, and then that projection, even if you get below FMSY, and you consider that an F rebuild, it will not rebuild in ten years under that, and, even under an F of zero, it doesn't rebuild within a ten-year rebuilding period, even with the recruitment that's coming off the stock-recruitment curve. I think there are points that are worth emphasizing, even if we don't have another projection done at using the poor recruitments in there, and maybe that's sufficient guidance for the council. Thank you.

DR. SEDBERRY: Thank you, Fred. I like that summary. I think it allows us to pass judgment on this assessment for the council to make the decisions based on the assessment that we have in hand.

DR. REICHERT: Fred, can you, and sorry if I missed that, but can you -- You mentioned the ending overfishing, and can you go through that real quick again? I was looking at the appropriate slides in Nikolai's presentation, and I am not sure that I understand what you were saying there, but that could be entirely on my end, and so would you mind going over that real quick again?

DR. SERCHUK: Sure. If we look at Slide 71 and 72, those are the projections, and we see that, under the FMSY, F is reduced to 0.18 in 2021, and, typically, if a stock is overfished, it's not sufficient just to get down to the FMSY, but you try to get a rebuilding F , and the slide after that, Number 73, has F going to 0.14, and it's below FMSY, and so you could, theoretically, say, okay, we've ended overfishing, and we have a rebuilding F , some F below FMSY that will allow some rebuilding to occur, but, of course, we know that, even under F equals zero, with the recruitment scenarios, the stock cannot be rebuilt within a ten-year period, and it takes a twelve-year period to do that, assuming that the recruitment stream that's coming off the S-R curve is likely and that the recent poor recruitment would not be coming in in the next few years, and that is recruitment of that nature will not be coming in, and that was the reason for going to a lower recruitment scenario, because you cannot rebuild this stock under F equals zero. It takes twelve years to do it, and only in the twelfth year do you have a 50 percent assurance that the stock is rebuilt. Before that, you don't have a 50 percent, and I think that's the criterion that is used when evaluating stock rebuilding.

DR. COLLIER: I have some MSA guidance on that, if you would like to hear it.

DR. SERCHUK: That's fine.

DR. SEDBERRY: Okay.

DR. COLLIER: In the MSA guidance, it indicates what adequate progress is towards a rebuilding goal, and so the Secretary shall review rebuilding plans at routine interval, that may not exceed

two years, to determine whether the plan has resulted in adequate progress towards ending overfishing and rebuilding affected stocks.

Such reviews could include the review of recent stock assessments, comparison of catches to the ACL, or other appropriate performance metrics. The Secretary may find that adequate progress is not being made if F rebuild or the ACL associated with F rebuild is exceeded and AMs are not correcting the operational issue that caused the overage, nor addressing biological consequences to the stock or stock complex resulting from the overage, when it's known.

A lack of adequate progress may be found when the rebuilding expectations of the stock or stock complex are significantly changed due to new and unexpected information about the status of the stock. If a determination is made under this provision, the Secretary will notify the appropriate council and recommend further conservation and management measures, and the council must develop and implement a new or revised rebuilding plan within two years. For secretarial-managed species, or fisheries, the Secretary will take immediate action necessary to achieve the adequate progress towards rebuilding and ending overfishing. Then there's a couple other pieces in there, if you would like to hear those as well, but I see hands raised.

DR. SEDBERRY: Let's hear from Shepherd first.

MR. GRIMES: Thank you, Mr. Chairman, and so, as I said before, I don't know -- I feel like I need to get all the facts for this together and think about it, but, just reading what's in the assessment report and then what was on the slides, it says this was declared overfished in 1992, and it went under a ten-year rebuilding plan, and then, in 2002, it was determined not to be rebuilt, and here we are in 2020, and biomass has been going the other way, and I don't see -- I mean, that's a very bad and complicated background, but it seems to me that it's pretty clear there is no adequate progress towards rebuilding that -- You know, we're thirty years into a ten-year rebuilding plan, but I think there's going to be a lot to figure out, in terms of what exactly we're going to have to do with this stock, in terms of management. I know that's not very helpful to your discussion, probably, but --

DR. SEDBERRY: No, that's helpful, Shep, and the things you're talking about are things that the council needs to address, and the SSC just needs to give its blessing, or whatever, to this assessment which is dire and has no clear way to improve its dire state, but the council will have to do something with it, and so I think our job is just to say the SEDAR 60 has been completed, and it's we consider it to be the best scientific information available, and it's got some uncertainties, but the uncertainties are far outweighed by the dire straits that the stock is in, and good luck with this, South Atlantic Council.

DR. ERRIGO: The SSC also needs to give an ABC.

DR. SEDBERRY: Right.

DR. REICHERT: That was the remark that I was going to make. I mean, that's where it gets complicated.

MR. GRIMES: Well, if I could, I was going to ask -- I mean, you guys have all dealt with this situation more than I have, but the council is going to have to pick a rebuilding period, or revise

the rebuilding plan, at a minimum, and I think you make whatever recommendations you make now, or at least as I envision it, and the council will start work on it, and then it will come back to you for your input on the rebuilding time period, correct, and then I think the SSC would have future input on the decisions, as far as the rebuilding plan, but I guess, if you come up with an ABC recommendation right away that's going to end overfishing, you have the possibility of an emergency rule or some sort of interim action to end the overfishing that could happen potentially without additional SSC input, but longer-term measures and rebuilding plan revisions would take longer to develop and could come back to the SSC for review.

DR. SEDBERRY: Right, Shep. I think you're right that the SSC would have a chance, some chance, in the future to review the steps toward rebuilding.

DR. SERCHUK: Just one more point. If you look at the projection tables, and look at the recruitment values that are being used for the first few years, and correct me if I've got this wrong, but the first column is just -- That's the number of recruits at age-one that is being -- I think that's the number of recruits that are going in. It says 1,019, 956, 951, 987, and so on and so forth, but, when I look at the assessment abundance at age-one, the recruitment that has been estimated in the most years, it's 620, 839, 613, 782, 345, and so some of the numbers that are -- All of the numbers that are going in for the first four years are significantly higher, maybe by 25 percent or 30 percent or 40 percent, than the most recent recruitment, and that has a major impact on the impacts of fishing. Thank you.

DR. SEDBERRY: Thank you, Fred.

DR. REICHERT: I am still trying to digest what Fred just said, but, first of all, thanks for that earlier clarification, Fred. That helped a lot. The question I had, and that doesn't address the remark that Fred just made, but just -- Can someone remind me that, in the case of a rebuilding, what is the charge of the SSC, and I am embarrassed that it's not right there on the top of my memory, but we set an ABC, and what is our responsibility in terms of a rebuilding plan? Could someone please refresh my memory?

DR. ERRIGO: I can. I can help. You still need to set an ABC. Oftentimes, you will go through your control rule and get a P rebuild, but that's a recommended P rebuild. Otherwise, the P rebuild would be rebuild at 50 percent, probability of 50 percent building, and then you will get an F rebuild at 50 percent, and you can recommend that from the assessment, and recommend that projection. However, you have said here that you cannot do that, and it won't rebuild, and there is no F rebuild, and so the same thing happened with red grouper, and you came up with something else for that, using the low-recruitment scenario, and so that's -- You have to find a way to come up with an ABC here.

DR. REICHERT: Thank you for jogging my memory, and exactly, and so, normally, that just rolls out of the ABC control rule P^* and P rebuild. Here, we are in a situation that -- Okay. Thank you. Sorry for my confusion here.

DR. SEDBERRY: That's okay, Marcel. I am just confused, and I think the point that Fred was trying to make is that the recruitment values that we're using are too high compared to recruitment values that are in other parts of the report, and I think that's what he was trying to say, and I'm going to get back to Fred in a minute, but, first, I have Wilson and then Amy.

DR. LANEY: Thank you, Mr. Chairman. I will go ahead and ask the question, and so I am unclear, based on the back and forth in our conversation here, whether we get useful information from running this additional projection with the average recruitment or not, and have we gotten to the point yet where we can decide that, yes, we need to do that, or, no, we don't need to do that, because I think I have heard arguments on both sides.

DR. SEDBERRY: I think that what we've concluded is that, yes, that that would be useful information to help inform the SSC and the council on a way forward with red porgy. What exactly the SSC would do with that right now, I don't know, and I'm not sure that we need it to make ABC recommendations, and so I'm a little confused about that myself.

DR. COLLIER: To that point, ABC for this species would equal harvest level as consistent with rebuilding, and that was established by the South Atlantic Council.

DR. SEDBERRY: I'm sorry, but could you say that again, Chip?

DR. COLLIER: ABC would be equal to a harvest level that is consistent with rebuilding.

DR. SEDBERRY: Okay.

DR. SCHUELLER: I guess I've been sitting here thinking what is the right way to go here, and I -- It seems like the ABC number that would be associated with this -- I would assume, or it sounds like, or maybe just this is what I think we should do, but recommend F equals zero, and so that's a close the fishery situation. The problem -- There is two problems with the projections that we've identified.

One is the recruitment issue that Fred has brought up, but then the other is the one that Jeff brought up, which is the discards, and so, even if F equals zero, there is some discard number, and I would assume that that number will be different depending on which recruitment we would use, and so it seems like the most plausible scenario would be to run a projection where the recruitment is set at the low values, and then we need to make some assumptions about what the discards would look like with that, and that seems to me like what the ABC would be, and it would maybe be an ABC based on discards only, which, several hours ago, Fred made the statement that I put in the notes of this should be a zero, no-take fishery with just incidental bycatch, which is basically what that results in. I guess that's where I'm sitting, and I don't know if that's like a reasonable recommendation for the SSC to make, but at least it would be a number.

Now, then we would have to get into the nitty-gritty of what kind of assumptions could be made to what the discards might look like if the F equals zero, because that's not really in the tables, unless I have missed it, and it looks like it's mostly assumed zero. Does that make sense?

DR. SEDBERRY: That does make sense, and I think what you're saying is that we need to run a projection that has the recruitment set at the average recruitment from the last three assessment years, setting fishing mortality -- With a fishing mortality of zero and a discard rate that would be -- That is whatever goes with a total moratorium on catch, and that would give us some projections about what might happen by 2032.

DR. SCHUELLER: Right. I mean, it could give you a probability of rebuild, and it could still be zero, but it would at least give the council information about if this fishery is no take, and the other fisheries are operating as they do now, and what would it look like for this.

DR. SEDBERRY: It seems like we've been down this road before during the red snapper moratorium and with warsaw grouper and speckled hind, and those are unassessed, and so, yes, I think we have done something like this before.

DR. REICHERT: I support that idea that Amy just brought up, but I just have a question. That does include F at zero, and that would basically put F at the bycatch mortality, equal to the bycatch mortality, correct? That's what we are proposing?

DR. SCHUELLER: Yes, and I suppose you could just set it up as that's the F , because that's the discard F rate.

DR. REICHERT: Exactly, but it's not F at zero.

DR. SCHUELLER: Right. It's F is zero in the directed fishery.

DR. REICHERT: Okay.

DR. SCHUELLER: So F is zero in the directed fishery, and then, in the discard fishery, what F rate -- It would be the current F rate in the discard fishery.

DR. SEDBERRY: Well, if F is zero, your discards are going to go up.

DR. SCHUELLER: Yes, and that's what I was thinking.

DR. REICHERT: Because George is right, and this is exactly the conversations that we had with red snapper. Okay. Thanks.

DR. SEDBERRY: Jeff has the answer.

DR. BUCKEL: No, I don't have the answer, but I think this is where we could get some feedback from the AP groups and some folks that are on the water, to let us know if there is any directed, and I'm sure on the commercial side there is, but there is directed areas that they probably go to get the red porgy, but, on the recreational side, it would be good to get information from others, to see if anybody directs on red porgy, and, if it's a very small percentage, then it would be the recreational landings now, and you take either 100 percent of that, or some percentage of that, and then those numbers of fish would then -- You would apply the discard mortality rate to it. That and whatever is currently in the assessment as discard.

Again, the folks that are on the water may have some better way of -- You know, what is landed now, what's brought to the dock. If you said we're not allowed to bring them in, would you still be -- Would they still be interacting with those fish, because they never were directed trips for red porgy, and they were directed on vermilion or whatever other species.

DR. SEDBERRY: Right.

DR. SERCHUK: The reason I offered the comment that Amy referred to is I have been in situations in ISIS, back when I was chairing the ISIS advisory committee, and the sort of advice we would give in situations parallel to this would be that there would be no directed fishery and that incidental catches be restricted to the lowest practicable level.

Now, I don't know -- That does give you what the level is, but it's the sort of advice that I think we're talking about, and it realizes that you're going to have some incidental catches, but you should try to ensure that they are minimized, but we don't know what that number is, and, really, the people out there that could tell us that are the people that are involved with the fishery, and I think it would be difficult for us to come up with a number for that, and, again, I don't know what the requirements are for us to come up with a specific number, but I'm thinking that that sort of advice is practicable in trying to recognize that we have the lowest spawning stock size on record, and we have the lowest recruitments on record, and that we can't rebuild the stock even under any of the scenarios that we have in less than twelve years.

DR. SEDBERRY: Thanks, Fred.

DR. ERRIGO: If I may, the task of the SSC is just to give a fishing level recommendation that avoids overfishing, and, normally, in a case for rebuilding, it rebuilds the stock in the rebuilding time, and, now, you have deemed that you cannot rebuild the stock in the rebuilding time, and, therefore, you can give an ABC that ends overfishing, and then you can recommend to the council that their ACL be set at the lowest practicable level and have them decide their amount of risk that they would like to take in setting that level, and that seems more like a management decision to me.

DR. SEDBERRY: Thanks, Mike. That seems reasonable to me, and we have a few hands that went up. Let me start with Shepherd.

MR. GRIMES: Thank you, Mr. Chairman. Just looking at the guidelines and scrambling to catch up with this stuff, but the guidelines talk about the ABC for overfished stocks, and they must be set so that the annual catch -- It says ABC for overfished stocks. For overfished stocks and stock complexes, a rebuilding ABC must be set to reflect the annual catch that is consistent with the schedule for fishing mortality rates, i.e., F rebuild and the rebuilding plan, but, without any kind of council feedback, or any kind of council plan set before you, it doesn't -- I don't see how you really do that in this context, and you don't know what an F rebuild is, because you don't have any kind of rebuilding timeline in front of you.

I was also going to note, and I'm scrambling to find it now, and I lost my page in here, but there is that provision in the guidelines where, if we reach the end of a rebuilding period, and we haven't rebuilt a fishery, or rebuilt the stock, that you continue to fish, or can continue to fish, at F 75 percent of FMSY until the stock gets rebuilt or the Secretary makes an inadequate progress determination.

Given the past history of this, I am thinking that we should already be at F 75 percent of FMSY, and maybe that -- The SSC has certainly commented, and you could make a lower recommendation, but it seems like the language in the guidelines about that inadequate progress determination from the Secretary means that we may have to adjust things based on feedback from

that determination and what NMFS does with this once it's accepted and has guidance from the SSC, or the council has some guidance from the SSC.

