SOUTH ATLANTIC FISHERY MANAGEMENT COUNCIL

SCIENTIFIC AND STATISTICAL COMMITTEE

Town and Country Inn
Charleston, SC

May 3-5, 2016

SUMMARY MINUTES

SSC Committee

Dr. Luiz Barbieri, Chair	Dr. Marcel Reichert, Vice-Chair
Dr. Carolyn Belcher	Dr. John Boreman, Jr.
Dr. Jeff Buckel	Dr. Steve Cadrin
Dr. Scott Crosson	Dr. Churchill Grimes
Dr. Brian Irwin	Dr. Eric Johnson
Ann Lange	Dr. George Sedberry
Dr. Amy Schueller	Dr. Fred Serchuk
Dr. Alexei Sharov	Laura Lee
Dr. Tracy Yandle	

Council Members:

Dr. Michelle Duval, Chair	Zack Bowen
Mark Brown	Chris Conklin
Ben Hartig	

Council Staff:

John Carmichael	Dr. Brian Cheuvront
Myra Brouwer	Dr. Kari MacLauchlin
Julie O'Dell	Chip Collier
Amber Von Harten	Dr. Mike Errigo
Julia Byrd	

Observers/Participants:

Joey Ballenger	Dr. Erik Williams
Tracey Smart	Dr. Mike Larkin
John Hadley	Dr. Nick Farmer
Dr. Katie Siegfried	Genny Nesslage
Dr. David Records	Rusty Hudson

Other Attendees Attached

The Scientific and Statistical Committee of the South Atlantic Fishery Management Council convened at the Town and Country Inn, Charleston, South Carolina, May 3, 2016, and was called to order at 1:30 o'clock p.m. by Chairman Luiz Barbieri.

DR. BARBIERI: Good afternoon, everybody, and welcome to the May 2016 South Atlantic Council SSC meeting. We have a fairly full agenda for the next couple of days, a lot to discuss. Hopefully you have had a chance to review the items in our briefing book.

Before we get started with the first order of business, I just want to make a couple of minor announcements. One is that I need to remind you to say your names when you start speaking for the transcriber, so that he knows that you actually are you and has the proper notations there. Another thing is we have a few folks that haven't been able to make it yet. Anne was one that had considered potentially not be able to be here today, and so I'm glad to see that she was able to join us. Steve Cadrin is on the way. He got stranded in Providence, and he will be here mid-afternoon, he hopes, and we have Brian on the phone. Brian has something that came up at the last minute and prevented him from being here in person, and we discussed, given the urgency of the issue that he is tending to, to have him join us by webinar. With that, Mike, anything else, in terms of general announcements, before we get started?

DR. ERRIGO: Not really. I just wanted to let everyone know that I will be taking over for John, running the meeting here. We were able to talk Chip into taking the notes on the screen for us, and so, other than that, I think we're ready to go.

DR. BARBIERI: Excellent, and, getting started with our agenda, our first order of business is introductions, and I believe that we need to go around the room with general introductions, so we can help the transcriber actually identify the voices and associate them with the right names. We will start with you, Eric.

DR. JOHNSON: Eric Johnson, University of North Florida.

DR. BOREMAN: John Boreman, North Carolina State University.

MR. HARTIG: Ben Hartig, SSC liaison, South Atlantic Council.

DR. SEDBERRY: George Sedberry, NOAA Office of National Marine Sanctuaries.

DR. GRIMES: Churchill Grimes, South Atlantic SSC.

MR. COLLIER: Chip Collier, South Atlantic staff.

MR. ERRIGO: Mike Errigo, South Atlantic Council staff.

DR. BARBIERI: Luiz Barbieri, Florida Fish and Wildlife.

DR. REICHERT: Marcel Reichert, South Carolina Department of Natural Resources.

DR. BELCHER: Carolyn Belcher, Georgia Department of Natural Resources.

MS. LEE: Laura Lee, North Carolina Division of Marine Fisheries.

DR. SCHUELLER: Amy Schueller, Southeast Fisheries Science Center.

DR. SERCHUK: Fred Serchuk, SSC.

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

DR. BUCKEL: Jeff Buckel, N.C. State University.

MS. LANGE: Anne Lange, SSC.

DR. ERRIGO: We also have Brian on the webinar. Let me unmute him. Go ahead, Brian.

DR. IRWIN: Brian Irwin, Georgia Cooperative Fish and Wildlife Research Unit.

DR. BARBIERI: Thank you, all. The next item on our agenda is Approval of the Agenda. Any comments or suggestions?

DR. REICHERT: I just wanted to say that I noticed that the numbering of the agenda and the numbering of the overview is different. The gray triggerfish actually has its own number in the overview, I assume, and so if you -- Especially the second half of the year, at the SSC meeting, if you mention or make reference to a numbering, I just wanted to make sure that people noticed that that numbering was off by one.

DR. BARBIERI: Any other comments or questions or suggestions regarding approval of the agenda? If not, the agenda is approved, given the comments made by Marcel. Before we go to the Approval of the Minutes, I would like to also point out that Dr. Tracy Yandle was able to join us as well, and for your voice recognition -- Tracy and Scott Crosson. By the way, in terms of tardiness, the members of the SEP are somewhat excused, since you've been working double-duty and you were here this morning, in what I heard was a very productive SEP meeting, and so thank you for that and for being able to join us this afternoon. For the purposes of voice recognition, would you please state your name and introduce yourself?

DR. CROSSON: Scott Crosson, economist, NOAA Southeast Fisheries Science Center, and thank you, Mr. Chair.

DR. YANDLE: Tracy Yandle, Emory University.

DR. BARBIERI: Thank you. With that, we go to Approval of the Minutes from our last meeting. Any comments or questions or suggestions from the committee regarding the meeting minutes? Seeing none, those minutes are approved as presented. That leads us into Agenda Item Number 2, Public Comment. As you probably remember, we have two opportunities for public comment, one at the beginning of our meeting and then one at the end, on our last day. Any members of the public? I see Rusty. Rusty, if you could approach the table and introduce yourself for the administrative record.

MR. HUDSON: Thank you, Mr. Chairman and committee. Good afternoon. I am Rusty Hudson. The Southeastern Fisheries Association's East Coast Fisheries Section asked me to submit this oral comment to the South Atlantic Council Scientific and Statistical Committee. We hope you had the opportunity to review the East Coast's written comment that we submitted.

We have produced a post-SEDAR 41 red snapper analysis comparing the SERFS age structure versus the Florida Fish and Wildlife Commission's fishery-independent hook and line and the fishery-dependent age structure analysis of the mini-season landings. Dr. Peter Barile is here to answer any questions you may have on this analysis. We believe this new evidence suggests that the sole index of abundance during the 2010 to 2014 rebuilding period has mischaracterized the age structure in the SEDAR 41 red snapper stock assessment.

On the recent SEDAR 25 golden tilefish update assessment, we are concerned about the high level of uncertainty in the update. We encourage you to examine the four issues addressed in our East Coast written comment. As the only review panel of the SEDAR 25 golden tilefish assessment, your scrutiny of this update is extremely critical. Thank you.

DR. BARBIERI: Thank you for that, Rusty. Any other members of the public who are interested in making comment? Seeing none, we will close the initial period for public comment, with a reminder that on our last day that we're going to have another opportunity for the public to present comments to the committee.

DR. ERRIGO: Luiz, there is someone on the webinar who has their hand raised for public comment.

DR. BARBIERI: We don't take comments through the webinar. Just to state this on the microphone, for the administrative record, we do not take public comments through the webinar, only with folks who attend the meeting in person or by mail or email, if following the council's established deadlines for submission of public comments.

That leads us into Agenda Item Number 3, which is our update, our routine update, on landings and ACLs for the 2014/2015 year, and I believe that we have Dr. Mike Larkin from the Regional Office to give us that update. The action items for the committee to attend to are to review and comment, with attention towards any ABC recommendation updates.

You may remember that, a year or so ago, we requested to have this update brought to us, so we could evaluate any progress, or lack thereof, regarding catch level recommendations that are made by this committee and to monitor that performance. Emphasis should be placed on Level 4 and 5 stocks which have concerning landings trends as compared to the ABC values. Another item is consider assessment schedule and research plan implications, as you see the performance status of some of these stocks. With that, Mike Larkin, if you're ready.

DR. LARKIN: First off, and it's probably a mistake on my part, but I really focused in on the landings relative to the ACL. I'm making a note here that I need to focus more on ACL and ABC, but what I'm going to show you here is the landings for 2015 and 2016, because, back in October, I went through the 2014 and 2015 landings. Now I'm moving on to the next step, to 2015 and any available 2016 landings.

The commercial landings, and, first, I'm going to go through the commercial and then I will go through the recreational, but the landings were summarized on April 21, 2016, and the reason I put that specific date in is because, in 2014, the commercial landings switched from reporting bi-monthly to now reporting weekly, and so that's why I give that date. If I waited until the end of this week, I would have more updated 2016 landings, and so keep that in mind. In 2015 and 2016, the dealers have report their landings every week.

At this point, both 2015 and 2016 landings are still preliminary. They are not considered final. The landings came from in-season monitoring of the dealer-reported landings. Landings are compiled and provided by the Science Center in Miami. Landings are assigned by fisher-reported catch area, rather than dealer location.

I'm starting off here with the 2015 commercial landings. They're still preliminary. They're still not final at this point, and, right away, you can see -- I'm going to highlight just really the ones that exceed their ACL and had an accountability measure kick in. Right away, you see blueline tilefish at 400 percent of their ACL, and it was closed on April 7, but that one actually had a -- You can say it had a moving finish line there. Amendment 32 reduced the ACL from 112,000 down to 17,000. Hence, that's why the great overage there, and it closed on April 7 for blueline tilefish.

Then golden tilefish hook and line, that one closed on December 8. It looks like it went about 1 percent over the ACL, and the tray triggerfish, the split season, the July through -- I skipped those. Golden tilefish longline was closed on February 19, and the landings sum up to about 95 percent of the ACL. Then gray triggerfish, the January through June season was closed on May 8, and that one ended up having about 89 percent of the ACL. Gray triggerfish, July through December, that one went 33 percent over, but closed on September 8, but still had an overage there.

For jacks, it closed on June 23. It was 25 percent over the ACL there. You can see it was 125 percent. Then, if you go down to snowy grouper, the landings were actually about 9 percent over the ACL, and they closed on September 22.

On the vermilion snapper, this one I thought it was more detail to split it. First, the January through June 30, that one closed on April 15. It went to about 99 percent of the ACL, the total landings, and then the July to December 31, you see a 4 percent overage of the ACL, and that one closed on September 22. Yellowtail snapper, actually that one should be highlighted in yellow too, to make it stand out, but that one, you can see the 2 percent over the ACL, and that one closed on October 31.