DR. SEDBERRY: Thanks, Shepherd.

MR. GRIMES: I'm not sure that's very helpful, and I apologize for that, but that's just what is in the guidelines.

DR. SEDBERRY: Dr. Crabtree, please enlighten us.

DR. CRABTREE: It seems to me that the first step in rebuilding any stock is to end the overfishing, and then we're going to have to figure out what the rebuilding time is and what is the F rebuild, and that's going to get into all these questions about recruitment and what do we do there, but it seems like, in the short term at least, you would want to figure out, using the low recruitments that you've seen in the last three years, here's the level of take that is consistent with ending overfishing.

Then you look at, okay, these are the discards that we think are going to occur in the fishery, and, if there is any additional take above and beyond that, then you can have some directed harvest, and it may be that the discards alone take up all the take there is, and then the fishery would have to be closed, and it may be then that we have to do something to bring the discards down, but it seems to me that step one is to figure out, given the low recruitments, what scenario is consistent with ending overfishing, and then is there anything that can be harvested beyond the discards, because I don't have much reason to think the discard patterns are going to change in the short term.

The trouble, and I've seen this in some other stocks, when we use the stock-recruitment curve, is if recruitments jump up, and you could end up setting the catch levels actually too high, and there's just no way of knowing if recruitments are going to go back up until you observe the fishery and see what really happens, and so it does seem to me that step one is an ABC, and the ABC ought to reflect how many fish can be landed that's consistent with ending overfishing, and then the council starts there, and then we'll go through and do a bunch of rebuilding timeframes and scenarios and more projections, and there will be some back-and-forth with the SSC, I'm sure, to get that taken care of.

DR. SEDBERRY: Thanks for those thoughts, Roy. They helped me to get organized, or at least be thinking where we might end up at the end of this meeting, but I wonder if we can now address the ABC control rule, as Roy was talking about, to set that to end overfishing. Let me take Fred and then Scott's questions.

DR. SERCHUK: Let me address the recruitment scenario. Roy has pointed out that, in the projections, you often have recruitment going up, and that's exactly what we have in our projections, and we have recruitment being at a 950,000, 987,000, and over a million, and the average of the last three years is only 500,000, and so we are expecting recruitment to at least be about double in our projections in the first few years and be higher than that, and so that's a very optimistic one, and that's one reason why I suggested all along that we use the average of the last three years, and I don't expect recruitment to jump by double in the very start of the projections. If it does, okay, but it would be highly irregular, given the pattern that we've seen in the last three years.

Then we could see what that means, and I think we need that projection with low recruitment before we can make any determination, quite frankly. We do have a run that shows what reducing the F from the current F of 0.30 to 0.18, which is the FMSY value, and to 0.14, which is 75 percent, and we have those runs, but they are all with the recruitment scenarios that are made off the stock-recruitment curve. Thank you.

DR. SEDBERRY: Thank you, Fred.

DR. CROSSON: I am just absorbing what Fred just said, and I need to think about that, but I guess my point was back to what Roy had said, and I don't disagree at all that we do need to end overfishing, and that needs to be the first priority, but I am wondering at what level, and, I mean, there's still this question of we just say you end overfishing, but you just keep the biomass at the current depressed level, or should we be setting an ABC that at least allows the biomass to start going up? I mean, it's a technical question, but I don't know what the answer is.

DR. ERRIGO: There are two ways you could do that. You can use the 75 percent FMSY, or you can go through your P^* analysis and use a P^* that's below 50 percent. Anything that's below the OFL should do it.

DR. SEDBERRY: We can do either of those things, and I'm not sure that that addresses Fred's concern about recruitment.

DR. REICHERT: Just a quick question. Scenario 4, F of zero, does that include bycatch, or is it just landings?

DR. ERRIGO: It has landings and discards.

DR. COLLIER: It is landings and discards at zero.

DR. REICHERT: Thanks. I probably could have figured that out myself, but I was just wondering.

DR. COLLIER: One of the complicating factors for this is red porgy were -- As Shep had said, they were in a rebuilding plan that was set back in 1999 with I believe it was an eighteen-year rebuilding plan, and so that was going through 2017, and you can see that we did not get out of -- We didn't reach the goals of that rebuilding plan, and so, as Mike said, there is different ways that you can go about this. You can set it at FMSY, or 75 percent of FMSY, or go through your control rule in order to set the fishing level recommendations, or document why you want to go in another direction.

DR. SEDBERRY: Do you think going through our ABC control rule might enlighten us, and I don't know, and maybe go through the ABC control rule with the idea of ending overfishing, and, as we develop that, further develop what we need to do to rebuild. I mean, would we be --

DR. COLLIER: The rebuilding plan is something that the council will have to do through their next FMP for this species, and that addresses the species. Obviously, because 2017 has already expired, they are going to have to address a new rebuilding plan for this species.

DR. SEDBERRY: Okay. Amy, I see you have your hand raised. Go ahead.

DR. SCHUELLER: It sounds like we'll have to come back and discuss this as a rebuilding plan gets put together, and I'm wondering if -- Roy mentioned the path forward first, and Fred mentioned this as well, is just reduce the overfishing, or eliminate it, and not reduce, but eliminate it, and so I was kind of throwing around in my head that I could see us doing a projection at 75 percent FMSY with the recruitment changed, as we've discussed, but then also implementing P^* on that, meaning, if we implement P^* , we give ourselves even more of a buffer, to ensure that that overfishing is not occurring, and it's just -- I guess it's a risk-based thing on our part, given that we're uncertain about what the recruitments will look like, and we're uncertain about what the discards will actually do with a reduction in F , and so I don't know, and so I guess I don't see this as an either ABC control rule or F at 75 percent FMSY, and it could be any combination of things, not to make this discussion even harder than it already is.

DR. ERRIGO: Just to clarify, so you're saying use 75 percent FMSY as the OFL, and then come down from that using the P^* .

DR. SCHUELLER: Yes, and it would -- It's an even more conservative approach, but, if the main goal is to try to guarantee that we're not overfishing, it might be worth doing that. If I'm off my rocker, everybody can let me know, but I feel a little -- It just seems like we need to be risk-averse here, and, anyway, but I will stop.

DR. SEDBERRY: Amy, this sounds good to me, and we do need to be risk-averse, because of what has been done for the last thirty years hasn't worked.

DR. REICHERT: Amy, I am trying to wrap my head around this, and so, yes, we need to stop overfishing. Looking at this -- I am still hung up a little bit on the discards and the landed and where that would come in, because, if this is an F , would we then need to look at how much of that is represented by the discards, to figure out how much fish can actually be landed, if any?

DR. ERRIGO: It's already broken out into landings and discards separately in the projections.

DR. COLLIER: George, John Carmichael has his hand raised to, I believe, address some of these recent comments.

MR. CARMICHAEL: Hello, everybody. This is quite a quandary, and I was struck by how similar this seems, to me, to red grouper recently. We had the assessment, and we're struggling to reach the rebuilding time, and I think what we ended up doing there was initially the ABC -- The SSC gave an ABC based on the F rebuild, and, in that case, I think it might have also been 75 percent of FMSY, and then NMFS just said that we weren't making adequate progress, and the council came up with the new rebuilding, and they came back to the SSC, with you guys, as has kind of been mentioned here.

I think one way you could do this is just to give the ABC based on the 75 percent of FMSY, and I think, based on your discussions, perhaps, with the new scenario of the lower recruitment, that will probably -- Looking at the current table of 75 percent of FMSY, you may end up in largely an unable to have a directed fishery type landings level to begin with, and I think the SSC could also give the council some advice and considering not having a directed fishery during the time the

council is working on this rebuilding plan, if necessary, or just look at that landings level and think about what it means with regard to a directed fishery, because I think is true that you are probably going to be back at the drawing board in coming up with a revised rebuilding plan for red porgy at a future meeting, and so, if you could just get yourselves to that point now, it might be a better use of time. Thanks.

DR. SEDBERRY: Thank you, John, and so I like that idea of getting to that -- If we could get to that point now. I am just not clear, in my head, what we need to do to get to that point. If a committee member have an idea of some steps we can take now to move forward, that would be great.

DR. ERRIGO: All you need to do is choose projections for OFL and ABC, and Amy put this proposal up here, and there are several other ways that you can do it, and you can just use an F equals FMSY scenario as OFL, and 75 percent FMSY is the ABC, and you can use the P* approach to give you ABC and OFL with the low-recruitment scenario, and you just need to make sure that you're not overfishing, which all of these things do, and that's it.

DR. SCHUELLER: I mean, to follow on what John just said, I mean, it sounds like his recommendation for a path forward here is to do the F at 75 percent FMSY with the recruitment based on the most recent years, and then that can be the value, and, I mean, I'm fine with that, and it seems like that might be a path forward that is straightforward. I mean, we can caveat it and say that the council could consider reducing that even further and closing the fishery. I mean, there isn't really a -- Every scenario here is a lose, really, and so it's sort of just picking which loser we love the best, which sounds really silly, but, yes, we can just -- To me, I don't have my druthers hard-core one way or the other.

DR. SEDBERRY: Pick which loser we like the best. That sounds like election day.

DR. SCHUELLER: As long as we run the projection with the low recruitment, I think will be okay.

DR. SEDBERRY: Right, and I like that. It takes care of the concerns that Fred brought up initially and that we've been talking about that has been underlying all of these discussions, and so I like that idea. John, I see you have your hand back up. Go ahead.

MR. CARMICHAEL: I was just going to follow-up with what Amy said. In the red grouper, you weren't quite ready, as an SSC, to say we believe the low-recruitment scenario is the path forward, and you weren't quite ready to give up on the possibility of the longer-term average being a good indicator of the future, and it sounds like, on red porgy, you have sort of -- You have a little bit different feeling about what the recruitment signal is telling you, particularly as you look ahead to the next few years, and I know what Fred had mentioned and what you talked about relative to MARMAP, and I think that's a pretty important point, because there may be some comparison to this and red grouper down the road and about why the SSC took a different viewpoint of short-term future recruitment, and so, as long as you address that, I think you're fine.

DR. SEDBERRY: Okay. I don't see any more hands raised right now, and so, Mike, can you step us through this, starting with provide fishing level recommendations applying the ABC control rule? Can we step through this with the idea of the -- I don't know where it goes though.

DR. ERRIGO: I have language here already for the scenarios under the low-recruitment scenario under F equals zero, F equals FMSY, and F equals 75 percent of FMSY and what each of those runs will be used for right here, and we're not going to -- If we don't use P^* , I will just get rid of all of this, and that will just go away.

DR. SEDBERRY: Okay. Go ahead, Yan.

DR. LI: Thank you, George. I am taking notes here, and, under those scenarios that we are going to run, how about the discards that Amy brought up earlier? The discards we treat it -- At what discard level assumption are we going to assume in these scenarios?

DR. ERRIGO: Discards are calculated in the projections, and Erik or Kyle can help out with this, and I don't know if they are a proportion of the landings, based on what they were at the end of the assessment, or if they are based on the F s, but discards are included in the projections.

DR. WILLIAMS: The discards are part of the F that we would use, and so the 75 percent FMSY would include a reduction in discard assumption as well, just proportional, and I will say that what you have on the board is very straightforward, because it's basically just rerunning all the projections that Nikolai presented, but with a lower recruitment scenario, because I think he had every one of these in there, the F equals zero, F equals FMSY, and F equals 75 percent FMSY.

DR. SEDBERRY: Okay.

DR. SERCHUK: Just one comment, Chairman, in terms of the number of years the projections should go forward with. Other than the F equals zero scenario, Nikolai only ran projections for ten years, and none of the ten-year projections, either at -- None of the projections, other than the F equals zero, did the ten-year projections at F 75 percent or F current or FMSY have the last year in the projection approaching 50 percent probability of being rebuilt, and so I don't know whether we would just want to have the F equals zero again projection go as far as necessary, to see how long a number of years it would take to have, in the last year, at least a 50 percent probability of rebuilding and to have the other ones just be ten-year projections.

Do you understand what I'm saying? We have a twelve-year projection now in the assessment for the F equals zero scenario, because, at the end of twelve years, you finally had a 50 percent probability of being rebuilt. In the others, there was no probability at all in the ten years that the stock would be rebuilt.

DR. ERRIGO: I understand what you're saying, Fred, and I actually was going to ask how many years you wanted the projections for OFL and ABC to be run. One thing you should note is that, when you change the recruitment for the projections, then, when you run F equals zero out, the benchmarks no longer hold, and so, I mean, you can run it out to the same benchmarks that were in the assessment, but those don't really fit with that recruitment value. You would have needed to have re-estimated the benchmarks, in order for them to be apples-to-apples, but, I mean, I think they could still run it out that far and see what happens. Erik or Kyle, if I said something wrong, jump in, but I'm pretty sure, if you changed recruitment in the projections, then you would need to re-estimate the benchmarks, in order for that to be apples-to-apples when you project out.

DR. SEDBERRY: We have a few hands going up. Dustin, Alexei, and then Fred.

MR. ADDIS: Thank you. When we run an F equals zero scenario, are we assuming there is no discard mortality or discards, because you would assume --

DR. ERRIGO: Yes, that is the assumption.

MR. ADDIS: Okay, because, in a fishery like this, you would expect discards to happen regardless, if the fishery is closed or not.

DR. ERRIGO: Yes, and this is a theoretical projection, basically. This would be like not just red porgy, but if you closed the entire hook-and-line fishery.

MR. ADDIS: Okay. Thanks.

DR. SHAROV: I guess this is a lot of good discussion, but we need to essentially make a decision, and I think that the proposal of calculating the ABC at F equals 75 percent of FMSY, and then recommending to the council to initiate a new rebuilding schedule, and I think that was the best proposal, and the most practical one, and we can come back later and continue the discussions of how to deal with it, but, at this point, this seems to be the most reasonable proposal. Thank you.

DR. SEDBERRY: Thanks, Alexei, and I agree with you, and the council will get back to us with the rebuilding scenarios for us to review, and so next -- What do we do next?

DR. REICHERT: Do we need to comment on any difficulties encountered in applying the ABC control rule? I would say that that is basically because, after, what was it, thirty years of rebuilding, we haven't rebuilt, and that created some issues in applying the ABC control rule, or maybe we could have applied the ABC control rule, but it is a moot point, given the fact that we are not rebuilding.

DR. SEDBERRY: Yes, and so what's the best way to word that?

DR. ERRIGO: I just put some basic stuff down here, that rebuilding progress has been stifled by a steady decline in recruitment since the early 1990s, and then, if someone has -- I can't remember if this was talked about, but, if that trend in recruitment continues, we'll need to reevaluate the productivity of the stock and the benchmark reference points, and I put that under is adequate rebuilding progress being made. It kind of goes under both. It kind of addresses both of these bullet points.

DR. REICHERT: Okay. I would just perhaps say "see below", so at least there is an indication that we did discuss it and addressed it, but we combined the two.

DR. SEDBERRY: Okay. So right where Mike's cursor is there, but "see below". Okay.

DR. REICHERT: Then at least we indicate in the report that we did discuss that.

DR. SEDBERRY: Right.

DR. ERRIGO: As for how long to run these projections out, I was thinking until the next assessment, whenever you guys would recommend that, and that would be like five years or --

DR. REICHERT: Well, didn't Fred address the length of the projections to --

DR. ERRIGO: He said F equals zero out until rebuilding occurred, and I know Nikolai ran them out for like ten or twelve years, but, if you want to use that long of a time period for the ABC, that's fine. Typically, your projections for ABC only go out as long as you think they are useful.

DR. SEDBERRY: Right, and I thought that Nikolai ran most of his projections out to 2026, with one of them to 2032.

DR. ERRIGO: Yes.

DR. REICHERT: Fred, was your request for a longer -- Just to tell us how long it would take for 50 percent probability for just that one projection, or did you want that for all projections?

DR. SERCHUK: I only wanted it for the F equals zero projection, because I wanted to find out how long it would take us, if F equaled zero, to get a 50 percent probability, and that's, I think, what Nikolai did with that.

DR. ERRIGO: Yes.

DR. SERCHUK: Thank you. The projections also assume that the reduction in F to F 75 percent, if it was implemented, would be implemented in 2021, and that would be the time when there would be a reduction from the status quo.

DR. REICHERT: Okay. Thanks, Fred. I appreciate it.

DR. ERRIGO: Okay. I'm good.

DR. COLLIER: Marcel, do you have your hand raised?

DR. REICHERT: Yes, because Mike also brought up the next assessment, and he said a five-year timeframe, and I would say that that's the longest we should wait, given the uncertainties and given the potential implications for fisheries in the region, and so I personally would like to see a somewhat shorter time between this and the next assessment, because, if we say the next assessment in three years, given the two years that have already gone by since the terminal year, plus the fact that it takes a year to get even an update done, I would say we probably should -- I would recommend an assessment to happen in the next couple of years, an update, but I would like to hear from others.

DR. SHAROV: I think I understand where Marcel is coming from, but wouldn't it be more useful to actually try to see some effects of the reduced fishing mortality, if the council decides to act, and we'll have at least say two or three years of truly reduced removals and lower Fs, and possibly stock response.

DR. REICHERT: Yes, that's a good point. I was more thinking that would give us an extra couple of years to see if the recruitment actually has changed, which would have some implications for projections, but, yes, you are right, and, if management actions are going into place, you want to have at least a couple of years for them to percolate through the population, and so --

DR. SEDBERRY: So is five too many then? Let's put in five and see how that plugs.

DR. SHAROV: That's just about right, practically.

DR. SEDBERRY: Okay. Is there anything going on to monitor recruitment? There is no pre-recruit index of abundance or juvenile survey, and we know so little about the early life history of this thing that I don't think there is any monitoring going on that could give us a juvenile index, and it would be great to have some ideas along those lines.

DR. REICHERT: I think you're right, George, and that may be a research recommendation, that it would be useful to develop a juvenile recruitment index. I'm not quite sure how to do that, but the --

DR. COLLIER: That doesn't necessarily have to be a juvenile recruitment monitoring. It could be just how can they monitor in between assessments on the condition of the stock.

DR. REICHERT: To that point, I think, of course, we will continue to monitor the stock using the fishery-independent survey, and red porgy, fortunately, is a species that we collect in sufficient numbers, and so we will continue to do that.

DR. SEDBERRY: This is a good place to put in a plug for continued funding of SERFS.

DR. NESSLAGE: I concur with all of those statements, and I would even make the comment stronger, that we should probably look at these annually, the council should look at them annually.

DR. SEDBERRY: At the indices?

DR. NESSLAGE: Yes, and the age comps.

DR. REICHERT: As an aside, we do provide that annual update to the council in June, but that's only for the chevron trap, because the other -- Like the video data and the age data, that generally takes a little longer to analyze and to provide, but at least it gives some indication, and, of course, like we did this year, we can potentially see if we can -- If this is a high priority, we can potentially see if we can prioritize the ageing and the histology and the reproductive parameters of red porgy, possibly, probably, at the cost of some other species, but it looks like this is a pretty important species.

DR. ERRIGO: Is there a trigger for that? Like, if you see the age comps showing higher recruitment, or the trap index going up or down, and you would reevaluate the projections or something, or reevaluate the fishing level recommendations, or -- I am thinking if the recruitment scenario changes. Is there a metric that says we have changed now and we're no longer in the low recruitment?

DR. SEDBERRY: I don't know if there's a trigger. I am trying to think of other examples, and I know the SERFS index certainly has kind of helped to drive re-looking at red snapper.

DR. NESSLAGE: I guess I was thinking more along the lines of it would be good to keep an eye on it for the next four or five years, because, if, in four or five years, even with management action, if there is no rebound, I don't know how useful it is to do the operational assessment in five years, and I think you're looking at a long time period here, and frequently assessing it might not help, but looking at the age comps, for any indication that there might be a change in recruitment, and then reassess is more what I was thinking, and it would be a trigger for assessment, but maybe I'm not thinking through this correctly.

DR. SEDBERRY: That seems reasonable, and, like Marcel said, the SERFS catch per unit effort index can come pretty quickly, and the age comps might take a little longer, and the video index would take a little longer, but certainly, if the SERFS catch per unit effort index shows sharp increases or decreases, that should be monitored, to see if we need to start looking more intensely or putting more money into getting the ages done or getting the videos done, and so I think monitoring those things, the SERFS index, the age comps from SERFS, and the video index from SERFS, would be useful.

MS. LANGE: I just think that there aren't actually triggers, and I don't think we have a level that we're looking for, but, if the age comps start showing an increase in recruits, then that should -- I think it should, as was just stated, potentially trigger another assessment, or let us know that it may be appropriate to reschedule an assessment. There is no particular level, other than an increase in recruits, I think.

DR. SEDBERRY: Thanks, Anne.

DR. BUCKEL: To get something quickly and not have to rely on the ages, Marcel, is there a way to -- For those younger fish, the data that you provided on those younger fish recently, is there some way, just by size, that you can create an index of say age-zeroes and ones, or ones and twos, based on the size-at-age information you have? That might save some time and be able to get -- Instead of the index of the overall trap catch, have the trap catch of the juveniles.

DR. REICHERT: Yes, and that was actually something that I was going to bring up, and we can certainly look at that. I think there's a huge variability in size-at-age, even in the smaller fish, but we can certainly take a look at that. If we can prioritize the red porgy, we can certainly do that, and I think it -- I am taking my SSC hat off and putting on my reef fish survey hat on, and it certainly would help if the SSC would tell us whether they would be interested in, for instance, at their spring meeting, if they would like to see those age comps and the index, because then I can take that to NMFS and to our colleagues and see if we can prioritize the processing of those samples, but, yes, we can also look at the length, and the length data are generally, obviously, a lot easier to generate than the ages.

DR. SEDBERRY: Thanks, Marcel. Did you have something else?

DR. REICHERT: If the SSC thought that that would be helpful.

DR. SEDBERRY: I think it would be, and I'm sure the new Chair will think the same thing.

DR. REICHERT: If I may, and not to put you on the spot, Erik, but is there other information that we collect that would be helpful for this, or for you guys?

DR. WILLIAMS: Nothing is really coming to mind right now. I am still thinking.

DR. REICHERT: That's okay, and we can talk about it offline. I mean, anything we can do to help provide as much information as we can, we will certainly work with you and Todd and Nate's group.

DR. GRIMES: I've been out of the game for a long time, but is there any such thing as ichthyoplankton surveys anymore, or historical records, that might be useful to look at?

DR. REICHERT: Sorry to butt in, but I don't believe so. I think Todd's lab is running an ichthyoplankton bridge net survey, but I think that's the only ichthyoplankton data that's being collected in this region, and I'm not sure what the status is or whether that's still going, and so SEAMAP actually has put that in their five-year plan to look at the feasibility of an ichthyoplankton survey, and the problem, and someone else brought it up earlier, but the problem is the link between ichthyoplankton and recruitment to the fishery. If there is no correlation between the two, then I'm not sure how much that would get you, but that's a different question, but there is no ichthyoplankton survey in the region.

DR. SEDBERRY: There was a historic ichthyoplankton survey in the 1970s, and red porgy were very rare in those samples, because red porgy spawn in winter, and so the eggs and early larvae were not available to the survey, and then, by the time the survey was done, those things grow really quickly, and they can avoid the net, and so the ichthyoplankton survey was not very useful for red porgy.

DR. LANEY: The Beaufort bridge net survey is still ongoing, as far as I know, and there's also a long, a very long, time series of ichthyoplankton at North Inlet, but I will defer to those of you who know the red porgy life cycle a whole lot better than me, and are those -- I think, as Nikolai pointed out, they spawn where they live, and so I'm not sure that those inshore ichthyoplankton surveys would even capture red porgy, and I would defer to the Beaufort Lab folks on that point.

DR. REICHERT: I don't believe they are very abundant, if abundant at all, in those surveys.

DR. SEDBERRY: They are certainly not in any of the old estuarine ingress samples, and, really, even -- They spawn where they live, but the older, more mature ones are deeper, out at the shelf edge, and I think they spawn closer to the shelf edge than say black sea bass.

DR. ERRIGO: So I'm good with what we've got here. I think, for the research recommendations, we can fill that out offline and get that in there, and so, if you want, we can move on past red porgy, to where we've got everything else.

DR. SEDBERRY: That's fine with me. I will ask the group. If anybody has anything remaining that they would like to discuss about red porgy, raise your hand.

DR. COLLIER: Just to be clear, for the projection that the SSC is requesting, they want a projection of F equals 75 percent MSY with the low recruitment to set the ABC?

DR. ERRIGO: Yes, that's what I understand.

DR. SEDBERRY: That is correct, and, if we've got that wrong, SSC, now is the time to speak up. I don't see any hands raised. Moving on, our next agenda item is the Council Workplan Update, and these were Attachments 15 and 16 in the briefing book, and, you know, normally during our meeting, we'll have council members talk about what's going on in their -- Not council members, but council staff talk about what's going on in their purview. I don't know what you had in mind for this, Mike. We have the details in the briefing book.

COUNCIL WORKPLAN UPDATE

DR. ERRIGO: Right. I think what I'm going to do is just ask if anyone has any questions with anything that's going on, anything in any of the materials that was in the briefing book, and, if not, or after we've answered those, then I think just to move on.

DR. SERCHUK: Chair, can I make a comment?

DR. SEDBERRY: Yes, please. Go ahead.

DR. SERCHUK: Were you supposed to take public comment after we were done with the red porgy assessment?

DR. SEDBERRY: I thought we took public comment after the presentation and before we started discussing.

DR. SERCHUK: Okay. If so, I apologize.

DR. SEDBERRY: Yes, I believe we did that, and we're going to public comment again at the end of the day here, and we take it at the beginning of the meeting and at the end of the meeting and then after every presentation for each agenda item, and I think we did do that for the red porgy, after Nikolai's presentation.

DR. ERRIGO: Yes, we did. There wasn't any comments, but you did ask.

DR. SEDBERRY: Okay.

DR. REICHERT: A quick question. Since we discussed the ABC control rule, and there's a question-mark, and I assume that refers to the council staff member, and is there -- Please remind me. Is there a timeline for that, or is that kind of on hold, because I think that, given the assessments that we are looking at, that is becoming increasingly important for us.

DR. ERRIGO: I might have Chip or John address that. I know that it's been put in a lower-priority category, and so it's on a longer timeframe than it was before, and I don't know if, Chip, you know what the actual timeframe is for it, or John.

DR. COLLIER: We have Brian online, and he might be able to answer that, and he deals with the council priorities. He might not be at his desk right now, but, right now, for the ABC control rule, it is not a high priority, like Mike had mentioned, and John was in charge of that amendment, but, since then, he has taken the Executive Director role, and so we're still working on who is going to be doing that amendment. Right now, it's looking like it might be me, but it does have a lot of work that needs to be done to it in order for it to become ready, and so it's going to be coming to you guys, and it's likely to take over a year to develop that one.

DR. REICHERT: Okay. Thank you.

DR. SEDBERRY: Okay, and so do we have any additional comments or questions about the council workplan? I know we haven't presented it in detail, but it was -- Again, it was in the briefing book, and there's a summary of it here in the overview document.

DR. SCHUELLER: I was going to ask what Marcel asked, if he hadn't, but what I was wondering is can the SSC make a statement saying that that should be a high priority, at least, for their consideration into the future? I mean, it just seems like we worked on that a bit, and then it just sort of has been set off to the side of the table, and I would hate to see it languish.

DR. SEDBERRY: There is certainly precedent for the SSC making statements like that, and, if the members agree, we can include an encouraging word like that as a comment on the workplan, and we have no specific action items for the workplan, but we can certainly comment on it. We would like to make this the -- What amendment is it? The Comprehensive ABC Control Rule Amendment. We need to be careful about our wording here, and what do we want to say? Any suggestions?

DR. REICHERT: I asked the question because of all of this, the ABC control rule, is coming back to us every single time we review an assessment and every time we need to update the unassessed stocks, and so that's why, as an SSC, I think that is an amendment that is very relevant for us, and that's why I was asking that, and so I agree with Amy that it would be good for us to -- If the committee agrees, to express to the council that we feel that that should have a level of priority, because of the importance for us as a committee.

DR. SEDBERRY: Okay. I agree that we should include a statement that the SSC would like to encourage that this be a higher priority, and are there any objections? Do any members have any objection to including something like that? Of course, the draft wording will be available for your review before we sign off on this. Churchill, is it to this?

DR. GRIMES: I was just going to ask you who you got to replace Rob Ahrens on the committee, the Ecopath thing.

DR. ERRIGO: We didn't. He actually finished his role in that committee. This committee was active right up until this meeting, and we, unfortunately, had to cut out this agenda item, for time.

DR. SEDBERRY: Right, and so that report was going to be presented to us at this meeting, in Charleston, and that's one of the reasons it was a four-day meeting, but, when we reduced it to a three-day webinar, we eliminated some items, so that everything would almost fit.

DR. REICHERT: George, can I make a quick comment?

DR. SEDBERRY: Yes.

DR. REICHERT: I believe we added Yan to that committee.

DR. SEDBERRY: We did.

DR. REICHERT: Just because we became aware that Rob may have to resign from the committee, possibly, before we finished our work, and so Yan participated and contributed to that committee.

DR. LI: Thank you, Marcel. I was going to ask if I was still there. Thank you for making that clear.

DR. SEDBERRY: Yes, you are still there.

DR. SHAROV: I happen to be on that committee too, and I thought there was supposed to be a third conference call on Ecopath, and that was not scheduled yet, and it was supposed to be scheduled at the end of April, if I am correct, and so I'm not totally sure that we are done, but, anyway, I just wanted to make sure that we are either done, or are we still supposed to finish it up?