These are -- Instead of the other ones that I showed you are a calendar year, January to December, except that they were only six months, but these have a different fishing year. Greater amberjack is going from May 1 to February 28, and black sea bass is going from June 1 to December 31. Those are the 2014/2015 seasons, and both of these, in this case, were below the ACL. Again, going through these different fishing years, and so I'm looking at the 2014/2015, king mackerel and Spanish mackerel are May 1 to February 28, and both of these were below the ACL.

Now, going into the 2016, what we have so far for 2016, and so moving on into more recent data here, you can see golden tilefish longline had a 2 percent overage, and that one closed on March 15. The gray triggerfish, January through June, that one was below the ACL, but that one closed

on April 2. The 2016 commercial landings, we can see, in this case here, that none of these exceeded their ACL, but we're still obviously in 2016, but you can see what we've got so far, up to April 21, the landings we have reported so far up to that date.

Then, to go into vermilion snapper, the January through June of 2016, that one was closed on March 29, and it was just 1 percent below exceeding the ACL. You can see the 99 percent there. Then the 2015/2016, for those different fishing years, for greater amberjack, it's March through February, and that one actually closed on January 21. It was at 98 percent of the ACL.

Black sea bass, just to keep it consistent with the earlier tables I had -- I know now it is a calendar year, but just to be consistent, you can see it's January 1 to December 31, but that one -- I'm sorry, but that should be. That's right. That's 2015. I need to update that with 2016 landings, but anyway, you can see, after that calendar year, only about 42 percent of the ACL.

The 2015/2016 commercial landings for the king mackerel, this was split even further, and so now there's the northern zone, which is from March 1 to -- All of these are March 1 to February 28. The Northern Zone is split up in -- Please, somebody there correct me if I'm wrong, but that's from North Carolina further north, all the way up to New York, and then the Southern Zone is defined from South Carolina down to East Florida, if I'm getting that correct. I think those zones are consistent for king and Spanish. In this case, you can see all these were below the ACL.

Moving on to the recreational landings, these landings were summarized in April of 2016 with all the available data we had as a week ago and as of -- I'm getting new data, and so as of today, as well. Landings for 2015 are still preliminary. We do not have any 2016 landings at this time. We're expecting Wave 1 of 2016, which is the January and February. We're waiting for that any day now, but I haven't received it as of yet today, but the Southeast Fisheries Science Center provides those landings.

Landings are summarized using either MRIP or MRFSS, which MRFSS nowadays is calibrated from MRIP, depending on how that ACL was defined. The landings also include not only MRFSS/MRIP, but as well as headboat landings. Landings were updated by NMFS to be consistent with the ACL monitoring. What I mean by that is post-stratification. For some of the stocks, we actually include Monroe County with the South Atlantic. If that is the case with how that ACL was defined, I would be consistent with that and do that here as well.

One thing I want to point out is that for most of the snapper grouper stocks that I'm about to show you here, in the past, what we would do is -- The way the accountability measure was set up, we would wait until the ACL had been exceeded and monitor it closely the next year. That following year, if we thought it was going to be exceeded or we predicted it to be exceeded or it was exceeded, we would close it. Now, as of February 2016, Amendment 34 changed that. Now, for all of these stocks, these snapper grouper stocks, we're going to monitor them in-season.

If suddenly we get new high landings in Wave 1 and so if we are to the ACL, we will close it right away, or we will monitor the landings. If we predict that they will exceed it, we will close it right away, and so I guess the point I'm trying to make is we'll be monitoring the stocks much more closely, the recreational landings much more closely, because the accountability measures have changed. We will do that much more closely in 2016. We don't want to go over and then act. We react right away.

Now the 2015 recreational landings. This has landings for all waves in 2015, but they're still not final yet. In blueline tilefish, we had a very large overage. Again, that one, in 2015, had a reduced ACL, similar to what I discussed before with the commercial. You can see when you reduce it down to 17,000 that you get a very high overage, and we closed that on June 10.

Cobia, New York to Georgia, we had real high landings this year. That one was actually 145 percent over the ACL, and so you see the total of 245,000 there. That one was an interesting one, for cobia New York to Georgia. That one, the accountability measure is still set up where you wait to see if it goes over and then we actually close it the next year based on when we predict landings will be met the following year. If you want, at the end, I can talk to that one in more detail, but, actually, that one will be closing on June 20 of 2016, that cobia New York to Georgia stock.

Then, to move on to the golden tilefish, 37 percent over the ACL. That one was closed on August 11. Hogfish, you see we're well over the ACL, 230 percent over the ACL, and that one was closed on August 24. Porgies, 3 percent over there. Some of these cases, like for porgies, it comes in and, by the time we finally do get the final landings, which is what happened with porgies, we said, okay, we've got landings for all six waves for the whole year of 2015, and then we sum it up and then we find out that we did exceed it. It was a small percentage, 3 percent over, but that happens in some cases. By the time we get all the landings, we find out that it went over.

Snowy grouper, that one actually closed on September 1, and the landings have actually been pretty low for 2015, but we predicted they would actually reach it on September 1, but, instead, the landings have been low, only 39 percent of the ACL. If you go to the next slide, now we're switching into the different fishing years instead of the calendar year. For king mackerel, in 2014/2015, you can see both king mackerel and Spanish mackerel are March 1 to February 28. In both these cases, they are below the ACL.

Now, the preliminary 2015/2016, and so, again, March 1 to February 28. In this case, we still have 2015 landings and we don't have any 2016 landings yet, but, in both cases, they're both below the ACL. For the greater amberjack and black sea bass, with their distinct fishing seasons, you can see both of these are both below the ACL for the 2014/2015 landings that we have so far. For the preliminary 2015/2016, again, none of these actually have 2016 landings yet, but anyway, in both cases, they have been, at this point, below the ACL. That's it, and so I would be happy to take any questions on these landings.

DR. BARBIERI: Thank you, Mike. Any questions or comments from the committee regarding the report?

DR. SERCHUK: Thank you for the presentation. I think it was well done. I'm a bit concerned. I know you flagged the stocks which exceeded the ACLs, which is a good thing, and, quite frankly, those often are accompanied by management actions that have either tried to limit it or constrain it, but I'm also concerned about the large number of stocks that are quite a bit below their ACLs, and I'm just wondering -- In some cases, maybe they're caught in mixed fisheries and so it's very hard to direct effort onto a particular species, and that may explain it, but, from a management point of view, really the goal is, in most cases, to try to have the performance near the ACL if the ACL correctly reflects the productivity of the stock.

I know that you focused on those ones that you've highlighted in yellow, where they were exceeded, but I'm also concerned about the ones particularly that are lower than 40 percent, perhaps. Is there a signal there that we should pay attention to? Thank you.

DR. LARKIN: I think a lot of that is just restricted by the data availability. I mean I will speak to the recreational first. We have a two-month wave, and then, in a perfect world, we would get those landings forty-five days after. They have not been on that time schedule, but let's say we get the landings sixty days afterwards. You're looking at you could have landings that were really high in January or February, but you wouldn't know really until May that you actually exceeded the ACL. That's the high end, but you're asking more about the low end.

Anyway, a lot of times, we can't wait until the wave actually is over. A lot of times, we'll use historical landings to predict. Wave 3 has been super high and so if we wait -- We will close in Wave 3, but, in reality, if we waited until we got the landings in Wave 3, we would be doing a closure in December, and it's way too late.

I guess a lot of it, with that issue, is just trying to make the assumption that historical landings will reflect future landings the way it's at now, because if we wait too late, we could be way over, but you know, in some cases, you can see we're wrong. Like snowy grouper, we thought that they would be much higher. Then, to our surprise, they were lower, but some of these landings are a lot more variable than others, like snowy grouper goes up and down and up and down, and some of them we can make much better predictions.

You see for the commercial, they're supposed to report weekly, but then you get late reporting. Then you've got -- Let's say it looks like we're really bumping up close to the ACL and we're waiting for the late reporting. John Doe, he reported 1,000 pounds of landings for this week last year, and so you make that assumption that that dealer is going to report it again, when, in reality, you may find out that that just didn't have any landings.

There is some error associated. That's why we try to do our best with what's the data that we have available and what are our assumptions of what we expect to happen, but, in reality, by the time you actually get the landings, or maybe the year already closed and you find out they were lower, and so I feel like we're much more accurate, because of the data availability coming in weekly with commercial, but, recreational, we have to get ahead of the game, and sometimes we're way under, but, if we wait too long, then we could have a huge overage. I don't know if that answers your question, but I hope so.

DR. SERCHUK: That answered part of my question. I think you focused on the differences in the data collection systems and how they come to bear on the values that you have for it, but, even on the commercial data, there are a number of species here which are below 50 percent for 2015. I guess let me rephrase my comment differently. Are there, other than the overages, are there signals in the landings database that we should direct our attention to with respect to either we have overestimated the productivity in the ACL or there has been an event in the fishery that's taken place that accounts for this very low landings relative to the ACL? That's the sort of insight that I'm looking for.

DR. LARKIN: We're always trying to isolate those signals to see if they're consistent over time or if they're variable, and then -- As you know, signals can change, especially as different closures

-- It's a very dynamic system. If you close Species A, they may direct all their focus towards Species B, or it's also driven by market price. Maybe the market price on Species A is so low that they're not going to fish for that this year. I do feel like we try to isolate and try to make predictions on these signals, but it's a dynamic system that we're trying to predict.

DR. BARBIERI: Thank you, Mike.

DR. REICHERT: I was going to point at, for instance, red grouper, which has been consistently under both commercial and recreational, and that's -- If I remember correctly, that's consistent with some other reports that they're just not there, and so that may be an example of something that we may need to pay attention to. The other thing may be that part of that is if we can see whether there's, for instance -- Correct me if I'm wrong, Fred, but if there's a consistent pattern over say the last couple of years, where the landings, commercially and/or recreationally, are consistently well under the ACL. That may be something we should pay attention. Isn't that what you're saying?

DR. SERCHUK: That's part of it. I'm concerned about the credibility. If we have an ACL out there and suggest you can take X, and people are only taking a quarter of X, there may be market reasons for that and there may be redirection of effort, but if the ACL suggests that that is the amount of resource that is available to be taken on an annual basis and the actual performance falls far short of that, I think it's incumbent upon us to ask the question of why, because it could be due to a number of reasons, and perhaps investigate it, particularly, as you mentioned, if it's a consistent pattern. Thank you.

DR. GRIMES: In the case of the king mackerel and the Spanish, or the king mackerel at least, my recollection is that, in the stock assessment, the stock went way up, and so the ACLs were increased, primarily due, I think, to the restructuring of how the stocks were allocated and that sort of thing, but I think that's a reason that it's just way below what the ACL was.

MR. HARTIG: I am heartened by this discussion, because the council has had these same discussions, and what I would like to see you all do is define what you want to see. From my perspective, it seems to me that when you set the ACL for any particular species, what was the trend in that catch over time and what was the fishery-independent information we have for each of those species?

That's the science, or the landings and the science, end of it, and then we could probably add some of the management that has occurred that may have affected catches. To me, if you define something here, I think it would be productive, going forward, for you all to look at these trends and then for us to pick up on that as well. I mean I think it would be very helpful to the council.

DR. BARBIERI: Thank you, Ben, and I think that here, in our overview document, when it asked us to pay attention to specifically Level 4 and 5 stocks, you may remember that we had this discussion at some point, that when we were making catch level recommendations based on ORCS or based on our decision rule, whether we were actually increasing -- By increasing the ACLs, we are increasing the landings for these species or whether we're just buffering for that variability that happens, but with no real impact on the growth exploitation level on those stocks.

Basically, the way I understood this action item, it's really first to look and reconcile the catch level recommendations that we are making here at a certain level and, as Fred pointing out, reflecting the productivity of different stocks, versus what is actually turning out, in terms of the actual landings. If we have to make any adjustments, if we see any red flags that could indicate that we miscalculated, that we overshot too high or too low, that this is an opportunity to -- It raises a red flag for us to be attentive to that issue.

If we are done with this item, I think this was actually a very good discussion relative to what we had last fall, and I think that it's something that perhaps we'll get ready, in terms of discussion for October, in better calibrating our catch level recommendations to where these things are actually being realized, in terms of landings. If there are no other comments or questions -- John.

DR. BOREMAN: I just want to put a word of caution in. The ABCs are set based on biological criteria, and the ACLs are the ABC minus some accountability for management uncertainty and other factors, markets or whatever, and so it's -- For me, I am comfortable looking at the ACLs and saying they're low, but to look back and say that must reflect on a poorly-chosen ABC, I think we've got to make sure that we separate out those two arguments, just that we're setting ABCs based on biological criteria that we see for the stock. If the catch is still continually low, then yes, we should go back and reexamine the ABC, but I wouldn't immediately damn the ABC if the fishery is not catching anywhere near the ACLs. Thank you.

DR. BARBIERI: Yes, and I think this is why the briefing book was basically telling us about those Level 4 and 5, where we really have to provide catch level recommendations based on landings-only information and we have poor biological -- For those, it becomes more relevant, and I agree with you on the other ones.

If there are no other questions or comments regarding this item, we will move to Agenda Item Number 4. We have our Southeast Reef Fish Survey Update. We are very thankful to the South Carolina DNR staff who have consistently come and given us an update on the SERFS survey. This is a non-decisional item, with no action items required. It's simply an informational update for the committee, so we can identify if there are any major changes in trends, in abundance, or any points for concern.

MR. BALLENGER: Hi, and I'm Joey Ballenger from the South Carolina Department of Natural Resources, and I will be going through the presentation today. I just, once again, wanted to thank you guys for the opportunity to update you guys with regards to our 2015 Southeast Reef Fish Survey sampling efforts and how some of those results look in regards to relative abundance trends for some snapper grouper species.

This presentation is going to be very similar to the one that Marcel has given the last couple of years. There is a lot of background information, and I'm going to go through that fairly quickly. If you have any questions and want some more information, please just let me know.

This is a reminder that the current Southeast Reef Fish Survey is composed of three separate entities: the MARMAP program, the SEAMAP South Atlantic Program, and the Southeast Fishery-Independent Survey. MARMAP is the historical fishery-independent survey, with SEAMAP South Atlantic and SEFIS contributing funds for reef fish surveys starting in 2009 and 2010, respectively. The survey primarily uses three research vessels, the R/V Palmetto, which is

used by MARMAP and SEAMAP, and the R/V Savannah and the NOAA Vessel Pisces, which is used primarily by SEFIS.

This year, I'm going to be talking about some of our relative abundance trends as observed using three primary monitoring gears, the chevron trap survey, which Marcel has talked about in previous years, as well as some data from our short bottom longline survey and long bottom longline survey.

Just a real quick reminder, the chevron trap is our primary workhorse monitoring gear for the Southeast Reef Fish Survey, and we're deploying that aboard the R/V Palmetto, the R/V Savannah, and the NOAA Vessel Pisces. We've been using consistent and standardized methodology to deploy this trap since 1990, and we're generally deploying it in depths up to approximately 110 meters.

As with all of our monitoring gears, we have a targeted soak time of approximately ninety-minutes, and we're baiting the chevron trap with clupeids. One of the things that when SEFIS came online that it allowed to do, it allowed us to add one to three video cameras on each trap, which has allowed us to get some more information regarding trap behavior, habitat type, and also develop some relative abundance trends based off of this video data directly.

The second monitoring gear that I wanted to remind you guys of is our short bottom longline. Unlike the chevron trap, only the MARMAP and SEAMAP programs are deploying this gear. Thus, it's being deployed exclusively from the R/V Palmetto, and we're targeting snowy grouper, jacks, tilefish, and some other deeper water snapper grouper species, and we're generally deploying this in high-relief habitat. This is a twenty-hook longline that's approximately twenty-four meters long, and we're baiting the hooks with whole squid.

Our final monitoring gear is our long bottom longline, which is deployed in depths of approximately 170 to 270 meters on mud-bottom habitat, or so-called tilefish grounds. Once again, it's targeting a soak time of ninety minutes. This longline gear has a mile-long ground line with 100 baited hooks, which are baited with whole squid. This is the only survey we're deploying from a vessel other than those first three that I mentioned. We're deploying this from the R/V Lady Lisa. It's a South Carolina DNR-owned research vessel, and our target species is primarily golden tilefish for this survey.

The survey, the fishery-independent survey, is designed to target potential habitat for snapper grouper species, with what's defining potential habitat depending on the monitoring gear in question. For the chevron trap universe, currently, we have a universe of approximately 4,000 sampling stations where we have verified live-bottom habitat, of which we're selecting approximately 1,500 to 2,000 stations each year, and these are the red dots you're seeing over here on the graph on the right. Each year, we're selecting 1,500 to 2,000 of these, ensuring that any one selected is greater than 200 meters apart from any other station selected in that gear.

Our short bottom longline survey universe is a little bit smaller. It has approximately 300 sampling stations, which, once again, is over known live-bottom habitat, in generally high-relief live-bottom habitat, and they are shown here in the blue in the graph on the right. Each year, we're selecting between 100 and 150 to sample randomly annually.

The survey is set up a little bit different, our long bottom longline. Instead of having individual stations, we have blocks of where we have identified fifteen blocks in tilefish habitat, where that habitat is generally mud bottom at appropriate depths. We target a minimum of two long bottom longlines deployed per block each year. Those are being shown in -- They're a little hard to show up, but they're the black squares, rectangles, you're seeing there on the continental shelf off of southern South Carolina and northern Georgia.

As I mentioned earlier, all three programs are deploying chevron traps. Generally, the MARMAP and SEAMAP South Atlantic is deploying chevron traps off the coast of South Carolina and North Carolina, while SEFIS is deploying chevron traps off of Georgia and Florida. That doesn't necessarily always hold though. Any other monitoring gears, our short bottom longline or long bottom longline, MARMAP and SEAMAP is doing all of the sampling with those gears.

For efficiency reasons, for data analysis, MARMAP and SEAMAP has been tasked with doing all the life history processing and analyses regarding life history information collected, while SEFIS is doing all the video processing and analysis related to the video collections. Because these are complementary surveys, we do have one combined database that is available online via seamap.org.

What did we actually do in 2015? We actually sampled slightly more than 1,500 chevron trap monitoring stations, 103 short bottom longline monitoring stations, forty-five long bottom longline stations, and we collected information from other gears on about 460 different collections, where other gears include CTD casts, water sample collections via Niskin Bottles, rod and reel collections, and various other gears.

The graph on the right is actually depicting the deployment, where we actually deployed our monitoring gears during 2015, with the blue dots being the SEFIS chevron traps, red dots being MARMAP chevron traps, pink/purple dots that show up as red being our long bottom longline, and then the orange being our short bottom longline, their deployments.

I will just go a little bit into what we actually observed in 2015. In total, we collected approximately slightly more than 43,000 individual fish for which we identified to species level and took measurements on, of which the top twenty species that I'm showing here represent approximately 99 percent of all fish collected. In total, of those 43,000-plus fish we collected, that represented eighty-three species that were collected via the monitoring gears.

I wanted to just highlight a couple of things. Approximately half of the species that fell in the top twenty are of commercial or economic importance. Those are showing up here in red. Just a note, black sea bass fell to the second-most common species we observed in 2015. It switched places with tomtate. Red snapper actually continued to move up on the list. It's now the number eight-most commonly-encountered species in our survey. Out of those 43,000-plus samples we measured and identified, we collected additional life history information on about slightly more than 11,000 individual fish. I'm not sure what's going on.

DR. BARBIERI: For those folks on the webinar, in case that's still live, just to let you know that we are having some technical difficulties here on this end, but we have a very capable crew here, technically capable, that is troubleshooting. We are taking a ten-minute break.

DR. BARBIERI: Before going back to Joey Ballenger, just to clarify to folks on the webinar, if you didn't hear when we first started talking about this, we had a technical issue here with the power line, and the webinar went out, and now we are back. Joey, if you can take it from where you stopped.

MR. BALLENGER: It looks like we're all back to the table once again, and so I will pick up where I left off a second ago. I already covered this information, but, basically, once again, we caught a little over 43,000 fish that we measured, representing eighty-three species. What I was getting ready to talk about is, of those, we collected additional life history information from a little over 11,000 individual specimens.

Here is just sort of the top fifteen species that we collected life history information from, and we actually collected life history information from a total of forty-one different species. We have our normal, usual players here. I did want to highlight that actually we're starting to see a few lionfish showing up in the chevron traps as well. We basically doubled our total lionfish catch in the history of the survey, from 1990 through 2013, in 2014 alone. It's only fourteen fish, but it's interesting that they're starting to show up a little bit.

Now I will go through and sort of talk about what we observed regarding catch per unit effort for some selected species from each of our primary monitoring gears. Just as a reminder, I'm going to present CPUE relative abundance indices from our chevron traps, our short bottom longlines, and our long bottom longlines. For each of these gears, I am including both monitoring stations plus any reconnaissance stations that eventually became monitoring stations in the first year that they were sampled.