DR. ERRIGO: Well, if you have another meeting scheduled, then that's fine, but I just didn't know the schedule of it.

DR. SEDBERRY: Neither did I, and I may have spoken out of turn there when I said it was done, but we were -- The SSC was supposed to get a presentation on this at our meeting this week, but we postponed that until October, and so that whole schedule may have shifted, and I'm not sure exactly what it is.

DR. SHAROV: Okay. I was just making sure that I haven't missed too many meetings, and so thanks, and I hope, by October, we will certainly have the results.

DR. SEDBERRY: Okay. I have Wilson, Marcel, Yen.

DR. ERRIGO: You have Chip.

DR. SEDBERRY: I'm sorry. How quickly I forget. Go ahead, Chip.

DR. COLLIER: In the comment there about the ABC control rule, if you guys, when you're developing your report, just list what issues you're having with it, and, that way, it can be clearly identified.

DR. SEDBERRY: Okay. What I'm trying to think of right now is examples of how it's come up during this meeting that we have to kind of defer to the revised ABC control rule, and I can't think of anything off the top of my head, but I will have to give that some thought, but thanks for that suggestion. Yes, it's always good to be specific.

DR. NESSLAGE: To that point, didn't we want to talk a little bit about ORCS and reexamining ORCS? Was that part of the ABC control rule? I think that's the big one, or one of the big ones.

DR. ERRIGO: Yes, I think that would be part of addressing the ABC control rule.

DR. SEDBERRY: Yes, and so it came up under our dolphin wahoo and a few other places, where we kind of deferred to revisions of the ABC control rule.

DR. REICHERT: The other point is that we are still making adjustments on risk that the council has indicated is their purview, and so that's all still part of the current ABC control rule, and so I would say that may be another reason why this would probably be good to have addressed sooner rather than later.

DR. SEDBERRY: Great. Thanks.

MR. GRIMES: I was just going to add, just based on my observation, that it seems like, every time you guys walk through the control rule, you vary from it somewhat, and it seems like that, in of and itself, is a good reason for the council to maybe move forward with at least reconsidering it, if not moving forward quickly with changes, and an example of this is I think, with both of the assessments you've been through at this meeting, you have not used the MRAG classification of stock risk, where your control rule indicates that you will.

DR. SEDBERRY: Good point. Thank you, Shepherd.

DR. LI: I just wanted to look at the Ecopath model review group schedule, and we did schedule the third webinar, but it was cancelled, and the webinar was to review the draft report, and so I don't think we're finished.

DR. SEDBERRY: Okay, and so the April meeting has been cancelled, and the report to the SSC is not going to be until October, and so I think there's -- With the change to the calendar, things may have just been postponed, and, again, I'm not on that workgroup, and so I'm not sure what the schedule is. Maybe I can get with Marcel on this, and we can figure out the details.

DR. REICHERT: That's fine, George, and I would include Roger Pugliese too, and there were some changes in the modeling personnel, which had some delays, and then some of the changes that were recommended were a little more complicated than maybe it should be thought, and then, with COVID, that kind of threw another wrench in the whole thing, and so it's my understanding that we can expect something maybe in the next couple of months, and then, when this was scrapped from the SSC agenda, the urgency to get everything done by this meeting kind of disappeared, and so there have been other things higher up on the priority.

DR. SEDBERRY: Okay. Thanks, Marcel. Any other SSC comments or questions regarding the council workplan?

DR. LANEY: I was just going to basically say what Marcel did. If you want to enlighten us all on the Ecopath schedule, talk to Roger, and it was also scrubbed from the Habitat AP agenda last week as well, and so we haven't been briefed on it either, and it sounds like, from what Marcel

just said, that there is more work that needs to be done on it, and so I'm sure we'll get some additional insight on when it will be coming to us for review at some point in the future.

OTHER BUSINESS

DR. SEDBERRY: Okay. I will get with Roger on that. Thanks for that suggestion. Any other comments or questions from the SSC regarding the council workplan? Okay. Is there any public comment regarding the council workplan? If you are a member of the public and would like to comment on the workplan, please raise your hand. Okay. I believe we are ready to move on to Other Business.

The first thing under Other Business is the National SSC Meeting, which, as of now, as of today, and this council change tomorrow, is scheduled to occur in Sitka, Alaska the first week in August. I think I've sent out an email to all the members regarding the themes for that workshop, but we have SSC members who have volunteered to attend and give presentations, and there will be council staff attending as well, or the whole thing may be cancelled, and that decision is to be made tomorrow, and so I will keep you posted. Not cancelled, but postponed. They wanted to wait until the first of May, to determine the logistics of getting people to Alaska from the east coast, the Pacific Islands, all over the country, and so it's a little -- It's a bit of a logistical problem, and I don't think it's going to happen in August, but we will see. I will update you when I have something. Any questions regarding the National SSC Meeting? Okay. This is the opportunity now for general public comment.

DR. ERRIGO: George, several SSC members hands went up, and they may have other business.

DR. SEDBERRY: I'm sorry. They somehow ended up at the bottom of my screen, and I missed them. Sorry. Alexei, go ahead.

DR. SHAROV: Very briefly, what kind of presentation can I expect for the South Atlantic SSC for the national workshop?

DR. SEDBERRY: One of the themes of the national workshop is managing fisheries under changing climate conditions, and, as you know, we've had that issue with blueline tilefish between the South Atlantic Council and the Mid-Atlantic Council, and Scott Crosson has volunteered to give a presentation on that, and so that's what the South Atlantic Council SSC is going to be presenting at the meeting.

DR. SHAROV: Thank you.

DR. SEDBERRY: Thank you.

DR. SCHUELLER: I had a question about the yellowtail review, and so that review workshop happened, and that was the joint review between the South Atlantic and the Gulf, and, next week, there was supposed to be the joint meeting between the SSCs to review that assessment, but I haven't seen anything about that, and what's the status of that?

DR. ERRIGO: That was cancelled. The meeting was cancelled, and I have not heard from them whether they have rescheduled it or what's going on, and so I will get in touch with Ryan Rindone,

who is the SSC person down at the Gulf, and see what's going on with that, if they have rescheduled it or not, if it's going to be via webinar, and see what the deal is, and I will get back to everybody.

DR. COLLIER: Julie Neer has her hand raised, and she will be able to address that. She says the joint meeting has been rescheduled to July 21 through the 23, is what Mike Travis had indicated, and it's indicating that Julie Neer is offline, and another person that might be able to talk about this is Dustin Addis. Dustin, do you have any recommendations on it, or thoughts on it?

MR. ADDIS: Thoughts on the rescheduling of the meeting?

DR. COLLIER: Yes, when it was rescheduled to.

MR. ADDIS: I have no clue at this moment. Sorry.

DR. COLLIER: Okay.

DR. SEDBERRY: Okay.

DR. SERCHUK: I had the same concern that Amy has, and I sent you and Mike an email this morning about this meeting that has apparently now been postponed, and presumably for the same reason that Amy brought it up, that I mentioned that I could participate in the meeting that was originally supposed to be next week, but I don't know whether I can actually participate if it's held in July, and so I just wanted to give you a heads-up on that.

DR. SEDBERRY: Okay. Thanks for letting us know, Fred.

DR. COLLIER: Julie is back online. She sent me a text saying the same thing, and so July 21 through 23.

DR. SEDBERRY: All right. Thanks.

DR. REICHERT: Mike, could you add yellowtail to that Table 1, so we have a complete overview of what's going on and who is involved in that?

DR. SEDBERRY: Yes, that would be a good idea.

DR. ERRIGO: Yes, I can.

DR. REICHERT: That would be great, and I always use these tables to look, if I have forgotten something or what's coming up, et cetera, and so that would be fantastic, and I appreciate that. Thank you.

DR. SEDBERRY: Seeing no other hands raised regarding the council workplan --

DR. ERRIGO: Now we're in Other Business, and there was one thing, if you guys wanted to see it, and I recalculated the ABCs for dolphin and wahoo. You didn't get a chance to look at them, because I didn't have them calculated.

DR. SEDBERRY: You put them in the table?

DR. ERRIGO: Well, I haven't sent out this new one. I will get that posted on the website, but this is dolphin, and it's this orange one. These are the new years that we use, from here to here.

DR. SEDBERRY: Okay, and so it did what we thought it would do.

DR. ERRIGO: Yes, and then wahoo is, again, the orange line.

DR. SEDBERRY: Okay. The results is as expected, although below the --

DR. ERRIGO: Yes. For wahoo, it's around 2.9 million pounds. For dolphin, it went up to 24.6 million pounds.

DR. REICHERT: Just a minor thing. If you bring up the wahoo, I think it would be -- When you send it out, it would be good to put those same black lines in there, to help people take a look at what we actually used as the time series. Thanks.

DR. ERRIGO: I will do that. I was just thinking the same thing. That's all I have.

DR. SEDBERRY: Thanks, Mike.

DR. COLLIER: Along those same lines, the SSC requested new projections for red porgy, and I was wondering how you wanted to review those. Do you want to do it through email and have those incorporated into your report, or how would you like those to be done?

DR. SEDBERRY: Can we do that? I'm not sure of all the Federal Register noticing and the public availability and that stuff, and can we deal with those projections via email, even though they would really only be visible to the SSC and include them in our report? That's fine with me, but I'm just not sure of the rules.

DR. ERRIGO: You have already discussed and decided what you're going to do with them. It will just be so you guys can see them, and it will go into the report so everybody can see them, and so there is no decisions being made on them.

DR. SEDBERRY: Okay. Yes, that would be fine, Chip. We will need to figure out a schedule for that, because the council meeting will be here before you know it.

DR. COLLIER: It looks like you want to have your report done by the 19th, and so that's two-and-a-half weeks. You want it done by May 19th, and I guess the question would go to Erik or Kyle of whether or not -- Or when they potentially could have these new projections done.

DR. WILLIAMS: I would say we could get it pretty quick, and I would say within certainly by the end of the first week in May, would probably be easily done.

DR. SEDBERRY: That would be good, and we would just leave a space in the report for that to be filled in and work on the other aspects of the report between now and then, and so that will

work. To finish up the agenda, I think what I would like to do, if it's okay with everybody, is do things a little bit out of order, and I don't know if this is possible or not, but first we'll take public comment, the final public comment of the meeting, and then we'll move from there to elections and covering the next meetings, and then we'll come back to our consensus statements and recommendations review, to just kind of run through Mike's notes and make sure that we have everything covered. Is there any reason we can't do that? Just speak up and let me know.

DR. SCHUELLER: I am fine with that, but I was just wondering if we could take a biological break.

DR. SEDBERRY: That sounds like an excellent suggestion. I see that Rusty has his hand raised, and so let's take a ten-minute biological break. When we come back, we'll take public comment from Rusty and whoever else would like to make public comment, and then we'll proceed as I outlined, and so let's come back here at four o'clock.

(Whereupon, a recess was taken.)

DR. SEDBERRY: Hello, everybody. Mike, are you ready?

DR. ERRIGO: Yes, indeed.

DR. SEDBERRY: Okay. Our next item of business is Public Comment, and I have Rusty Hudson in the queue, and, if there's anyone else that would like to speak, please raise your hand, but go ahead, Rusty.

PUBLIC COMMENT

MR. HUDSON: Great job, Chairman Sedberry and the SSC and the staff at the South Atlantic Council and everybody else from the council and others viewing. One thing I can say is I don't like being on a corded headset and mic. I'm getting a cordless, and that way I can walk around, and I can go and get a cup of coffee, and I don't eat bagels or doughnuts. I only look at them, and so I kind of miss that, humorously.

The first thing on king mackerel is I was real happy with the discussion, and I will just cut to the chase. Making that penalty go away, when we were not overfished and not overfishing, which kind of was a problem in my mind back at the last assessment recommendations, and so making that go away is a good day, and so thank you, and great work.

Greater amberjack, I just wanted to bring up that my fishing pictures and my time fishing in the 1960s and 1970s with the for-hire fleet out of Daytona, predominantly, is you would probably see about a forty-pound average size whole-weight amberjack in those inshore reefs, and we even sold those, but, as far as when we sold them, I mean, we sold them, or we wound up filleting them for the people and charging them a price of two-dollars for the shoulders off of the amberjack.

That way, it was brought up that we did not have to show them and explain the parasites or worms that people would see in them in, generally, bigger fish, and we would see up to seventy pounders, and we had a ninety-something pounder mounted in the restaurant and stuff like that, but that was

then, and these pictures, once Chip and all them get their standardization down pat, they might be able to fish up some information through the 1960s and 1970s for you.

Red porgy, it was brought up about offshore of the big break, the big ledge, twenty-eight-fathom ledge, whatever, 165 foot at the top and 200 foot at the bottom, generally, and, off of central Florida, let's say Fort Pierce on up to St. Augustine, down there, they're real close, and they're tighter, and they can get out to the deeper areas and stuff, but they've got a lot more current. Once you go to Cape Canaveral, everything vectors off, and so you're further from land, and so it's a little harder for Ponce Inlet people and Daytona and St. Augustine to go way offshore, but, at the bases of the Oculina, and remember that we've created two expanded anchors for no anchoring, and the first has been in effect since 2000, from Port Canaveral south, out to 600 foot, and most of the pinnacles are 220 to 330 foot.

At the base of these things, along with the snowies and some gray tile, blueline tile, are these big, fat red porgy that it was described, and those are supposed to be your males, I would assume, and then we expanded the new area, from Port Canaveral north. Now, if you wanted to go up to this one area to the north end of this expanded Oculina area, to get some porgies and whatever in that area, that 220 out to 330, you're going to have to run fifty-five miles from Ponce Inlet, one way, and so you don't see the for-hire fleet doing that stuff, and they have had these cold-water events, the glaciers melting or whatever, and some of that -- And the lionfish.

I mean, there's a whole bunch of things that could be going on, and, of course, what is the pressure on these animals? I have seen the downturns in MARMAP, and in SERFS, I guess, and I have seen the downturn in what people see, but it wasn't that long ago when I asked some people, a decade ago, where are the -- If you go offshore and catch some of these big red porgies out there in that deeper water, and, oh, we don't need to, and we catch them in here okay, and that was like a decade ago or something, and so those things have changed, but I just wanted to throw them items out there. Great job for tough circumstances, historically, and we'll see if we're going to have to make a habit of this over the next year.

DR. SEDBERRY: Thanks, Rusty. I appreciate those comments, and I appreciate the photographs and information that you provided to the FISHstory project and all of your comments over the years. You've been a great help to the SSC and to our stock assessments, and so I appreciate your comments.

MR. HUDSON: I did send Chip an item from a year or so ago on red porgy, and it's a great description of everything in the Atlantic Basin. Thank you.

DR. SEDBERRY: Thank you. Is there any other public comment at this time? I do not see any other hands raised, and so the next thing I would like to do, in skipping over the consensus and recommendation review, and we can come back to that, but I would like to take up the elections, and this is Agenda Item Number 11, and we have two -- Our elections are done every two years or three years, and I can't remember, and it's every hundred years, it seems like.