For the chevron trap, this series covers 1990 through 2015, and I have an asterisk here because 1990, when we started that sampling station, was about eight to nine months after Hurricane Hugo came through the core of our sampling habitat at that time, and we see, for a few species, that they seem anomalous with regards to CPUE, with regards to neighboring gears.

For our short bottom longline and long bottom longline, we're using 1996 through 2015. To identify valid stations, we use species-specific depth ranges. Our response was catch per trap hour, and we're only going to present delta-GLM standardized relative abundance indices here, and the resulting index is actually being normalized to the long-term average. In all of our graphs, the error bars are going to represent 95 percent confidence intervals.

Just as some caveats, this is a summary overview and not an update of stock status, as you guys are all well aware of. There's a lot more information that goes into an actual stock assessment, including length composition, age composition, and other indices, landings data and so forth.

Further, the index shown here may differ from those that were used or developed for the actual stock assessment, SEDAR stock assessment, process. They may differ in regards to constraints and how the covariates are stratified, units, and so forth. Finally, many species have not been assessed or updated through the SEDAR process, and not all trends and analyses have been discussed in a SEDAR framework. Just to orient you, species in blue are those species we collect routine life history information from. Species in green, we currently do not routinely sample for life history. That will be in the titles of the graphs.

Going straight into results, the first one coming up is gray triggerfish. Once again, you can see the relative abundance trends shown here, and this is one of those that immediately you see that 1990 point, to where it shows that's being a bit anomalous compared to the 1991 and 1992 and so forth. This index was developed for the SEDAR 41 stock assessment, but the review panel also questioned that 1990 point, to some degree.

Second is greater amberjack, and this is the monitoring gear of short bottom longline survey that we had the best data for greater amberjack, but, as you can see, our abundance index is highly variable, and there's a lot of uncertainty about that, and so it may not be that informative to a stock assessment model.

Next up, we have tomtate. That should be the chevron trap. This is one we've shown for several years. They seem to have a decreasing trend through the mid-2000s and then, subsequently, we've seen generally an increase in abundance through the terminal year. Remember that this was the most abundant species we caught via the chevron trap in 2015, in absolute abundance.

Another member of the Haemulidae family, the white grunt, is shown here. This is one that we saw relatively low abundance from say the mid-2000s to the early 2010s, but, in the last couple of years, we've seen the relative abundance of this species increase in our survey.

Moving on the Lutjanidae, here is the relative abundance trend that I have for red snapper. As you will see during the SEDAR 41 stock assessment, it was determined to only use the survey from 2010 through 2014, but, to be consistent with previous years, we're showing the full time series index, based on the chevron trap catch here. This is similar to what we've shown in previous years, and it is of note that is fairly consistent with what the SEDAR 41 stock assessment model shows happened regarding relative abundance of red snapper historically.

Just because I sort of anticipated a question regarding what's driving this increase in relative abundance in the recent years, here is, based off of our 2015 life history samples, some age composition of the red snapper catch from the chevron trap survey. What I'm pointing out here is the 2005 through 2007 year class and the 2012 through 2013 year class are all year classes that the SEDAR 41 suggested were above average recruitment years.

You can see that all of those are still showing up quite prominently in our survey, and also I will just note this 2014 year class, the stock assessment couldn't say anything about it, but it's showing up as almost 30 percent of our catch in 2015, while the model suggested that the age ones were only selected at 7 percent, 0.65 selectivity for age ones in the survey.

Moving on, here is the survey for the relative abundance trend for vermilion snapper. It's generally been decreasing, but, once again, this is sort of one of those that we see that anomalous year in 1990, relative to the other years. Here is another one that I presented to show here that will be included in our trends report, but, in SEDAR 32, this index was not used in SEDAR 32 as a fishery-independent survey, mainly because of what you're seeing right here, the high uncertainty relative to the relative abundance of blueline tilefish catch over the short bottom longline.

Golden tilefish, as I mentioned earlier, the primary use of the long bottom longline survey -- It's one of the reasons we do this survey, is to target golden tilefish. 2015 was the first year we had

resumed this survey since 2011, due to funding issues. You notice there is the overall trend, but 2015 appeared to be quite low as far as relative abundance of golden tilefish.

Moving on to our serranids, first up is bank sea bass. You can see this one here, we saw sort of a spike in the early 2010s, but followed by a decrease of low relative abundance in the most recent terminal years. Here is the same thing shown for black sea bass. It's showing a spike in the relative abundance of black sea bass in 2011, followed by sort of a steady decline in relative abundance through the most recent terminal year, to where it's around the long-term average in 2015.

Here is the relative abundance of gag, based off the chevron trap survey. Once again, I would just highlight that there's a great deal of uncertainty regarding the annual estimates, and also it may be another case where the 1990 year may be a little anomalous compared to some other ones.

Here is our data regarding chevron trap catches of red grouper. This index was used in the most recent SEDAR stock assessment for red grouper, and you can see that it sort of peaked in relative abundance in the mid-2000s, early to mid-2000s, but, since then, we've seen a steep decline in relative abundance of red grouper. Somewhat analogously, we had -- Here is the relative abundance for scamp. Once again, it's showing relatively stable levels through the mid-2000s, followed by a decline in most recent years.

As I mentioned earlier, one of the target species of the short bottom longline survey is snowy grouper, and here is the relative abundance of snowy grouper in the short bottom longline survey. There is a fair amount of uncertainty here, but it does seem like there may be an overall trend as an increase in snowy grouper relative abundance.

Moving on to the porgies, starting with knobbed porgy, this is one that you all have seen for several years now, but, once again, I will just highlight that that 1990 value might be a little anomalously low. It could be because of Hurricane Hugo or possibly other reasons. We're not sure what's going on. Here is red porgy. I am just pausing to let you guys look at this for a second before I move on.

Then, finally, I think this is the last one I have. This is for stenotomus species. This is primarily scup, but we have a difficult time in the field identifying longspine porgy from scup, and so we include them as just stenotomus, but we have seen a general decline in recent years across the scup.

In 2016, our sampling season actually began today. I think the first SEFIS crews actually left Savannah, Georgia, a couple of hours ago. It will be going through the end of October or mid-October, October 14. Once again, the Southeast Reef Fish Survey will be using three monitoring vessels, the R/V Palmetto, R/V Savannah, and the NOAA Vessel Pisces.

That said, as you may or may not be aware of, the R/V Palmetto is undergoing extensive repairs this field season, and it still actually is in the yard at this point in time, but we have been able to contract out the NOAA Vessel, small research vessel, Sand Tiger to conduct some sampling during the month of May and June in this region.

All traps will have two video cameras, and there will be a CTD collection associated with a set of traps, and we will continue to do our hook and line sampling for diet studies, DNA sampling, and other ancillary projects. MARMAP and SEAMAP South Atlantic intends to continue doing our

short bottom longline survey and long bottom longline sampling, to collect that information on snowy grouper, blueline tilefish, and golden tilefish, among other species.

With that, I would just acknowledge Wally Bubley and Tracey Smart for their extensive involvement in conducting these analyses, MARMAP and SEAMAP and SEFIS staff and students, and our research special crews. As you're probably all aware of, these large-scale, fishery-independent surveys take a lot of people to pull off annually. With that, I will be happy to answer any questions.

DR. BARBIERI: Thank you so much for that update, Joey. It's always very informative to have you guys come over and give us the survey update. Any questions or comments from the committee?

DR. SHAROV: Joe, could you please give us an idea, if possible, how much of this information is being used -- Is most of it designed, or at least thought to be, or being directly used in the stock assessments for species that are of interest? Those that are not, what else are they used for? Then I have a second question, if I don't forget it.

MR. BALLENGER: I think one of the primary goals or mission statements of our survey is to provide long-term data regarding the relative abundance trends of snapper grouper species, and I think our data has been used in the assessment of greater than twenty-five species, via either the SEDAR stock assessment process or the ASMFC stock assessment process, and we've been involved in approximately forty different stock assessments since the advent of SEDAR in the early 2000s.

In addition, one of the primary other uses of this information is to provide life history information, particularly in regards to reproductive biology, for snapper grouper species in this region. For most species, we are the only lab providing reproductive biology data for snapper grouper species along the Atlantic coast.

DR. SHAROV: Thank you, and, since this is designed to be an index of change in relative abundance, and there are some species for which potentially you might have a saturation effect, or the gear saturation effect, that you have to deal with. I understand the challenge. The challenge is with the various catchability for different species and there may be long-term trends or some random changes in the catchability, but, nonetheless, have there been any attempts to maybe design sort of a total biomass index, to see whether the overall biomass is changing or there are long-term trends, et cetera?

MR. BALLENGER: I think there were a couple of questions in there. The first one, I think Nate Bacheler, who is one of the scientists on the SEFIS program, and some colleagues have looked at some of the questions regarding saturation for some of the different finfish species. I can't remember the actual citation off the top of my head, but that is one of the reasons we had a relatively short soak time of ninety minutes, compared to what you may see in the commercial trapping industry, for the most part. It's to try to limit the possibility of that saturation effect occurring.

The second question is we haven't looked at total abundance or total numbers or total biomass trends recently. I think there was some work at looking at that nominally historically, but I haven't

looked at that in the last few years, but it is something that we thought would be of interest, to see if the overall biomass or abundance is decreasing or increasing in the survey. That said, one of the things complicating it is some of the species we're talking about differ quite dramatically in regards to body size, and so if you looked at numbers versus biomass, you might see quite a different picture, in some cases.

DR. BARBIERI: Any other questions for Joey or comments regarding the survey? Seeing none, Joey, thanks again for coming over and giving us the update. It's very valuable to the committee. That puts us into Agenda Item Number 5, SEDAR Activities. You received three attachments, the SEDAR 50 project schedule, and that's for blueline tilefish, the draft SEDAR 50 terms of reference, and the draft red grouper terms of reference.

In terms of action items for the committee to address, determine whether the terms of reference for SEDAR 50, blueline tilefish, are sufficient as written or if modifications are recommended. Identify SSC representation for SEDAR 50, blueline tilefish.

Consider the red grouper update, you know, right now, red grouper is on the schedule for an update, but there is the possibility of having that handled as a standard assessment, and so the SEDAR program is requesting our recommendation on this topic. If we do recommend that a standard assessment be conducted instead of an update, then we need to revise the terms of reference accordingly. Then, finally, identify participants for the red grouper assessment. I think we have Julia Byrd to give us discussion of this item.

MS. BYRD: Hi, everyone. I'm Julia Byrd, and I'm one of the SEDAR Coordinators. As Luiz said, there are two kind of upcoming projects that you all have action items for. The first one that I wanted to walk through is SEDAR 50, which is an Atlantic blueline tilefish assessment. This is going to be a joint assessment between the Mid-Atlantic Council and the South Atlantic Council. SEDAR is kind of taking the lead to coordinate everything, and so the schedule is kind of up on the screen. It's also Attachment 4.

There's a stock ID workgroup meeting that's going to be held in advance of the data workshop, and that's going to be June 28 through 30 in Raleigh. There is going to be a data workshop at the end of October, October 24 through 28, here in Charleston. The assessment process is going to take place over a series of webinars from January through April of 2017, and then the review workshop is going to be May 23 through 25 of 2017. The location is kind of to be determined there.

We will be looking -- We already have SSC folks who volunteered to participate in the stock ID workgroup meeting, but we'll be looking for SSC participants for the data assessment and review workshops, but, before we get there, I think it will be good to go through the terms of reference first.

As far as the terms of reference go, there were two documents that were provided to you guys. The first one is 5a. These are the draft terms of reference. They were reviewed by the Southeast Science Center, and then we provided them to the Mid-Atlantic Council, and we're providing them to you guys for feedback. I'm not going to run through all of them. If you guys have specific questions or want to provide feedback on certain terms of reference, we can kind of go through those.

The other document I wanted to point out to you all is Attachment 5b. Terms of reference were provided to the Mid-Atlantic Council, and then they provided the following feedback. I guess what we're looking for is feedback from you guys on the terms of reference, and then we need to try to incorporate language to address the Mid-Atlantic Council's feedback, or, if you have questions or concerns about the Mid-Atlantic Council feedback, now would be the time to kind of bring those up, if that makes sense. I will leave the Mid-Atlantic Council's feedback up first, or I can go back and forth between the documents as you guys need me to.

DR. BARBIERI: With that, folks, if there are any comments from the committee regarding the terms of reference, as Julia pointed out, and then we can be integrating some responses or modifications to the existing terms of reference relative to this, if we deem appropriate.

DR. SCHUELLER: I will go first, I guess. I will state that I disagree with the Mid-Atlantic SSC's recommendations for changes on the terms of reference. Specifically, the terms of reference should not request a spatially-explicit model. They should request a model suited for the available data, as they are currently written by the council and SEDAR in their present form.

DR. BARBIERI: You guys captured that, right, because that was spot-on. That was good. Can you repeat that, if you wrote that down, Amy?

MS. BYRD: I guess maybe one thing I will say about that is I know both councils were interested in -- Depending on the outcome of the stock ID workshop, if the recommendation from that group is to have one stock, then I think both councils are still interested in managing the fisheries within their jurisdictions separately, and I'm going to pull up A5, Attachment A5, and this one that I'm highlighting here, this is Data Workshop Terms of Reference Number 1, and this Term of Reference Number 1 is what the stock ID workgroup is supposed to do, and it's 1c. It reads: Provide recommendations to address council management jurisdictions, to support management of the stock or stocks, and specification of management benchmarks and fishing levels, by council jurisdiction in a manner consistent with the productivity measures of the assessment. I think that kind of gets at that, maybe, without noting a spatially-explicit model, but I don't know.

MR. CARMICHAEL: Also, to direct to Assessment Workshop Term of Reference 2, if you want to highlight that, because that also addresses it. The discussions have been that, regardless of whatever comes out of the stock ID and the modeling efforts, there needs to be reference points that allow management by council jurisdiction, and I think that's the primary intent behind the first comment, and I suspect John Boreman could correct us if we're wrong about that, but that's my understanding, regardless of the issue about spatially-explicit a priori modeling decisions is valid, certainly. The term of reference about the type of models there, AW2, addresses that, but it gives that particular caveat about the management jurisdictions.

DR. BARBIERI: Just as a point of order, Fred, I know that you're in the queue, but since John Boreman is addressing that question directly, to that point.

DR. BOREMAN: These are not all SSC recommendations, first of all, from the Mid-Atlantic. These are the council recommendations. The council did ask the SSC for input. Only about two of them are actually from the SSC, and so the rest are related to management, as this one is, and I believe the tenor here is that if you can get a spatially-explicit model, it would make the management side -- It would make their work a little easier, in terms of how to come up with some

way of allocating catch or developing separate ABCs or whatever the councils want to do there, but I don't think it's an absolute requirement to have the spatially-explicit model. It's just that if the data can support spatially-explicit models, then it should be looked at. Thank you.

DR. BARBIERI: Thank you for that clarification, John.

DR. SERCHUK: I'm a little bit concerned about some either overlap or redundancy between the draft terms of reference for the data workshop and the draft terms of reference for the stock ID workshop. I want to make sure that, to the extent that we can provide the groups with explicit terms of reference, that they can answer them without having the next group not take cognizance of that.

What I'm hearing is the stock ID workshop should be looking at the discreetness, from a biological point of view, of genetics, and I think that's a first step. In most cases, that may be necessary, but not sufficient, to categorize management units. You can have sixteen stocks and one management unit. It really depends upon, from my perspective, how the discreet stocks will respond to a set of management measures.

If you have a minimum size, for example, and it will benefit all stocks, sometimes it's better to have one minimum size than to have sixteen of them. That has ramifications for enforcement, it has ramifications for data collection, it has ramifications for how you survey, and so on and so forth.

The message that seems to be coming out of this is we're going to have two management units, no matter what happens. That's a preference, and that is fine, and I think, if that is the case, then I think the genetics people ought to be looking at, given whatever conclusions they come to, what would be the impacts of having two management units with respect to the stock identities that are identified? Maybe they only identified one, maybe there will be two, or maybe there will be six. I don't know, but it seems a little bit odd to then talk about, at the stock ID workshop, talk about management units and then talk about it again at the data workshop. I would like to see a little bit more synchrony between the two, if that's possible, sort of a division of labor, so they're not redundant. Thank you.

DR. BARBIERI: Great suggestion. I see a lot of folks around the table nodding in agreement, Fred, and so very good suggestion.

MS. BYRD: I guess one thing I just wanted to mention was for the stock ID workgroup, they're responsible for Data Workshop Term of Reference Number 1. Their product, at the end, will be a working paper that will go to the data workshop. I'm assuming that the data workshop will go with the recommendations coming out of the workgroup.

The other thing I wanted to note is, for the stock ID workgroup, there are participants who are council staff and SERO and GARFO staff. Those people typically are observers in the general SEDAR process. They're not kind of panel members, but, in this case, they are, to try to address this 1c here dealing with the management jurisdictions. Those folks were brought into the process to help address that. John, I don't know if you want to say anything else about that.

MR. CARMICHAEL: Yes, I think that's right, and I think Fred's comment about going in that we know we have two management units and approach it so then what does that mean to the stock perspective. I think that's a helpful way of looking at it, and it may help with some of that synchrony between that workshop and then the data workshop, dealing with this term of reference.

DR. REICHERT: That was a question of mine, if we should make that a specific term of reference, what is the impact of two management units?

MR. CARMICHAEL: I think we can give that as guidance. I mean it says in there that -- It basically states there on c, under the stock ID, that there is two management units, and so we can provide that, I think, without having to go in and edit any of these.

DR. REICHERT: Okay. I'm comfortable with that.

DR. BARBIERI: Any other comments or questions?

MS. BYRD: Going back to some of the Mid-Atlantic Council's comments, I guess the first three I think our discussion so far has covered, but Number 4 and Number 5 are kind of ecosystem considerations and issue of climate change. They are suggesting adding terms of reference for those, and so it would be good to get kind of feedback from you guys on that.

I know it would be helpful to maybe put some language together. I know the Gulf of Mexico Council normally includes kind of ecosystem consideration language in their terms of reference. That's one place we can use language from, or, John, I'm not sure in the Mid-Atlantic if that's typically included in the SAW/SARC terms of reference as well. That's another place we could grab language.

DR. BOREMAN: Yes, Number 4 is -- Our SSC put that in there, Dr. Hood, our resident ecosystem expert, but, typically, we request this in all of our terms of reference for the Northeast now. To us, it's become boilerplate, and so you can look at the standard terms of reference for the Northeast and SAW, and it's just there is a push now in the Mid-Atlantic to move towards ecosystem-based approaches to fisheries management. We're trying to get into that frame of mind, and this is one way to help us.

What we do is we look at food habits and oceanographic conditions, all stuff related to why the fish are there, and Number 5 is a similar topic. We've been looking -- We, the council, has been investigating the climate change effects, working with the Northeast Center, Jon Hare and company. We are seeing some pretty dramatic effects of climate change now in the Mid-Atlantic. It's a transition region. Fish are moving through, but definite distributional shifts that probably could be attributed to climate change, even though there is alternate hypotheses. Fluke is one, and black sea bass. Now they're seeing black sea bass in the Gulf of Maine, for example.

This is a hypothesis that I will mention later when we talk about blueline tilefish. It's one hypothesis that we're looking at. Is the increase in landings we're seeing in the Mid-Atlantic due to a distributional shift, or is it just a shift in the fishery, or is it just the fishery catching fish that haven't been exploited as heavily in the past? This is the question too, and so that's why so the climate change issue is of interest.

DR. BARBIERI: Thank you for that, John. Good clarification there.

MR. CARMICHAEL: I think what we would need is someone here on the SSC to suggest a term of reference, perhaps, to address these, if it's going to get its way in there. They didn't suggest a term of reference, just that this should be in there somewhere, and so if someone here has an idea of where it could be added and how it could be added, we could get moving on it.

DR. BARBIERI: If we don't right now, one other alternative is we can get back to this and give you some time to think about this, or, during the development of our report, we can actually have a bit more time to provide some suggestions on how to develop that term of reference there that would incorporate the ecosystem components.

MS. BYRD: I can look through a Northeast assessment to get the draft terms of reference for ecosystem considerations and provide the one from the Gulf for you all to look at as well.

DR. BARBIERI: Yes, and that will be a great help. Thank you, Julia.

MS. BYRD: That was all I had on blueline tilefish, and next we're going to -- Sorry, Fred. Go ahead.

DR. SERCHUK: I just have a question. Maybe this is standard practice, but it talks about there will be a number of assessment workshops that will be done by a series of webinars. My initial gut reaction to that is this is a very ineffective way of doing assessment materials. Typically, assessment workshops are synergistic. That is, you will do something and then someone will come up and do something else, and I'm just wondering about the logic behind this, particularly because most of the information that I've seen, in terms of the assessment terms of reference, relate to data sources and how we're going to compile stuff and whether something is sufficient and spatial stuff.

That seems, to me -- Normally, I would expect that if you needed seven days or ten days to do it, you could convene a working group for that long, to try to go through all of that stuff in a discreet fashion, utilizing all the expertise in the meeting. I'm just surprised to see this as a series of webinars. Thank you.