ELECTIONS

DR. REICHERT: Every two.

DR. SEDBERRY: Every two, and so we have elections for Chair and Vice Chair every two years, and so I was elected two years ago, and so my term is up, and I have decided not to run for another term, and so we have two members who are willing and able to be nominated as Chair and Vice Chair, and this one is a little bit of a challenge this time, because, normally, the Vice Chair would just step up to the Chair's position, and we would elect a new Vice Chair, but Rob Ahrens has resigned from the SSC, and so we don't have a Vice Chair to step up.

I was interested in -- We had to come up with a candidate for -- Or we need to come up with a candidate for Chair and a candidate for Vice Chair, and so, starting with Chair, Genny has agreed to be nominated for Chair, and so what I would like to do at this time is call for a nomination and a second on Genny as a nomination for Chair.

DR. REICHERT: **George, I would like to nominate Genny as a candidate for Chair.**

DR. SEDBERRY: Thank you, Marcel. Do I have a second?

DR. LANEY: I would second the nomination.

DR. SEDBERRY: Thank you, Wilson. Are there any other nominations for Chair? Okay. I don't see any hands raised, and so, ordinarily, I think under Roberts Rules, we would move and vote that the nominations be closed, but I'm just going to close the nominations, if that's okay with everyone. We can take a vote, and we can do that by asking if there are any objections.

DR. REICHERT: George, it would be good to ask Genny if she accepts the nomination.

DR. SEDBERRY: Thanks for reminding me. I am being very flexible with the Roberts Rules, and we should probably ask the candidate to leave the room, but I don't really know how to do that, and so I'm going to ask Genny if she accepts the nomination.

DR. NESSLAGE: I do. Thank you, George.

DR. SEDBERRY: Thank you, Genny. Okay. So there is no other nominations. Are there any objections to Genny as Chair of the South Atlantic Council SSC for the next term, which lasts two years, starts now and lasts for two years? **I see no objections, and so, by acclamation, Genny has been elected Chair of the SSC.** Congratulations, Genny.

DR. NESSLAGE: Thank you.

DR. SEDBERRY: I will be in touch with some tips.

DR. NESSLAGE: Please do. I am going to need them.

DR. ERRIGO: What he means is his condolences.

DR. SEDBERRY: Anyway, I really appreciate you stepping up and volunteering to be a candidate and agreeing to the nomination, and congratulations on your election, and so thank you very much. Then the other position we need to vote on is Vice Chair, and Jeff Buckel has agreed to be

nominated for Vice Chair, and so I would accept whoever would like to nominate Jeff for Vice Chair.

DR. NESSLAGE: **I would like to nominate Jeff as Vice Chair.**

DR. SEDBERRY: Genny has nominated Jeff as Vice Chair. Do I have a second?

DR. SCHARF: I will second it.

DR. SEDBERRY: So somebody seconded it.

DR. SCHARF: Fred Scharf.

DR. SEDBERRY: Okay, Fred. Are there any other nominations for Vice Chair? Jeff, are you still excited about being Vice Chair?

DR. BUCKEL: I am jumping up and down over here. Jeff Buckel has been nominated and seconded and has accepted the nomination. **Are there any objections to Jeff's election as Vice Chair of the SSC? I see no hands raised, and so congratulations, Jeff.**

DR. BUCKEL: Thank you.

MS. LANGE: Congratulations, Genny and Jeff.

DR. NESSLAGE: Thank you. Thank you, Jeff. Thank you, George.

DR. SEDBERRY: Thank you.

DR. REICHERT: I am not sure whether this is the appropriate time or the end of the meeting, but I just wanted to thank you for your two years as Chair. From personal experience, I know that it takes a lot of time and a lot of energy and a lot of effort, and so I really appreciate you leading us through these two years and providing your reports to the council, and so thank you for that.

NEXT MEETINGS

DR. SEDBERRY: Thank you, Marcel, and you've been -- We had kind of a rough year, with our Vice Chair resigning, and often not being able to make it to the meetings, and so you filled in a lot during my term as Chair as well, helping me out, and I appreciate that as well, and so thank you very much. We now have a Chair and Vice Chair.

At this point, normally, or at least the way it was when I was elected Chair, is the new Chair takes over the meeting at this point, but it feels a little awkward to do that, given the circumstances of the webinar and also the fact that Genny has gone from being a member to being Chair without the benefit of the experience as Vice Chair, and so I don't want to throw all this stuff at her at once, and so I thought, if it's okay with protocol and with the membership, that I would just finish up this meeting, and then Genny will take over after her official approval by the council at their June meeting, if that's okay with everyone. Any objection? Okay.

Again, we will come back to the consensus statements and the recommendations review, but I wanted to go over these last few agenda items, which are the upcoming meetings. For the SSC, we have the week of October 13 to 15, for a meeting in Charleston, and that is the preferred week, October 13 to 15, and we have October 20 to 22 as a backup, but some SSC members have indicated that those dates are not good, and so, if we can, let's settle on the October 13 to 15 dates for our fall meeting. Any discussion of that? I don't see any hands raised, and so October 13 to 15 it is. I am assuming, Mike, that -- Well, I don't know what -- Is that subject to approval by the council, or is there anything else that needs to be checked on that, or we're just recommending that as a date?

DR. ERRIGO: No, you will say that that's the date that works best for you, and then I will work with the people back at the office and everyone to get that set up.

DR. SEDBERRY: Okay. Great. Thanks. Then I assume that's okay with you, Genny?

DR. NESSLAGE: I will make it okay.

DR. SERCHUK: I wonder whether it would be appropriate, because the second sentence refers to me, and, last year, I had to choose between the two SSC meetings, and I attended the South Atlantic Council meeting last year. Unfortunately, they conflicted, and I wonder whether you or Genny, or maybe somebody from -- Or maybe Mike, but if you could just send a note to the New England Council and notify them that we've already scheduled our SSC meeting for the week of the 13th to the 15th.

DR. SEDBERRY: I don't see why not. Can you do something like that, Mike? I would not know who to contact.

DR. ERRIGO: Yes, I can do that. I think I would send it to their staff person who is in charge of the SSC.

DR. SERCHUK: That's Chris up there.

DR. ERRIGO: Okay.

DR. SERCHUK: Thank you.

DR. SEDBERRY: Thanks, Fred.

DR. SHAROV: We were supposed to have an extended meeting this week, in April, one more day longer than usual, and this didn't happen, and, for example, the Ecosim/Ecopath modeling has been pushed to October, and I wonder if you considered, or staff considered, the October meeting to be one day extended compared to the normal. Right now, it shows only essentially two-and-a-half days, considering travel, et cetera.

DR. SEDBERRY: Right. I don't know. Mike, do you have a preliminary agenda and have a feel for whether we can do the fall meeting in three days or we need that extra day that we had -- You know, some of the things that we were going to do this week, we have postponed, and so they're going to come up eventually, and do we need to have an extra day?

DR. ERRIGO: I don't have an idea yet. I will work on the agenda as time goes on here, and I will get a better feel for what's coming up. It will depend on what the council and the SSC want to or need to discuss. If there is time, or if you want me to make time, for it, we can even discuss the ORCS, or part of the ABC control rule, but that depends on what else there is to do.

DR. SHAROV: It would be great if you guys could give us a heads-up a little bit ahead of time, so that we could plan accordingly, not that I am desperate to have a four-day meeting instead of three, and I always love to see everybody, but I just thought that I would ask, because it's what we planned for April, and so I just wanted to make sure of that, to know how to plan for the October meeting.

DR. ERRIGO: As soon as I have a handle on it, I will let the SSC know.

DR. SEDBERRY: Great. Thanks, Mike. Yes, it always helps, in planning, to know as far in advance as possible how long the meetings are going to take and when they're going to be, and so we'll get that figured out as soon as we can. The other meetings coming up are the regular council meetings, which there will be a presentation from this meeting to be presented at that meeting by Genny, and that meeting is scheduled for June 8 through 12, and is that going to be a webinar, or is that going to be an in-person meeting?

DR. ERRIGO: That's going to be a webinar. That decision was made after this went out, and so I didn't have a chance to change that.

DR. SEDBERRY: Okay.

DR. COLLIER: We're pretty sure it's going to be a webinar. We're still trying to narrow that down a little bit, and there was still some discussions going on, and it's not likely to be in the Keys though.

DR. SEDBERRY: Okay. If the agenda is abbreviated, because of the circumstances, there will still be an SSC report at that meeting, and is that correct?

DR. ERRIGO: Yes. They will definitely be discussing some of the assessments that you guys went over, for sure, and I'm pretty sure that dolphin and wahoo will get discussed, and I'm not sure about the other unassessed species, but there will be plenty that you guys discussed this week that will be on the council's agenda.

DR. SEDBERRY: Okay. There is three committees, I guess, that are meeting that week that we have addressed assessments for, Snapper Grouper, Dolphin Wahoo, and Cobia Mackerel, coastal migratory, and my brain is fried right now, and I can't remember where mackerels fit in.

DR. ERRIGO: Mackerel Cobia.

DR. SEDBERRY: Mackerel Cobia. Okay. So three different committees that the SSC will report to at that meeting, and then the regular South Atlantic Council meeting in Charleston, and then the one in December will follow our October SSC meeting.

DR. COLLIER: The one thing I don't see listed on there, George, is the joint meeting with the Gulf Council to discuss yellowtail snapper. That's going to be in July.

DR. SEDBERRY: Right. That's a SEDAR thing, isn't it?

DR. COLLIER: Well, it's going to be the SSC review.

DR. SEDBERRY: That's right. Okay.

DR. ERRIGO: It's actually a Gulf Council SSC meeting.

DR. SEDBERRY: Okay.

DR. COLLIER: It's a joint SSC review of that assessment.

DR. SEDBERRY: Right.

DR. ERRIGO: I will get in touch with Ryan, because I will bet that they are planning on having it in person, since they moved it to July, and just confirm everything with him.

MS. LANGE: Didn't Julie just say that it was going to be a webinar on July 21 through 23, or is that a different meeting?

DR. ERRIGO: It's July 21 to 23, and did she say it was a webinar? I'm sorry. I missed that then.

DR. SEDBERRY: Yes, I missed that, if she said.

MS. LANGE: Someone said it was a webinar. Chip was reading a note from her, I thought.

DR. ERRIGO: Okay. If it's going to be a webinar, then anyone can attend who has the time, and there's no travel involved.

DR. REICHERT: A quick organizational question. Is that a webinar where the SSC is supposed to review this assessment, and so do we need the full SSC for that, ideally, because I got a little confused, because there was a sub-group of the SSC who was going to review the assessment, but that may have been a different meeting, and so please clarify that.

DR. ERRIGO: That is a good question. When we were meeting in person, we had a sub-group that was going to be there in person that was representing the South Atlantic SSC.

DR. COLLIER: They are planning an in-person meeting for this as well.

DR. ERRIGO: Okay, and so they are, and so it will again be a sub-group that will be representing the South Atlantic SSC, and they are going to probably broadcast it by webinar, so others can sit in on it, but it will be the sub-group who is there in person that will be representing the South Atlantic SSC and be participating in the review.

MS. LANGE: I apologize. I misunderstood.

DR. SEDBERRY: Okay.

DR. REICHERT: Me too, Anne, and so don't worry.

DR. GRIMES: Who is the sub-group?

DR. ERRIGO: Forgive me. I don't have that in front of me, and I don't remember off the top of my head, but I will gather that information and let everyone know.

DR. COLLIER: There wasn't a sub-group that was assigned to this meeting. It was whoever wanted to attend could attend the meeting.

DR. ERRIGO: That may change now, because it's in July, rather than in April.

DR. SERCHUK: I indicated initially that I could attend the meeting in May, but I cannot attend the meeting in July, and I would just point that out to you. Second of all, just to notify Mike that the person in charge, the staff member, is Chris Kellogg at the New England Council. Thirdly, I would prefer, and I know we've discussed, the SSC has discussed, this in the past, but I would not prefer to extend our meeting to another day. Rather, I would prefer to have a half-day webinar to discuss some of the committee reports, and that would, I think, be much more efficient, and I know we discussed having webinars as a possibility in between our regularly-scheduled spring and autumn meetings, and I hope that idea is still one that we can consider. Thank you.

DR. SEDBERRY: Thanks, Fred.

DR. REICHERT: I agree with Fred at this point. Either we add a full SSC meeting, or we'll just keep the length of the meeting as-is. Personally, at the end of a two-and-a-half or three-day meeting, I am fried, and so I'm not sure how productive a longer meeting is going to be, and so I second Fred's suggestion.

CONSENSUS STATEMENTS AND RECOMMENDATIONS REVIEW

DR. SEDBERRY: Thanks, Marcel. Any other questions or discussion relative to upcoming meetings? Then I think the last thing we need to cover is the consensus statements and recommendations review and the report. As I mentioned, the way this would normally be done, I think, is that -- Or historically has been done, is that Genny and Mike would put together the report for this meeting, but, again, since Genny has not had the opportunity to be Vice Chair and be involved in this before, I thought I would help her out with this one, and I can work with Mike to get at least a first draft of the report going, and then bring in Genny at that point.

From this meeting, the notetakers who took notes today, again, submit your notes to Mike and I, and we will compare them to the Google Doc that Mike has been working on all along and supplement, complement, add or subtract to it as necessary, based on the notes that we receive from you, but the one thing we do like to do before we break up is there's often additional discussion via email after the meeting is over about what was said and what wasn't said, and I am really reluctant to make changes by email when we don't have the whole group gathered together to make sure that this was something that we all agreed on.

The important consensus statements and recommendations and research recommendations and the conclusions that we come to, I like to go over them before we break up, and we do that from Mike's notes, to make sure we have captured all the important statements and we have them worded in a way that meets the SSC's intent. We will take some time here to scroll through this and see if there's anything that any member feels needs to be changed, and I'm going to be reading through this along with you as we go, and so I may miss your hand raised, and so, if you have something, I think it will work if you just speak up.

The first major item, agenda item, that we took up was the terms of reference for the three species that are undergoing SEDARs in the near future, and so it's SEDAR activities, and it's red snapper, black sea bass, and Spanish mackerel, and so starting with the red snapper.

DR. ERRIGO: This is a little disjoint, I think, because we went round-and-round about this particular bullet point here.

DR. SEDBERRY: The TOR Number 5?

DR. ERRIGO: Yes.

DR. SEDBERRY: Yes. I am going to have to actually pull up the document to see it.

DR. COLLIER: Do you want to include that TOR language in there?

DR. ERRIGO: What I was thinking of doing is including the updated language.

MS. LANGE: Do we need all the other lines? Can't we just say that we recommend changing the language to, and then put it in, the proposed language?

DR. SEDBERRY: This is that language where it makes it sound like a panel of the SSC is going to be convened, and it's really just SSC members convening with the panel.

DR. ERRIGO: Yes, that's correct, and I think we could just say to replace the language in TOR Number 5 with the proposed language.

DR. SEDBERRY: Right. That will do it.