MR. GRIMES: Relative to blueline tilefish, what exactly is the status of the Mid-Atlantic Council's action on that? Are they wanting to combine that with golden tilefish for their management plan, and that was sent forward for secretarial approval or something like that? Does somebody know? Was it approved? What's the status of that? John probably knows.

DR. BARBIERI: Actually, Church, if you don't mind, since Fred asked like a direct question, if we could address that one first, in terms of the webinars.

MS. BYRD: I guess what I will say is that SEDAR has been moving towards trying to do the assessment kind of stage process via webinars, one, to kind of save money, I guess, and also, in some ways, I guess to kind of streamline the process a little bit. I know, for the SEDAR 41 assessment we just did, we did a combination of assessment workshops and webinars, and so if you guys feel strongly that it a workshop would be more productive, then you can let us know that, and we can pass that kind of recommendation on. I'm not sure how directly that answers your

question, but that's kind of the way we have been moving towards, but if you feel like it would be important to have a workshop for SEDAR 50, please let us know.

DR. SERCHUK: My recommendation would be, if at all possible, I would urge you to do it as a meeting or two meetings. I am very familiar with the ICES setup over in Europe, and generally they have working groups that will meet for five or six or seven or eight or ten days, depending on the suite of terms of reference.

I am also very familiar with the process that was used up in the Northeast, and often they will have assessment workshops that they can't get it all done and they will schedule another one, but I've never heard of the webinars, and I'm a little bit leery of the webinars, because people can listen, but not do the work, and so on and so forth. I would strongly urge, if at all possible -- I think it would be cost efficient, even though it looks like it will be more expensive. I think you will come up with a better product. Thank you.

DR. BARBIERI: Thank you, and this has generated a lot of interest, and so I have Marcel and Scott already in the queue.

DR. REICHERT: Very briefly, I completely agree with you, and I would support that.

DR. CROSSON: I actually thought -- Marcel, what was the SEDAR for Spanish mackerel that you and I were recently the SSC representatives on? Wasn't that done through a series of webinars for the assessment?

DR. REICHERT: The king mackerel?

DR. CROSSON: No, it was Spanish mackerel. You and I were the SSC representatives on the Spanish mackerel assessment.

DR. REICHERT: I think we had a workshop, if I remember correctly.

DR. CROSSON: Did we have a workshop, or did we do webinars for that? I might be mistaken.

DR. REICHERT: Maybe I am mixing king mackerel and Spanish mackerel.

DR. CROSSON: One of these, we did a series of webinars, and I thought it was quite efficient, actually, to the fact that you didn't have to keep stopping and waiting for somebody to go through and do more analysis and waste a lot of folks' time, and so I did not find it to be disruptive to the process, having been part of this same species stock assessment the time before.

DR. BELCHER: A question to that point. Was it a benchmark or was it a standard? What type of assessment was it, because, obviously, as we've been talking off mic about it, the benchmark, to me, seems like you would want more face time, given the amount of discussion that should go into, versus an update or a standard, which is kind of more a turn-of-the-crank approach.

MR. CARMICHAEL: A standard can have a workshop or a webinar. Often, they will have a workshop, because there is just one and it's a review of the data and discussion of the model. The process in SEDAR for the benchmarks is there can be workshops. We did a workshop with red

snapper recently, but we've been moving to this more flexible process, where the analysts are more free to go do the work and explore the models and reach decision points and vet those over a webinar to have the public transparent discussion.

Going way back, we started, obviously, with everything under workshops. There was one discussion, at one point, when we realized how hard it is to get all of that stuff done in one week, that you do try things and see how they work out and come back to them. At one point, the idea was that we should really have a two-workshop assessment process, much as Fred described. You get in and you get the model functioning, you start to explore it, you get back together and refine it, but, really, it was economics and peoples' time that kept us from doing that, even though I it -- Back, at least ten years ago, people thought that was really one of the ideal ways to go about it. Now, dealing with further economic realities and peoples' time, we have fallen back into not even having the assessment workshop as the default.

DR. CROSSON: I completely agree, John, that you should always be following the economics, but I think part of it was -- I think a key part of it also is that I've seen with SEDARs before where they run out of time at the end of a week and they end up punting it to the next stage of the SEDAR. Then it gets to the review panel and you have all of these unanswered questions, and it causes problems down the line, and so that's another, to my mind, a good vote for hopefully we can reduce that, and we can do the webinars instead.

DR. SERCHUK: Just one final comment. I am concerned of the nuances in terms of, no matter what you do, we want it in two management units. When you start looking at the data and what's available for simple models of biological productivity, whether it's yield per recruit or whether it's an analytical model, and then you've got to say, wait a second, our stock traverses a line here. You're really talking about some thinking about how to go from a biological unit, and I'm talking about -- When I say biological unit, I'm talking about a productivity unit.

Then, on top of that, you might have to address John's issue of you have a productivity unit, but part of it is now traversing northward, because of climate change. There are all sorts of nuances here, and I don't think it's the sort of thing that you can say, well, this group go back home and this group go back home and come back with the answer. I just think there are so many kinks here that are coming forward that I think group route would be the best way of going. That's just my opinion.

DR. BARBIERI: I think that's an excellent point, and, by the way, one of the reasons that this is one of our agenda items is because the SEDAR program really welcomes this feedback, and they don't handle this as a one-size-fits-all. If the committee feels strongly about some assessments that should be handled in a specific way, let's make sure that we articulate this well in our report. I think that this commentary today has been very helpful to Julia and John and the rest of the SEDAR staff in terms of letting them know how the committee feels regarding the nature of this particular assessment.

MR. CARMICHAEL: Yes, and if you could be explicit and perhaps suggest -- The webinars are laid out with the three and the specific milestones. If you really believe that in one of those to replace that with an in-person meeting, that would certainly go a long way toward us doing that. Perhaps if anyone from the Science Center wants to comment on that, that would be appreciated and appropriate at this time, too.

DR. BARBIERI: Keep in mind that we have a report to the council in June, at the June council meeting, and there is a SEDAR Committee the council has that will have the opportunity to weigh in on these things and most likely support our recommendation here and work with the Center and the SEDAR program in facilitating what recommendations have been made.

MR. CARMICHAEL: Do we think maybe at Milestone 2, sort of in the middle there, or do you think it would be better near the end? I think maybe at 2 that you're sort of in the meat of it and you should have things functioning at that point. That might be where you really have to come to grips with the various lines and how you're going to craft this management unit advice from whatever the stock is.

DR. BARBIERI: Any thoughts on that, on where within the series of milestones there that we would place the in-person workshops? Let the record show that Dr. Erik Williams from the Science Center will weigh in.

DR. WILLIAMS: Thank you for the opportunity to weigh in, and I agree with the comments that are being suggested, both Fred's suggestion for in-person workshops and the economic considerations that have to be taken into account, but, if we're going to do in-person workshops, I think there is -- When we look at the situation with blueline, it is unique, because we are dealing with this jurisdictional issue, and because we're dealing with these other factors, like potential spatial models and potential climate change factors to consider.

I think maybe a general logical path to take is look at how we would get to a final product while considering all the possible options available to us, and so, in that sense, there is probably a logical way of addressing this, in the sense that maybe some webinars could be done at first to look at all the available data and what models might be appropriate for that data, without actually fitting them, just sort of in a theoretical here is the data and here is models that might work with that data.

Then Webinar or Workshop 1, In-Person Workshop 1, would actually come to the workshop with some of those data fit, using some of those techniques that were decided upon, look at the results of that, look at how the models are behaving with that particular data, and then Workshop 2 would be to then assimilate all of that and come up with a final recommendation for a set of models or a model that would then be put forward for management. If we kind of go along those lines, maybe that's a logical way to break the two in-person workshops.

DR. BARBIERI: Thank you for that, Erik.

DR. REICHERT: If we would have, time-wise or funding-wise, only an opportunity for one workshop, which one would you consider to be most effective as an in-person meeting?

DR. WILLIAMS: That's a good question. I would probably say the first one that I described, where we have some initial fits with a very wide range of model types, because that's the discussion. That's the point where the synergy that Fred was describing might occur, where people are going to look at the performance of these different models and come to some agreement of which ones are working and which ones are not working and which ones are addressing the issues and which ones are not.

DR. BARBIERI: Thank you for that, Erik, because now we have a suggestion there on the table on where to put those things. Any additional thoughts or comments from the committee? I mean, basically, as Erik was describing that idea, I saw most people around the table sort of nodding in agreement, and so, if no disagreement with that, I think we start looking at that as a possibility. Then Churchill had that question, which I think is a fundamental question in terms of understanding the --

DR. GRIMES: I forgot what I asked now. From the conversation, it sounds like it's a fait accompli that there will be separate management units, and so, if they modify their tilefish management plan to include blueline tilefish -- Somebody sent around a news release from the Mid-Atlantic Council that said that they had sent that forward for secretarial approval, and so it has been approved by the Secretary or by the Regional Administrator?

MR. CARMICHAEL: Submitted by the council, but not approved by the Secretary yet, is my understanding. They had a whole suite of management actions addressing permitting and reporting and the regulations, as well as where they would put it within their management program. Approved by the council a few weeks ago, I guess at their last meeting, and submitted to the Secretary, I would assume, by now, and so it's up there being processed and reviewed.

DR. GRIMES: In fact, that's actually -- It will go to the Regional Administrator in the Northeast, I presume.

DR. BARBIERI: John Boreman might have more details on that.

DR. BOREMAN: Currently, the blueline tilefish fishery in the Mid-Atlantic region is being run under an emergency action that the council requested of the Secretary last fall. That emergency action expires in June, and so the purpose of the action taken at the last council meeting was to develop regulations to go into place in June, to pick up where the emergency action left off, and that's why the SSC was requested to come up with an ABC that they can use to base their regulations on. Right now, I think it's part of the golden tilefish plan. It's an amendment to the plan, but I don't think it's a plan amendment, because that would take a few years. I think it's just a framework action or something.

DR. BARBIERI: Thank you for that clarification, John. That was helpful.

DR. IRWIN: I was just going through and I had a comment to Erik's thoughts on the timing of the in-person workshops, unless we've already moved on too far from that.

MS. BYRD: Go ahead.

DR. IRWIN: I was just going to chime in with agreement on the value of the in-person workshops and then make a suggestion for the timing. I like to think about alternative models as essentially representing different hypotheses, which we want to then confront with the available data. In my experience, working on these types of things in inland waters, the most valuable time to meet in person is when the data are in hand and some preliminary models are available, or at least some potential model structure has been identified. I kind of view the best timing as sort of being about a third of the way through the process, just really roughly speaking. Thanks. That's all.

DR. BARBIERI: Thank you so much for that, Brian. Very good point.

MS. BYRD: I almost switched away from blueline tilefish before I asked if there are any volunteers from the SSC to participate in the data, the assessment, and the review workshop stages. Before moving on to red grouper, I wanted to ask for volunteers for first the data workshop, which is going to be October 24 through 28 of this year, and it's going to be in Charleston.

DR. REICHERT: Since I'm in Charleston, I don't mind attending the data workshop.

MS. BYRD: Great. Thanks, Marcel. Anyone else? If folks need to look at calendars or schedules and kind of get back to me, I will be around the whole meeting, and so people are welcome to let me know throughout the meeting, too. Next, will be assessment. It sounds like -- Go ahead, Jeff.

DR. BUCKEL: I had one comment on the TOR for blueline tilefish. Sorry that I didn't jump in when we were doing that before. This is for the stock structure workshop. Church and George, I think you talked at a previous meeting about ROV data in the Mid-Atlantic, where you had counts of blueline tilefish, or the lack of counts, and so that's not explicitly listed here. It would be nice to see if you had the latitudes and longitudes of those observations and then if there's current fishery-independent or fishery-dependent data from those same latitudes and longitudes, just to answer some of this question about if it's movement into the Mid-Atlantic or not, to help with that climate change movement or if they were always there. It does say look at all blueline tilefish information, but just to explicitly have something in there, if Church and George confirm that that's what I remember correctly.

DR. SEDBERRY: I mean I think I've mentioned previously that I was involved in a project off of Virginia for many years, and we didn't see any blueline tilefish, in spite of using numerous methods to sample the right habitat. We saw one, and I think Church had mentioned possibly seeing some blueline tilefish during golden tilefish submersible work off of New Jersey. Does that sound familiar?

DR. GRIMES: No, I don't think I said that. Actually, I do remember seeing a paper that was submitted to a journal that was reporting observations of blueline tilefish off of Virginia.

DR. SEDBERRY: Yes, that's Steve Ross's paper.

DR. GRIMES: Steve Ross's paper, exactly.

DR. SEDBERRY: That just came out late last year, I think, or early this year. That's the one I sent to you.

DR. SHAROV: I am not volunteering at this moment.

DR. BARBIERI: Too late for that. I'm sorry, Alexei.

DR. SHAROV: I just wanted to clarify, on our decisions on terms of reference. I heard you say earlier that we will work it through either our later discussion or by volunteering somebody to write a proposal, but it is my understanding that we are the ones to finalize the TORs. Is that

correct? If so, I was not clear as to whether we were all in the same position on specifically incorporation of the Mid-Atlantic Council's suggestions on TORs.

Clearly, the most important one is a dual management and the need to consider two management regions, regardless of whether there will be two stocks identified or one stock identified and then the need of separate management areas. When are we going to, and are we going to, make a clear statement of what terms of reference should be in there, because, from my perspective, it's the most important contribution that we as SSC members could provide here. Thanks.

DR. BARBIERI: Just to answer, and then I think I'm going to ask Julia and/or John to jump in, but I think that, in this case, basically it's a joint assessment that's being conducted between the Mid-Atlantic and the South Atlantic, and so it's an opportunity to kind of combine terms of reference that address science needs for management advice in both areas, and that council has requested that unit up there, or even if it isn't a unit, but that portion of the stock be managed separately, which would be to generate some kind of reference point or some other way to manage up there.

MR. CARMICHAEL: Yes, that's correct. It's being handled as a joint, much as we do joint with the Gulf. The South Atlantic, because this is running through SEDAR, has the slight lead, and so we provided them the terms of reference and they reviewed them. The leadership of both councils have agreed, in consultation with the agency and the leadership there, that there needs to be separate management units, and so we at least know that going in. I think you all's suggestion here on the terms of reference, and, to the extent that you can incorporate the suggestions from the Mid-Atlantic, then these will be officially approved by the South Atlantic Council at their next meeting in June. Then we'll be along the way. I think we addressed all of the question.

DR. SHAROV: So then, eventually, somebody would have to derive the one single list of terms of reference that would be satisfactory to both councils. As I understand, at this point, we just simply have to provide our opinions on terms of reference that are currently being offered on both sides, correct?

MR. CARMICHAEL: Yes, and we think that the Mid-Atlantic comments 1 and 3 are certainly well addressed. 2 is generally implied within our process, and certainly within the assessment workshop term of reference about evaluating the data and what's appropriate. 4 and 5 are the ones that aren't explicitly included at all in what we have now, and my expectation was, before this meeting is over, there will be a recommendation from this group about how those could be folded in, whether it's one term of reference or two terms of reference, but somewhere within the data and assessment components, to look at both the ecosystem considerations and consider this issue of climate change.

Once you guys have done that, that's part of your report and your recommended changes to these terms of reference. That goes to the South Atlantic Council, presuming they approve it, and then we ship it off. Julia ships it off to everybody, saying these are the final ones.

DR. BARBIERI: Is that clear, Alexei?

DR. SHAROV: Yes, I guess so. I just wanted to sort of hear where we're converging to, and so I think clearly the terms should be all-inclusive, that they should be inclusive of all the principle

interests of all the parties involved, and I think that clearly could be done, but the rest should be left to -- They should be inclusive and, at the same time, flexible enough for the data people and the assessment people to make an appropriate decision that is driven by science and not by only management needs as to what is the best approach to the assessment of the stock. Thank you.

DR. BARBIERI: Thank you, Alexei.

DR. SERCHUK: I'm a little bit concerned about the SSC/Council Term of Reference Number 2, and the reason I'm concerned about it is it's sort of -- I get the feeling that it prejudges what's going to come out of the process. If I'm wrong, I'm sorry to bring this up, but I get the feeling that if the data sources say that we ought to do an analytical assessment and we can do it in the southern part and we believe that the animals above the Virginia/North Carolina border are part of the same population, and so we would like to include them in the analytical assessment, what we're saying -- What this says to me, it says do not go there, do not go there.

If you have animals that are north of Virginia and you can't do an analytical assessment, for the group north, then we don't want to include it in any analytical assessment, and I hope that is wrong, because the charge to the scientists is to utilize the data sources that you have to best model the stocks as you see them.

I get the feeling, what little I know about blueline tilefish, is the data are probably sparse north of Virginia/North Carolina. They are probably limited to CPUE data, and that the data are collected, because if the fleet moves around, and so there is no background over time in all areas. You're likely to get a high-density area here, and the next year it's over here, but you haven't really done a full suite of sampling.

If I have misread this, I apologize, but I'm a little bit concerned that this overarching issue of how to manage the stocks is now being cast down to how we want you to analyze the information, and if we can't do an analytical assessment including the animals -- If you can't do an analytical assessment north of the Virginia/North Carolina border, don't include them in any assessment and we will do a data-poor method, and I don't think that's the right approach. I feel very uncomfortable, as a scientist, going into it that way.

Actually, I feel very uncomfortable about trying to have different management units coming out, from a management point of view. It seems, to me, if you had one stock, but you had overlapping management jurisdictions, the management body should be able to get together and say, how are we going to move forward on this? It seems more of a management question, but my concern is not that. It's a science issue that looks to be like the scientists are being handcuffed, in a certain way. Pardon me, but that's the way I read it.

DR. BARBIERI: I think those are very good points, and actually, incidentally, Church and I were talking about this before the meeting started.

DR. GRIMES: I support what he says 100 percent. I totally agree with him.

DR. BARBIERI: Yes, and we were discussing these very points, Fred, because, looking through this, we were puzzled by how prescriptive this is, coming from -- It's almost a backwards sort of

-- It's management advice to science, instead of scientific advice to management, but I guess it is what it is.

From my perspective, in terms of our report to the council, I think it's a matter of capturing those points, Fred, specifically, and making -- Because the council is expecting this type of feedback, and this is why this is being put before us, to provide comments, and so let's make sure that we capture these type of comments in our report, and we can advise the council accordingly. Julia, does this complete --

MS. BYRD: Not yet. I'm sorry, but I really appreciate all the discussion and feedback on terms of reference. It's very helpful to have you all's input. Before I move on from blueline, I wanted to see if there any volunteers for either the assessment, and it sounds like it's going to be maybe a mix of a workshop and some webinars. It will probably be in 2017, January to April. I don't know the exact dates of when a workshop would be held, but if there's anyone interested in participating in the assessment stage, that would be good to know now, or you can look at your calendars later and let me know. Then, also, we will need volunteers for the review. We will need a review panel chair and then also reviewers, and so, Laura, would you prefer to be a chair or a reviewer?

MS. LEE: A reviewer.

MS. BYRD: It is May 23 through 25 of 2017 is the review.

DR. SERCHUK: The assessment participants, will that be a CIE review as well?

MS. BYRD: We typically will have three CIE reviewers, and then, in this case, we'll have representatives from both the Mid-Atlantic SSC and the South Atlantic SSC.

DR. REICHERT: But, to clarify, the CIE is only involved in the review process and not in the assessment process?

MS. BYRD: That is correct, yes. They will only be involved in the review workshop, and so it looks like everybody else needs to check their calendars to see if they may become available, so we may revisit this later in the meeting. The dates are firm for the review workshop and the data workshop. I will work maybe to see if we can get a schedule for actual dates for the assessment as well, while we're here. If not, we'll let you know those later.

DR. BARBIERI: Given the recommendations made by the committee, and like we've done for previous meetings, we're going to have the opportunity to revisit this tomorrow and/or Thursday, after you've had a chance to check your calendars and see your availability to participate.

MS. LANGE: Given my current travel status, if the data workshop is going to be here in Charleston, I will volunteer to be the second person on that one.

MS. BYRD: Okay. Now I think that's it for blueline tilefish. Marcel.

DR. REICHERT: Before we move away from blueline tilefish, I've got a couple of questions relative to the schedule. The data workshop is the week after our SSC meeting in October, correct, if I checked my calendar correctly.

MS. BYRD: I think that's right.

DR. REICHERT: I would be very interested in getting a brief report at our SSC meeting about the stock ID workshop, if at all possible. Then, relative to the schedule, if I look at the remaining schedule of the SEDAR 50, that means that we will review the assessment at our October 2017 meeting, and that we will report on that in the council meeting of December 2017, correct? Okay. Thanks. When is the Mid-Atlantic SSC meeting next year? Do you know already, John?

DR. BARBIERI: For next year?

DR. REICHERT: In terms of I assume the Mid-Atlantic SSC will review the stock assessment also, and so I'm just kind of trying to figure out what the sequence or the schedule is, and I know we can discuss this later, but I'm just trying to figure out what the overall schedule is here.