DR. ERRIGO: The SSC recommends --

DR. SEDBERRY: I know that that language was written out somewhere, and maybe it was --

DR. ERRIGO: I have it in an email, and I will put it in here.

DR. SEDBERRY: Okay, because we did go over that, and we did come up with wording that we all agreed on that would go into each of these regarding Term of Reference Number 5.

DR. SERCHUK: Because of the issues that we had with the updated natural mortality estimates in one of the assessments, I am -- I don't know what is allowed when we say, for example, evaluate

any changes allowed for this update, and I wonder whether we need to maybe, in the future, take a closer look about informing, in our terms of reference, what we allow to be changed in an update and what we don't, just to be clear to the individuals that are going to be involved with these assessments. That's just a thought, because we had this problem about, well, we didn't want to change the natural mortality from the Lorenzen, because that's what we had. I am not sure what is allowed in an update.

DR. ERRIGO: This assessment here will be an operational, and it does range between an update and a standard, but, the way the terms of reference were written, it's much more towards a standard, if not more than that.

DR. SERCHUK: That's why I am seeking clarity.

DR. SEDBERRY: So is this a recommendation that the SSC needs to make to the SEDAR Committee, or is this something that needs to be taken up by the SEDAR Committee?

MS. LANGE: I just assumed that it might have to be taken up by SEDAR.

DR. GRIMES: My comment was I had written this down as a notetaker, because I remember Fred saying this, and is it a great idea for us to be real prescriptive about how they should conduct a new assessment? I mean, if they come up with a better way of doing something, for example a way of estimating natural mortality, do you really want to constrain them so that they can't use something like that?

DR. SEDBERRY: Right, and there is a SEDAR Committee to deal with things like that. I'm not sure that this is something the SSC should be doing.

DR. SERCHUK: I am just concerned about the best scientific information available.

DR. GRIMES: Right, but, if you tell them that they have to do it in a way that was not as good as a new way, that seems contrary.

MS. LANGE: Is John Carmichael still on the call, because he might be able to clarify the difference between standard assessments and the old updates, which only allowed you to add new data and no changes. Does the new operational assessment -- I think that's the lowest level, and does that allow updating things? I think it does.

DR. ERRIGO: The new operational assessments are still kind of in flux, but they allow -- It depends on the terms of reference, and they can allow either just the turn of the crank or, like you said, we just want the Science Center to take care of it, and we're not going to have any workshops, and we're not going to do much online, and we'll just do this, or we can say that we're going to convene panels, and we're going to assess new data sources and this and that and the other thing, and it depends on how the terms of reference are written.

DR. COLLIER: George, Julie Neer, who is a Program Manager for SEDAR, has her hand raised.

DR. SEDBERRY: Go ahead, Julie.

MS. NEER: Mike covered it pretty well, but, yes, basically, the operational assessments are governed by what is called -- Initially, it's by a statement of work, and the statement of work is something that the council puts together, in discussions with the SSC frequently, to discuss what they would like to see in this next assessment.

They give that information to regard to scope of work, how much we can change or not change, whether we want a panel or no panel, that sort of stuff, whether there's a new dataset that we are aware of that we would like to be considered, and that statement of work goes to the Science Center. The Science Center considers the statements of work for all the cooperators within SEDAR, and it helps them determine what they can and can't do, and so they might be able to say that we can do all of these assessments, however, these three have to be more towards the update range, which means no panels and no workshops and that sort of thing, or we can do one of these that takes us more time and addresses all these issues, but you're only getting one.

It's a negotiation, and it's a planning tool. Once that is decided upon between the Center and in discussions with the council, then you guys get the -- Then the terms of reference are developed, and they come to you, and ultimately back to the council again, and so you guys sort of control the level of detail, but I think it's important, like what Church said, is you guys are doing statements of work and terms of reference a year or a year-and-a-half out.

Like Church's example, if we say we are using Charnov, and you put that in the terms of reference, and, by the time the assessment gets started, there's a new best method, you have locked the guys into that Charnov, and so I do think Church made a good point that you need to think about how prescriptive you are being, and it may tie the analysts' hands, on some components, and so you just need to think about it's a balance, and I hope that helps.

DR. REICHERT: I am not sure if Mike is still editing that, and I may have missed it, but that -- Outside of the workshop, and then we have to review it at the SEDAR workshop in December, and I thought that workshop was going to be held in December and then reviewed at the SSC meeting.

DR. ERRIGO: The outside workshop was going to be held hopefully this summer, in June or July.

DR. REICHERT: But it says, "and will be reviewed at the SEDAR workshop in December".

DR. ERRIGO: The SEDAR workshop is for red snapper, the data workshop.

DR. REICHERT: Okay. Sorry. You're right. Okay. Thanks.

MS. NEER: So perhaps add "at the red snapper SEDAR workshop in December of 2020".

DR. REICHERT: Thank you. Yes, I think that would be great.

MS. NEER: Or the SEDAR 73 workshop, which is what red snapper is going to be numbered, whichever you prefer.

DR. REICHERT: So the results of that workshop will not be reviewed by the SSC separately, but as part of the SEDAR 73 assessment report?

DR. ERRIGO: Actually, if it's held over the summer, it will probably, or it could, come to the SSC in October.

DR. REICHERT: Well, but that's not in here, and that's why I'm asking that.

DR. COLLIER: I don't know if we'll be able to get it done over the summer, Mike. I mean, we're not even in the planning stages yet.

DR. ERRIGO: Okay.

DR. SEDBERRY: Okay. Are we good with this? Okay. Are we ready to move on to black sea bass? Again, we had the language problem with TOR Number 5.

DR. ERRIGO: I will get the language for this one, the exact language, how it was reworded, for black sea bass.

DR. SEDBERRY: Right. Any other comments? Are you ready to look at Spanish mackerel?

DR. SERCHUK: The note at the bottom of the Spanish mackerel TORs says this assessment will follow an operational assessment approach that is similar to an update assessment, and that was not in black sea bass, and I'm just wondering why it was placed in Spanish mackerel.

DR. ERRIGO: That's how the scope of work was written, I think, and how the TORs were written.

DR. SERCHUK: It seems to me that the terms of reference are very much similar between the black sea bass and the Spanish mackerel. They talk about an update, and then they talk about specific things to look at. Everything else after that is essentially the same, but we have this additional note on Spanish mackerel, and I'm just wondering whether that note is confusing, and, if it isn't confusing, why wasn't it added to black sea bass?

DR. ERRIGO: I'm sorry, but I don't have them in front of me. Is TOR Number 5 the one about the -- Yes, it is.

DR. SERCHUK: But, after 6 on the Spanish mackerel, it says "note", and there's a little sentence there.

DR. COLLIER: Julie Neer is recommending taking that out.

DR. SERCHUK: Okay. That's fine. Again, I want to be consistent, and that's all.

MS. NEER: Again, that was more of a note for the scope of work, to help the Science Center with their planning, and it probably should not have been included in the actual terms of reference of how to do the assessment. Sorry.

DR. SERCHUK: Thank you.

DR. SEDBERRY: Thanks, Fred. Thanks, Julie. Okay. Scrolling on down, I think the next action item has to do with the selectivity workshop, and that was just selecting participants. Do we have anything else to say about that? We just needed to request participants. Okay. That's pretty straightforward. Okay. Go ahead, Mike.

We looked at the new weight methodology, which affected the landings, which had an effect on the ABC recommendations, and so we've updated our ABC recommendations, and that got a little bit more complicated when we talked about dolphin and wahoo, but we stuck with the third-highest, but a different range of years, a broader range of years, and then we made a statement about revisiting ORCS in the ABC control rule revision.

DR. ERRIGO: This note is here, and I could probably just take that out, but we had the same recommendation in October.

DR. SEDBERRY: Yes.

DR. ERRIGO: But, if this wording is okay with everyone, I can just remove that.

DR. SEDBERRY: As long as it doesn't conflict at all, in any way, with October. Otherwise, we get called on those kinds of things, when we say one thing one meeting and then something a little different the next meeting, but I think that's exactly the way it was worded, but we probably should leave that note in there for now, Mike, so we can check and make sure.

DR. ERRIGO: Okay.

DR. NESSLAGE: I thought we were going to put in justification -- I thought there was justification in one of our reports, based on concerns about MSE performance of ORCS, and were we going to add that? Otherwise -- Or is the council already familiar with why we're concerned about ORCS? It might be worth adding some detail there.

DR. ERRIGO: Yes, and you guys can add all that detail when you're going over the report. These are just the high-view consensus statements.

DR. NESSLAGE: Right, but I just was saying that I think that note you put there for yourself, because I thought I remembered you saying you put that in there just so that we would remember to put in the report that bit, and so don't take it out yet, is what I'm saying, because we might need it as a mind-jogger later.

DR. ERRIGO: Okay.

DR. NESSLAGE: Thanks.

DR. SEDBERRY: There is wording for that justification in the October report, and so that will help us to remember to look at that.

DR. ERRIGO: Are we good with the blue addition here that Amy put in? That wasn't in the original wording, but, if we're good with that, I'll make sure it gets added. I will add it in.

DR. SEDBERRY: This is something that one of the notetakers added in, and I want to make sure that the SSC agrees that this was our intention.

DR. SCHUELLER: I added that in because this particular section is very specific to dolphin and wahoo, and we had discussed that this also needed to be addressed for snapper grouper, and so it's just making sure we're consistently putting out the message that this needs to be addressed for all groups of species.

DR. ERRIGO: I see. Okay.

DR. SEDBERRY: Thanks, Amy.

DR. ERRIGO: Okay. If we're good, we can move on to king mackerel. I think we decided that will stay in for king mackerel.

DR. SEDBERRY: I think that's right, Mike, and that was because of the difference in the timeframe.

DR. ERRIGO: Yes, and this was a different kind of assessment. Given the TORs, the analyst wasn't keeping it as much -- This was an update, this assessment, and I think it's the last real update, and it's not an operational, and so they did not change certain things as much as they could, and they stuck to the original assessment, benchmark. They stuck to the original benchmark.

DR. SEDBERRY: Okay, and so I think we're okay with the review assessment bullet. The identify, summarize, and discuss assessment uncertainties, our note there is there's an issue with the convergence gradient being too high, which raises concerns of the correlation of parameter versus -- Genny sent out a more specific email about that to members, and I wanted to make sure that what she says in that email is something we have discussed and we agree, and she got kind of specific about what needed to be done there, and I'm not very knowledgeable about the model, and so I wanted to make sure that, what she has there, that we're all in agreement with.

DR. NESSLAGE: I think that would be under research recommendations, George, and I happy to make this particular bullet we're looking at now -- I will clean that up, once I get to editing the report, and make sure it's very specific, so they know what we're talking about.

DR. SEDBERRY: Okay. We do need to clean it up, because I'm confused about it, but that's not too surprising, but, yes, I just want to make sure it's clear what we're talking about there. Okay.

DR. SERCHUK: Can I just ask what the impact of the issue is relative to what a manager should take away from this statement?

DR. NESSLAGE: Fred, I remember you asking for that, and I will make sure that I will address it, and I think we don't know what the impact is, and that's what scares me, and so, if the convergence gradient is high, that means the model is not -- Well, it often means that several of the parameters are correlated and it can't estimate them well independently, and so the answer you might get -- The estimates coming out of that model may not be the actual -- Well, one, the model probably can't estimate everything that they are trying to estimate with the data that they have, which is, in my mind, a problem.

How big of a problem, I can't tell you, because we don't know which parameters, and they didn't seem to want to open up the core file for us yesterday, which it's a pretty quick glance, and it might be a pretty quick fix, and it might be nothing, or it might be something substantial, and so I really can't say, without knowing exactly which parameters are affected, but --

DR. SERCHUK: Okay, because I'm just concerned about the managers are going to read this and say, should I believe the estimates of stock status and fishing level recommendations or should I not, and that's what this --

DR. NESSLAGE: I know, and, honestly, I was -- I am very concerned about it, but other folks didn't seem as concerned, and so I backed off, but --

DR. SERCHUK: Okay. It's just that I know, for us, we understand what the correlation of parameters are, and the convergence gradient being too high, but, if we can put it into language that says, nonetheless, we think the estimates are in the ballpark, or something else like that. Otherwise, I don't think we're doing our job properly in trying to communicate to the people that have to evaluate our advice, in terms of how it affects the stock status and fishing level recommendations, and it's not a criticism of you at all, Genny, but it's just I think we have to be very specific about these things, in terms that a layperson could understand. Thank you.

DR. NESSLAGE: Yes, and I don't know what to tell you, because, in putting on my assessment hat, as I said before, this could be a big deal, or it could be nothing, and so, because the interpretation of the analysts, and it seemed most of the SSC, was that this was an update, it's not something that should be explored, even though I disagree, and I think models that don't converge, no matter what sort of assessment track they are on, should be thoroughly explored until they actually settle on a decent solution. I think that's a dangerous road to go down, but I recognized that I was in the minority, and so I can try to clean this up, so that it's clear that there's an issue that needs to be addressed by the next assessment, which is where my language that I sent around was going, and I'm happy to concede the point now, but I think it needs to be fixed by the next assessment.

DR. SEDBERRY: Genny, if you feel like you're in the minority, that may just be because you know something that we don't know, and that's important, and it just needs to be explained so that we can all understand what you're seeing that I don't see, at least, and I don't see it because I'm not familiar with this stuff, and so it just needs some words put around it to explain what's going on there.

DR. NESSLAGE: Let me take a crack at it, and, before it gets sent out to the whole SSC, and then you guys can tell me if it's, and Fred especially, if you think that it's communicated correctly to the right audience, and does that sound fair?

DR. SEDBERRY: Yes.

DR. ERRIGO: So are we good with this section here? Are we good to move on?

DR. SEDBERRY: I think so.

DR. GRIMES: This can be done later, but I was just going to say, in that second thing you have, uncertainty surrounding how the winter mixing zone landings are handled, whether they were assigned to Gulf or Atlantic stock, and maybe that's clear.

DR. SEDBERRY: No, that's better.

DR. ERRIGO: I will move us down here.

DR. SEDBERRY: There's the ABC control rule amendment development being brought up again.

DR. ERRIGO: I did not -- I forgot to put in "pieces". That certainly needs to be wordsmithed, but that's just the gist.

DR. SEDBERRY: Right.

DR. ERRIGO: Then we'll go down this bullet here, and I put this in here in response to that apparent age truncation from the commercial handline, due to the decrease in sampling off of North Carolina, but, if you don't agree with it, or if you need it to be changed in some way, please let me know.

DR. SEDBERRY: That truncation definitely shows up in the figures, and the explanation for it was that the sampling that had occurred off of North Carolina was no longer occurring, and so it seems to me that, if you're going to have a consistent time series of samples, that that needs to be restored, and so that looks good to me, Mike.

DR. ERRIGO: All right. Good, and I just pasted in Genny's proposed language for a research recommendation regarding the convergence issue.

DR. SEDBERRY: Okay.

DR. LANEY: Mike, was that the increased sampling of commercial handline, or was that the tournament sampling that we were talking about? Remember Steve said that he was going to check on that? We talked about the fact that he wasn't sure whether there might be a bunch of samples sitting in a warehouse somewhere, and he was going to follow up on that. Was it him, or was it tournament?

DR. ERRIGO: The issue with age truncation came from the fact that there was a decrease in sampling from commercial handline in particular, and there was another issue that you guys were talking about with tournaments being sampled, that they didn't get the samples from tournaments, and that they were making some assumptions or something like that. I think that's what that was.

DR. SEDBERRY: The truncation that I was thinking of though was in the commercial handline data, and you can just see going down the years, the more recent years, that it was cut off.

DR. ERRIGO: Steve actually has his hand raised, which he will know better than me.

DR. SEDBERRY: Okay. Go ahead, Steve.

MR. POLAND: I don't have an answer to that, and I was going to ask the same question that Wilson asked, because I was under the impression that it was the tournament sampling, and, from the discussion we had on Tuesday, it was that there was a one-off program up here, and Sea Grant actually funded it, where they worked with tournament fishermen to log their lengths of all king mackerel caught during tournaments, and so retained fish as well as released fish, and we do sample tournaments at the division, but that's only for collecting age structures, and we collect lengths for those fish that we sample, but we're trying to fill size bins, to fill out the age-length bins, and we're not sampling lengths to characterize that fishery, because, I mean, there's no way that we can get an accurate, unbiased sample of all king mackerel landed and discarded in the tournaments.

As far as the commercial sampling, I myself was unaware that our sampling numbers have dropped, and I will look into that and contact our staff who are in charge of fishery-dependent sampling and king mackerel, and, also, the data that was provided for the assessment update, and try to see what's going on there.

DR. SEDBERRY: Thanks, Steve. Genny, did you have a question?

DR. NESSLAGE: I was just going to jog people's memory that this had to do with the last two years of data, and he thought it had to do with the hurricanes impacting the sampling of older fish off of North Carolina, and he ended up taking the age comps out of the model in those two years because of it, and so, if it was a temporary thing, it will be coming back, and that's great, but, if Steve can follow-up on that, that would be awesome, but that's where that was coming from, as I recall. It's not increasing it, but seeing if it will return to prior levels, I guess, is the question.

MR. POLAND: That might very well be the case. I mean, we have not changed anything, as far as our sampling protocols, for our fishery-dependent sampling, except recently, with the COVID-19 and the fact that we can't go sample anymore, but it was most likely hurricanes. In 2016 and 2017, we had two hurricanes that did impact the Outer Banks quite a bit, but I'm still going to try to figure out what was going on.

DR. NESSLAGE: George, or Mike, can we scroll back up? Can we just massage the wording, so it's clear? It's not that we want to increase sampling, but investigate -- Something like identify if sampling levels will return to pre-hurricane levels, something like that, in North Carolina. It's not that the pre-hurricane levels were bad. It was, as I recall, the discussion was that we just hope that folks will be able to get out on the water, or out to actually sample again, someday, after the pandemic. Thanks.

DR. SEDBERRY: Thank you. I know what is meant there, Mike, but I think we'll need to wordsmith that a little bit, but I think you can leave it by just calling it pre-hurricane levels, and now we know what that means.

DR. ERRIGO: Okay. Cool. Thanks. I also put in Wilson's research recommendation, and here is Genny's here, and this one had to do with the mixing zones, the mixing zone catch ratio.

DR. SEDBERRY: Right. Okay, Mike.

DR. ERRIGO: All right.

DR. NESSLAGE: Sorry, but we scrolled past it, and were we supposed to provide guidance on timing for mackerel? The reason that I bring it up is right there. The reason that I bring that up is because my research recommendation deals with that, and I don't -- If we need to do that in this report, I think I would not feel comfortable making that determination until we see the results of their exploration of the cause of the convergence problem, because, if it turns out to be something easy to fix, sure, you could do an update with the appropriate TORs, or you could do a very simple operational, whatever, but, if it ends up being a big problem, they might need to make major changes to the model, and so that does make sense, what my concern is?

DR. ERRIGO: Yes.

DR. NESSLAGE: Can we say that like we're going to table that until we see the results of their exploration? Perfect. Thanks, Mike.

DR. SEDBERRY: Okay. Greater amberjack. We've had quite a bit of discussion about them, and I want to make sure that we've captured our recommendations and concerns here.

DR. SHAROV: I think we need to provide a little bit more information on uncertainty, and so when it asks to identify, summarize, and discuss assessment uncertainties, if you want to, we can list -- They have looked at a number of things, but, generally, with respect to the determination of the stock status and the consequences to fishing level recommendations, the only component that the model was sensitive to was the natural mortality, and hence our discussion that we had, and so I think what we should start with here is we should say something to the extent of like sensitivity analysis indicated that the model results are, or were, most sensitive to changes in the natural mortality rate, and that would be correct, because everything else had a very negligible effect on the estimates.

Then, again, by looking at the results of the sensitivity analysis, we probably should be saying that the application of the natural mortality rate used in SEDAR whatever number it was, or say in the previous assessment, lead to a different interpretation of status of the stock, and maybe add that that could call for caution, but, after that, just say, however, the SSC considers that the Charnov method is appropriate, or most appropriate, for estimating in this assessment, and then this choice is also supported by the likelihood profile analysis, or M, and that would do it.

So we identify the principal sources of uncertainty that affect the determination of status of the stock, and we say that the lower M that was explored in this assessment was based on the life history analysis of the previous assessment, and it provides a different determination, but the SSC believes that the new methodology is most appropriate and that it is supported by -- In addition, it's supported by the likelihood profiling for M.

DR. ERRIGO: It would be great if you could provide language, because that was a lot.

DR. SHAROV: Sure.

DR. ERRIGO: Is the gist here that the sensitivity analyses indicated that the model was most sensitive to M and that the choice of Charnov over Lorenzen had a large impact on the stock status result in the assessment, which was shown in the sensitivity results? The SSC considers the

Charnov method an appropriate method for estimating M in this assessment, and the likelihood profile of the Charnov M scalar suggests that M is likely to be higher than the Lorenzen M , perhaps even higher than the Charnov M . The SSC notes that, if the Lorenzen method was used, the biomass exploitation status would be much closer to or beyond their benchmark limits for much of the time series.

DR. SHAROV: I recall that Fred didn't like the use of the specific names applied to the methods, and so the suggestion was to keep it to more layman sort of language, and so, instead of using Charnov and Lorenzen, we could say method used in the SEDAR whatever previous number, 15, versus SEDAR whatever the new one is, 59, if that would make it more readable.

DR. ERRIGO: Yes, and definitely there should be wordsmithing, to make it more readable. Those are in there so that you guys know what is going on, for sure.

DR. SERCHUK: I am still having a little heartburn about the last bullet in this section about the SSC notes that if the Lorenzen method was used, because we say that the SSC considers the Charnov method appropriate, and why would we say that, if we use another method, the results would be different? We know that, and I just -- If we think the Charnov method is appropriate, I think we should leave it at that. Thank you.

DR. SEDBERRY: Thank you, Fred. I don't remember whose suggestion this was or why we put it in there, but, at some point, we wanted it.

DR. SCHARF: George, I wonder if that statement -- It's an accurate statement, and it does show the large effect of changing the methodology and estimating M , and I wonder if it might be better above, under the first bullet, and there's a statement that says the choice of the Charnov M over the Lorenzen M had a large impact on stock status, and maybe it would be better if it followed that.

DR. ERRIGO: The reason why it's down here is because, if you read this bullet, it asks you to describe the risks and consequences of the assessment uncertainties with regard to status and fishing level recommendations. That's why it got stuck down there.

DR. SERCHUK: I guess my heartburn is, Chairman, that the choice of the method of natural mortality should not be conditioned on whether one is closer or beyond benchmarks. It should be on the method itself and not on the implications of the method, and that's where I get concerned about that final statement there.

DR. SEDBERRY: I don't think that final statement is saying that's why we chose it, but it's --

DR. SERCHUK: No, but that's what -- I think somebody will interpret it that way, Chairman.

DR. SEDBERRY: Well, I can't predict how people will interpret things.

MS. LANGE: I agree with Fred. I think it counters -- We have stated that we believe the Charnov method is the appropriate method, and, in the previous bullet, we say the choice had a large impact, and that's that, and I don't see a reason to include confusing bullets that counter and raise a question on why we made the decision that we did. It sounds as though we made the decision

because of the results, and I don't think that was the reason we did. We felt that the Charnov was the more appropriate method.

DR. NESSLAGE: This bullet is specifically with regard to the risks and consequences to fishing level recommendations. The risk and consequence of adopting the Charnov method is that, if the Lorenzen is actually closer to the truth, the stock status -- We could be overfishing and overfished, and so that's the risk of making this jump.

We all know that natural mortality is the hardest thing, one of the hardest things, to deal with in these assessments, and, yes, we have some indication and a lot of good work going into the decision to move to Charnov, but I don't think any of us would bet our nest egg for retirement on Charnov being better than Lorenzen. If that's the case, then maybe -- I wouldn't, and so I think it's worthwhile telling the council that there is a risk associated with this jump, and we could be very wrong, but, overall, we think it's the best available science, and we stand behind it, and I think we can be honest with them, but that's my two-cents.

DR. SEDBERRY: Thanks, Genny.

MR. POLAND: I just wanted to follow-up with Genny's comments. I understand what this means, and I understand the point that the SSC is making, and certainly I could see how it could be misinterpreted or construed, but that's really -- I guess it will be Genny's responsibility to communicate that to us in June, and, myself as the SSC liaison, to also communicate what this means to the council, so it isn't misinterpreted, and so I don't -- What I see on the screen, I'm comfortable with it, and I'm just throwing that out there.

DR. SEDBERRY: Okay. Thanks, Steve.

DR. SHAROV: I just wanted to say that -- What Genny said is exactly what I was trying to sort of communicate to the committee. This is about describing the risks and consequences, and, if the committee doesn't like the description of the consequences of the analysis, or the fact of the use of the natural mortality estimates from the previous assessment, which is part of this assessment, then I would suggest that -- Then what do we say? Offer something else, and what do we want to say about the risks and the consequences of the assessment uncertainties? I think what we tried here is a description of the potential risks, and I don't want to repeat myself multiple times, but I think the argument was made pretty clearly.

DR. SEDBERRY: Thanks, Alexei.

DR. LANEY: I agree with Genny's point on this, and, as long as she and Steve are both there to explain it to the council, I don't think there will be any misinterpretation, at least at the council level, and I agree that this is a risk and a consequence of the assessment uncertainty, and so I think we ought to stick it in there, and it's just being true to our deliberations and to the council, to tell them what's up.

DR. SEDBERRY: Okay. Thanks, Wilson.

DR. SERCHUK: I can go along with the comments that have been made, in terms of agreeing to a consensus, but what it suggests to me is we don't know whether the Charnov or the Lorenzen

are better, because they can have large differences, and it seems to me that we should then ask, in future assessments, to do it both ways, and the discussions we had seem to -- In some cases, the Charnov may be better, and in some cases the Lorenzen may be better, and so, if we really want to capture what the impact of using either is, we should probably request that both be done and see what the differential impacts are in every assessment where natural mortality has to be estimated. Thank you.

DR. SEDBERRY: Thanks, Fred. Are we good now with the way that the major bullet that starts with identify, summarize, and discuss assessment uncertainties and its sub-bullets are worded? Any suggestions or further discussion? Again, this is a draft, and we'll get a chance to refine this and wordsmith it in the next couple of weeks.

DR. SHAROV: Just one thing. If you want to, with respect to the uncertainty on M, there were also results of the bootstrapping on the M estimates for the Charnov method, which suggested the sort of distribution of high probabilities of Ms being higher, which would result even in a more optimistic viewpoint of the status of the stock, and so this is also a result, although it doesn't carry a risk, and it's sort of a very -- It's in the direction of a sort of positive outcome, and so it might not be necessarily of a concern, but, if you want to be complete, you can mention that as well.

DR. SEDBERRY: Mike, are you working on some wording for that? Yes.

DR. ERRIGO: How about I just put that in there for now, to sort of remind you that you want to incorporate that in there? Probably that's enough to jog your memories of what it means.

DR. SEDBERRY: Any additional questions or comments or suggested edits on this section? Okay, Mike. We have the fishing level recommendations. Any additions or subtractions or edits to this section? I don't see any hands raised.

MS. LANGE: Are we on the research section as well, because I thought that Mike was going to include in there the recommendations that were on the slide from Kevin.

DR. ERRIGO: I will get those in there, yes.

MS. LANGE: Okay. I wasn't pressing, but I just wanted to make sure that was considered.

DR. ERRIGO: Yes.

DR. SEDBERRY: Thanks, Anne. We can maybe just put a placeholder there, so we make sure that we don't forget, but I think I have that in my notes, too. Thanks, Anne.

DR. LANEY: I sent some in for the research recommendations also, relative to habitat.

DR. ERRIGO: Yes, and I have those, Wilson.

DR. DUMAS: On the bullet where it says there is new information suggesting the productivity of greater amberjack has changed, instead of changed, should we say either increased or decreased, to sort of make clear the direction of the change, in case people can't see the chain of causality between M has increased and age-at-maturity had decreased? I would recommend, in that bullet,

replace the word “changed”, and it occurs in two places, but replace the word “changed” with “increased or decreased”.

DR. SEDBERRY: Yes, it’s always better to be as specific as possible.

DR. DUMAS: Right, and then warranting a change in the PSA, and so do we want to say warranting an increase or a decrease in the PSA score?

DR. ERRIGO: When you go from a medium to a low, that’s a decrease in the score.

DR. DUMAS: That’s what I’m saying. If it’s not immediately obvious which way those things change, if you have to stop and think about it for a while, then, if you’re somebody who is not intimately familiar with the connection between these things, it might not be apparent.

DR. ERRIGO: Yes, because the actual numerical score goes down. Our score went from a two to a one, and so that was definitely a decrease.

DR. DUMAS: So M is natural mortality, right, and that increased, and so natural mortality increased, and so productivity of amberjack increased, and is that what we’re trying to say?

DR. ERRIGO: Yes.

DR. DUMAS: Okay.

DR. ERRIGO: Wilson, I just pasted your research recommendations in here, and I’ve just go to clean them up.

DR. LANEY: Thanks, Mike. I will just say, for the record, these were all derived from the discussions that we were having.

DR. SEDBERRY: Right. Thanks, Wilson.

DR. ERRIGO: Okay. I think we’re good.

DR. SEDBERRY: Then, finally, the sad story of the red porgy.

DR. ERRIGO: I assume we’re good with the top three?

DR. SEDBERRY: Yes, and those look good, Mike. I don’t see any hands raised. The uncertainties, risks, and consequences.

MR. ADDIS: Should we be more specific about how fisheries data has shown changes to age, size, maturity, and growth rate?

DR. SEDBERRY: Again, it’s always better to be specific.

DR. ERRIGO: Yes, we can, and it could be age and size-at-maturity first decrease, and then increase, and then I don't know how the growth rate changed. Marcel would have to comment on that.

DR. SEDBERRY: So you could say there's been fluctuations, or is that any better than changes? It's not changing in any particular direction.

DR. SERCHUK: I have a slight revision to the bullet above it. The largest effect is on projected catches and rebuilding status, and the fishing levels that are used in the projections are determined, and they are either status quo, F 85 percent, or F zero, and so the projected fishing levels are determined, but the catches are dependent upon the recruitment pattern, and is that not correct?

DR. ERRIGO: What I meant by fishing level is the fishing level recommendation, and I meant landings, but I will change that.

DR. SERCHUK: It's actually catches, because we have discards in there as well.

DR. ERRIGO: For this bullet here, I was hoping that maybe Marcel, during the writing of the report, could flesh it out a little bit more, since it was his group that did this work about exactly what changed, how it changed, and so forth.

DR. SEDBERRY: Well, they published some work over the years to show that size and age-at-maturity and sex change have changed, apparently in response to fishing pressure, but definitely that it has fluctuated as fishing pressure and management have changed. We've had a lot of discussion about fishing level recommendations, and I think we captured accurately what we finally settled on there. There needs to be a little bit of writing clarification here and there, but I think it's all there.

DR. ERRIGO: If no one has any heartburn with that, we --

DR. SEDBERRY: All right. Monitoring and research. We suggested a few things there. I don't see any hands raised.

DR. LANEY: I am not going to do it right now, but I will send in some habitat related ones, based on our discussions, and we will note, again, just so everybody else knows, and I will distribute it, but David Whitaker did provide a paper to me that documented the impact of red lionfish on red porgy, and so I will distribute that and share it with everybody else, and, again, I think that, based on our discussion, we can come up with some habitat-related research recommendations, and I will send those to Mike later.

DR. SEDBERRY: Thanks, Wilson.

DR. ERRIGO: We can take care of that later offline, the research recommendations.

MS. LANGE: So, Mike, you're going to be putting in the ones from the assessment?

DR. ERRIGO: From the assessment, yes.

MS. LANGE: All right. Good.

DR. ERRIGO: We did have one bullet here.

DR. SEDBERRY: Right, and, again, we can add a few words to that, referring back to some of the -- It's at least one instance above, and some others as well, where we refer to waiting for the ABC control rule revision before --

DR. ERRIGO: You also changed the MRAG for both king mackerel and greater amberjack, and so you deviated from the control rule.

DR. SEDBERRY: Right.

DR. ERRIGO: There was nothing in the control rule for red porgy. If everyone is good with that, I think that's it. We did it.

DR. SCHUELLER: I took some notes to expand on this, and I can do that, and I just had a question about this document, and I actually really like that it's this sort of collaborative editing mode, and are we basically expected to add our notes and stuff directly to this, as suggestions, and then you and Mike and Genny, as she's taking on the Chair role, can go through and edit that and accept, and is that how we're handling that this time, because, typically, we just email them in.

DR. SEDBERRY: Right, and I wasn't very clear about that, because I wasn't sure how well this was going to work when we first started this meeting, but it seems to me that it works pretty good, and the way the document is set up is that your permissions are only to add suggestions, and I don't believe that you can edit it, but you can add your edits as suggestions, and it's sort of like track changes in Word, and I think that works pretty well. For those that are comfortable with the Google Doc, if you would like to add your notes, or edits, to this Google Doc, and particularly the notes, I would say go ahead and do that. Mike, what do you think?

DR. ERRIGO: If you want to do that in Google Docs, you can. I will also send out the Word version, for those who are having issues with getting on Google Docs or don't have a Google account or what have you, and so I will also send out the Word version to everyone, so that they can get their edits in there and send them back.

DR. SEDBERRY: Those of you who are most comfortable just typing up notes, like you've always done, or writing them in a notebook, like you've always done, you can send those along to me and Mike, as you have always done. This is kind of the first time we've done this, and so we're just going to be completely flexible about it.

DR. NESSLAGE: George, we might want to -- For those who are going to edit on the Google Doc, you might want to give them a deadline, so that we know when to take it down off, when it's safe to take it down.

DR. SEDBERRY: Right, and so, when I was thinking about editing, it's really just to add the notes you have taken during this meeting. When we actually send out a draft for review, I would rather -- This is really going to be up to you, Genny, since you will be doing the final editing, but I would rather do that as a Word document that you would use track changes for.

DR. NESSLAGE: I agree completely.

DR. ERRIGO: I would also prefer that, because it is very difficult to keep the formatting where I need it to be in Google Docs. It doesn't translate perfectly between Google Docs and Word, and so, when we have a draft, it will be much better to do it by Word.

DR. NESSLAGE: That's what I am saying. We need to take it down off of Google Docs at some point and start doing everything in Word, but we need to know that everyone has put -- If they are going to put their notes in Google Docs format, we need to know when it's safe to do that, and so can we agree on a deadline for having our notes either sent or entered into Google Docs?

DR. SEDBERRY: Yes, and how about close of business on Monday? Would that be agreeable to everybody? Either have your notes in the Google Doc or sent in the traditional manner to Mike and me.

DR. SCHUELLER: Can we get another day?

DR. SEDBERRY: Okay. I don't know why I'm in such a big rush, and we actually have more time between now and the June meeting than we usually do, and so what do you think -- Genny, do you think that going until Wednesday of next week would be okay?

DR. NESSLAGE: I have never done this. You would know best.

DR. ERRIGO: We do have more time than we did last year.

DR. SEDBERRY: We do have more time. Sometimes it gets frantic, but this time it's not so frantic, and so let's just say -- How about noon on the 6th?

MS. LANGE: Then Mike will get us the Word, for those of us who would prefer to use the Word?

DR. ERRIGO: Yes. I can send a Word version out, and that's easy.

MS. LANGE: Okay, because I would rather do that than try and work with Google.

DR. SEDBERRY: Yes, and we're not going to be working with Google, but we're just going to be putting the notes in the Google Doc. When you get a version to edit, a draft version of the report to edit, it will be a Word document that you will edit in track changes, like we've traditionally done. All right?

DR. ERRIGO: Sounds good to me.

DR. SEDBERRY: Okay. Before we disconnect here, I, again, want to thank the council staff and the council members, our council liaison, and the presenters from the Southeast Fisheries Science Center and everybody that has participated, and this has been a long haul, and it was -- We weren't sure what to expect, and we had planned a busy, very busy, four-day meeting, and it ended up being an even busier three-day meeting, and very long days, and so I appreciate everybody's enthusiasm and stick-to-it-ness on this, and it has been really long, and I appreciate everybody's

input and all the work that you've done on this, but, again, the committee members and the council members, the council staff, NMFS staff, everybody, has just been a tremendous help on this.

Then I would like to, not only for this meeting, but for my two years as Chair, and I've had so many people help me out on this, and sometimes I -- I told Mel yesterday that I felt like I was sitting in the backseat of a car being driven by a student driver, and I was also the student driver, and all of you have helped me tremendously over the past years, and I appreciate the way you've worked together cooperatively to get these things done, and this can sometimes be grueling, and you've managed to hang in there and just deal with it and with me, and so I appreciate that a lot.

I also want to thank Genny and Jeff for stepping up to become Chair and Vice Chair, and I appreciate that more than you will ever know, Genny. I will buy you a beer sometime, whenever we can get together.

DR. NESSLAGE: That sounds great, and thank you for your service. You've been fantastic, George, and thank you. We are giving you a round of applause, virtually.

DR. SEDBERRY: Thanks. I see that Steve Poland has his hand raised, and so, before I disconnect us from each other, Steve, go ahead.

MR. POLAND: Thank you, George. I just wanted to, on behalf of the South Atlantic Council, thank everybody for meeting under these circumstances. Going through three assessments is grueling when it's in-person, and it's the added difficulties of doing this over a webinar, and I just wanted to thank everybody for sticking around for the last two-and-a-half days and having some really good discussions, and we really do appreciate it, and we appreciate the job that each and every one of you do.

George, I appreciate your service as Chair for the last two years. You're the only SSC Chair that I know, and so you will be the benchmark that I compare everyone else to, but, again, just thank you for your service, not only to the council, but to all the stakeholders, and, ultimately, all the citizens of the United States, for providing us the scientific information we need to manage their fisheries, and thanks to all the council staff and support staff and presenters for meeting under these circumstances, and, also, congratulations Genny and Jeff, and I look forward to working with you. Be safe.

DR. SEDBERRY: All right. Thanks, everybody, and we will be in touch.

MS. LANGE: Thanks, George. Congratulations, Genny and Jeff.

DR. NESSLAGE: Thank you, all. Good-bye.

(Whereupon, the meeting adjourned on April 30, 2020.)

Certified By: _____ Date: _____

Transcribed By:
Amanda Thomas
June 2, 2020

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Scientific & Statistical Committee

Attendee Report: Meeting (Day 1 of 3)

Report Generated:	
04/29/2020 07:39 AM EDT	
Webinar ID	Actual Start Date/Time
738-342-803	04/28/2020 12:56 PM EDT
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Addis	Dustin
Alvarado	Nicolas
BROUWER	MYRA
Barry	Rob
Bell	Mel
Bianchi	Alan
Bubley	Walter
Buckel	Jeff
Burgess	Erika
Burton	Michael
Byrd	Julia
Carr	Matthew
Cheshire	Rob
Cheuvront	Brian
DeVictor	Rick
Dingle	Julie
Dumas	Chris
Errigo	Michael
Finch	Margaret
Flowers	Jared
Foss	Kristin
GRAY DILEONE	ALISHA
Gamboa-Salazar	Keilin
Grimes	Shepherd
Grimes	Churchill
Griner	Tim
Guyas	Martha
Hadley	John
Harmon	Roger
Harrison	BeBe
Hart	Hannah
Helies	Frank
Hemilright	Dewey
Hiers	Homer
Horton	Chris
Howington	Kathleen
Hudson	Rusty

Iberle	Allie
Iverson	Kim
Jepson	Michael
Johnson	Eric
Klasnick	Kelly
LARKIN	Michael
Laney	Wilson
Lange	Anne
Lauretta	Matthew
Li	Yan
McCawley	Jessica
Mehta	Nikhil
Neer	Julie
Nesslage	Genny
Poland	Stephen
Pulver	Jeff
Ralston	Kellie
Reichert	Marcel
Rindone	Ryan
Roffer	Mitchell
Scharf	Fred
Schlick	CJ
Schueller	Amy
Sedberry	George
Serchuk	Fred
Sharov	Alexei
Sinkus	Wiley
Smart	Tracey
Smit-Brunello	Monica
Spurgin	Kali
Sweetman	CJ
Thompson	Ryan
Travis	Michael
Whitaker	David
Wiegand	Christina
Williams	Erik
Willis	Michelle
Wyanski	David
Yandle	Tracy
beckwith	anna
collier	chip
crabtree	roy
crosson	scott

Scientific & Statistical Committee

Attendee Report: Meeting (Day 2 of 3)

Report Generated:	
04/30/2020 07:38 AM EDT	
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913-852-379	04/29/2020 07:54 AM EDT
Attendee Details	
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Addis	Dustin
Allen	Shanae
Alvarado	Nicolas
BROUWER	MYRA
Bell	Mel
Bianchi	Alan
Bissette	Jesse
Bohlin	Bruce
Brown	Craig
Bubley	Walter
Buckel	Jeff
Burgess	Erika
Byrd	Julia
Byrd	Julia
Carmichael	John
Carr	Matthew
Cheshire	Rob
Cheuvront	Brian
Cox	Derek
Craig	Kevin
DeVictor	Rick
Dingle	Julie
Dumas	Chris
Errigo	Michael
Evans	Joseph
Finch	Margaret
Fitzpatrick	Eric
Flowers	Jared
Foss	Kristin
Gamboa-Salazar	Keilin
Glasgow	Dawn
Greene	Karen
Grimes	Churchill
Grimes	Shepherd
Griner	Tim

Guyas	Martha
Hadley	John
Hart	Hannah
Helies	Frank
Howington	Kathleen
Hudson	Rusty
Iberle	Allie
Iverson	Kim
Johnson	Eric
Klasnick	Kelly
LARKIN	Michael
Laks	Ira
Laney	Wilson
Lange	Anne
Lauretta	Matthew
Li	Yan
Long	Stephen
Lorenzen	Kai
McCawley	Jessica
McGovern	John
Mehta	Nikhil
Moss	david
Nee	Shannon
Neer	Julie
Nesslage	Genny
Poland	Stephen
Pulver	Jeff
Reichert	Marcel
Rhodes	Cameron
Rindone	Ryan
Salmon	Brandi
Scharf	Fred
Schirripa	Michael
Schlick	CJ
Schueller	Amy
Sedberry	George
Serchuk	Fred
Sharov	Alexei
Shertzer	Kyle
Siegfried	Katie
Sinkus	Wiley
Smart	Tracey
Spurgin	Kali
Sweetman	CJ
Whitaker	David
Wiegand	Christina
Williams	Erik

Willis	Michelle
Yandle	Tracy
beckwith	anna
burton	michael
collier	chip
crabtree	roy
crosson	scott
vara	mary
walter	john

Scientific & Statistical Committee

Attendee Report: Meeting (Day 3 of 3)

Report Generated:	
05/04/2020 07:48 AM EDT	
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Alvarado	Nicolas
BROUWER	MYRA
Bell	Mel
Bianchi	Alan
Bissette	Jesse
Bubley	Walter
Buckel	Jeff
Burgess	Erika
Byrd	Julia
Carmichael	John
Cheshire	Rob
Cheuvront	Brian
Craig	Kevin
DeVictor	Rick
Dumas	Chris
Errigo	Michael
Evans	Joseph
Finch	Margaret
Fitzpatrick	Eric
Flowers	Jared
Foss	Kristin
Gamboa-Salazar	Keilin
Glasgow	Dawn
Greene	Karen
Grimes	Churchill
Grimes	Shepherd
Griner	Tim
Guyas	Martha
Hadley	John
Harrison	BeBe
Hart	Hannah
Helies	Frank
Howington	Kathleen

Hudson	Rusty
Iberle	Allie
Iverson	Kim
Johnson	Eric
Klasnick	Kelly
Klibansky	Nikolai
Laney	Wilson
Lange	Anne
Li	Yan
Long	Stephen
Lorenzen	Kai
McGovern	John
Mehta	Nikhil
Nee	Shannon
Neer	Julie
Nesslage	Genny
Poland	Stephen
Pulver	Jeff
Reichert	Marcel
Salmon	Brandi
Scharf	Fred
Schlick	CJ
Schueller	Amy
Sedberry	George
Serchuk	Fred
Sharov	Alexei
Shertzer	Kyle
Siegfried	Katie
Sinkus	Wiley
Smart	Tracey
Spurgin	Kali
Sweetman	CJ
Thompson	Ryan
Travis	Michael
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