DR. SERCHUK: Marcel was talking about when the data workshop was going to be held in relationship to the SSC meeting. Could not the SSC meeting be held after, the week after?

MR. CARMICHAEL: He was talking about the data workshop.

DR. SERCHUK: I thought Marcel was talking about trying to get the results of that workshop.

DR. REICHERT: No, I was wondering if we could get a brief update on the stock ID workshop and not the data workshop.

DR. SERCHUK: Okay. I am now concerned about the other workshop. I'm concerned about the data workshop. It's October 24 to 28, and I'm just wondering -- Typically, the SSC meeting is in early October, but could we have it in November?

DR. BARBIERI: By the way, and I don't mean to respond for staff, but those adjustments are made. This very meeting, the timing of this meeting that we are at, it was usually our April meeting, and it was adjusted slightly to accommodate this type of thing, so we could review SEDAR 41 and provide catch level recommendations to be evaluated at the June meeting.

My suggestion, at this point, if we have provided enough input, is that we give staff time to sort of review their scheduling, because, right now, they are trying to sort of reconcile so many different suggestions that they're going to have to reconstruct that schedule, to some extent, and touch base with the Center about the possibilities there.

MR. CARMICHAEL: I guess, with regards to what would be the purpose of the SSC meeting right after the data workshop, because, normally, the full SSC doesn't come in here until we get to the final product in the review, and so we have the SSC representatives at the data workshop to take part and see what's going on and certainly to stay aware. Normally, we don't worry about having a data workshop happen before an SSC workshop, because there is really nothing to report back to the SSC at that stage. It's still so early in its development.

DR. SERCHUK: My point is economics. You can have the data workshop before the SSC and capitalize on the fact that there will be some SSC members that will be in Charleston that will stay here. You could have it the other way, that some people would just come in for the SSC, come in

for the workshop, and then stay on for the meeting. I'm just trying to think whether there might be any utility, as we've had this after -- This one was delayed, and I agree with the Chair. Maybe it's something that you might think about in relationship to the overall scheduling of all these events. Thank you.

DR. REICHERT: To clarify, we generally don't review a data workshop or even an assessment workshop during the SSC meetings. The reason I was interested in an update is because this is the first time that we are holding this stock ID workshop, which I believe is a result of the best practices, that we discussed as the best practices, and so that's why I suggested to perhaps get an update on that specific workshop, just as a clarification.

DR. IRWIN: I just wanted to have my name added to the list of those interested in the assessment workshop, but I have some constraints on my ability to travel, and so I will have to keep that in mind as that gets fleshed out. Thanks.

MS. BYRD: Great. Thanks so much, Brian. Anything else on blueline tilefish? Thank you, guys. The next thing that I wanted to talk about was red grouper. You guys discussed this at your April 2015 meeting. The assessment was delayed a little bit. These are the terms of reference that you guys reviewed at that meeting, and so this is a red grouper assessment that's scheduled to be an update assessment, and so it will be updating SEDAR 19.

Right now, the schedule is for there to be a data deadline at the end of August and then for it to be completed in January of 2017. What we would like some feedback on is you guys from today, and, again, you provided some feedback on this when you discussed it back in April, but, since this is an update assessment, it will just be updating the data sources that were used in SEDAR 19 through 2015, the terminal year, and there is some SERFS -- There is a SERFS video index that could be available for inclusion in this assessment.

If that data were to be included, it will need to be changed to a standard to incorporate those new data sources, and the terms of reference would need to be updated to reflect that. If there is a standard assessment, we will need a panel, and the assessment would likely be held over a series of webinars, because I think the only new piece of data would be the SERFS video index.

Last time, when you guys talked about, some things that I just wanted to mention is that you discussed it some, and it seems that red grouper are well sampled by chevron traps, and there is a longer time series of chevron trap data. However, there is also this video data that you guys may feel that's important to include. I also reached out to Erik at the Science Center, and there some interest there in potentially including the video data as well, and so we just wanted to get your thoughts on whether this should stay an update or should be changed to a standard for inclusion of the video index. If it a standard, then we'll be looking for participants, and it would be held over a series of webinars.

DR. BARBIERI: Thank you, Julia. Any input, first, on the type of assessment, if anybody feels strongly one way or the other?

DR. REICHERT: I don't feel very strongly, but, remembering the discussions we had on standards and updates, if we add a -- If we change an index, I think that would elevate it, based on our

previous conversations, would elevate it to a standard, and so that would be my recommendation, but I'm open for discussion there.

MR. CARMICHAEL: With a standard, the expectation would be that if you're bringing in a new piece of data that yes, you need to do a standard as opposed to an update, and that you should specify the data that are going to be brought in. Basically, what you're doing is you're specifying the flexibility be considered in this standard, and you're accepting that that flexibility is not so great that you feel that it has to go through the full benchmark.

DR. REICHERT: Then I would suggest to the committee to recommend a standard and the additional information considered is the video index, whether it be what's called a CVID index, as we used in the gray triggerfish and red snapper, or a separate index that can be discussed at the panel level, but that would be the additional type of information that we should consider or include.

DR. SERCHUK: Can I just ask a question? What's the model that's currently used to evaluate the population?

DR. BARBIERI: Right now, it's the BAM Model that's used, the Beaufort Assessment Model. It's a statistical catch at age model.

DR. SERCHUK: Which can easily handle a new data series, correct? There is no feeling that the model will change, correct?

MR. CARMICHAEL: No, not that we're aware of.

DR. SERCHUK: Okay. I'm just trying to educate myself. Thank you.

DR. BUCKEL: Marcel, I'm all for including new data, fishery-independent data in particular, but could you speak to the trapability of red grouper and basically if adding the video is just going to be redundant with what you see in the traps? I guess, to move from the update to the standard, are the data telling us something different or is it just going to be redundant?

DR. REICHERT: I have not seen any of the red grouper video data, and so I do not know. I cannot speak to that. We generally feel, although we catch them in relatively low numbers, we do catch red grouper very, very regularly, and so I don't think there's an issue with them avoiding the trap, but, in terms of the comparison of the two indices, I don't think we have that available yet, and so I can't tell you that, but I would assume that that would be part of the consideration in the panel discussion, in terms of the interdependency of the two indices, as we had with gray triggerfish and red snapper.

DR. BARBIERI: Excellent point, Jeff, and I mean this is something that will have to be discussed at that workshop in terms of those two are being redundant or really complementary to each other, in a way that adds new information content. Julia, my understanding is that the recommendation of the committee at this point is that we move forward with a standard assessment, instead of an update, and I don't know your expectations, in terms of timing, for us to provide modified terms of reference.

MS. BYRD: It's my understanding that the only new piece of information that the group would be considering is the video index, and so the terms of reference will need to be adjusted to note that. Then the other thing, and, again, this may be something that people want to check their schedules, is we would to know if anyone would be willing to participate as a panel member in that red grouper standard assessment. I will be working with the Science Center to develop that actual schedule, but the idea would be that it would be a series of webinars.

DR. SERCHUK: Just one final question. What's the time series involved with this new data series? Is it a long time series, like three or four or five -- I'm just wondering.

DR. REICHERT: The video index started in 2010, I believe, and so that would be five years of data.

DR. SERCHUK: How long is the assessment series?

DR. REICHERT: The only thing I know is that the trap series starts for red snapper -- I believe we did not use 1990, and so that would be 1991 through the terminal year, and I'm not sure what the assessment series is and how far we would go back or how far the assessment went back. I don't have that off the top of my head.

MS. BYRD: Mike just looked that up real quickly, and I think it's 1976. 2008 was the terminal year, and so it's 1976 to 2008 was SEDAR 19.

DR. BARBIERI: Okay, and so now we are looking for volunteers for that panel that's going to be composed to evaluate the standard assessment. You don't have to commit.

DR. SHAROV: Can you tell us what the timeline is for this particular assessment?

MS. BYRD: I will have to work on the schedule with the Science Center, but I'm assuming it will be a series of webinars that will start kind of later this fall and maybe go into winter, but I don't have the exact schedule yet, and so I can work on that with the Science Center and then get back to folks.

DR. BARBIERI: Thank you. I think, Julia, this completes Agenda Item Number 5. This has been the gift that keeps on giving, Agenda Item Number 5.

DR. REICHERT: One thing. There's a Table 1. This is something the SEDAR Steering Committee will discuss?

MS. BYRD: Yes, and so the SEDAR Steering Committee is going to be meeting via webinar on the 9th, on Monday, and so they will be talking about kind of reviewing the schedule, which is this Table 1 in your overview document that Mike is kind of pulling up right now, and they will also be talking about feedback on kind of the assessment prioritization, any feedback from the SSCs on that, as well as any feedback from the SSCs on the research track, and so those are the main kind of things that will be on their agenda on Monday, and then I guess the other thing I will note, just as an FYI, coming out of the SEDAR Data Best Practices Workshop last year, there was a recommendation to form a Data Best Practices Standing Committee, and the Steering Committee

recommended forming that group, and that group met for the first time in April. That's just as an FYI to you guys about that as well.

DR. BARBIERI: Thank you, Julia. Before we move to the next item, Agenda Item Number 6, how about we take a ten-minute break, and so let's reconvene at ten minutes after four.

DR. BARBIERI: Our next agenda item is to get an Update on the Mid-Atlantic SSC Meeting Concerning Setting an ABC for Blueline Tilefish. There are two attachments as part of your briefing book. Attachment 7 is the Blueline Tilefish Working Group Report and Attachment 8 is the Mid-Atlantic Council's SSC March Meeting Report. This is an informational item and no action items are required, and we are thankful to Mid-Atlantic Council SSC Chair John Boreman for bringing this update to this committee.

DR. BOREMAN: Thank you, Luiz. I will try to keep this brief and try to keep it at the 30,000-foot level. You have the report. I will just give you a little background first. As you know, the council requested emergency action because of the high landings in 2014 of blueline in the Mid-Atlantic. The council went into panic mode and passed some regulations, but, last fall, they also requested the SSC to work on developing an ABC recommendation to come back to the council for blueline tilefish north of the North Carolina/Virginia border.

We formed a working group that ultimately was chaired by Tom Miller. It also had Cynthia Jones and Mike Schmidtke on it. Cynthia and Mike were here at our last meeting, I believe, to discuss their information that they've been collecting on blueline tilefish. Also, Dave Tomberlin, who is an economist, was on the working group.

The charge to the working group was basically to review the data-poor approaches that can or cannot be used for developing an ABC for blueline north of North Carolina. Based on that review, the SSC will determine what data-poor method is most suitable to use. The working group report, and that's in the packet, decided to follow suit with what we did with black sea bass last year, and that is adopt the DLM took out of the DLM toolbox, developed by Carruthers, et al., as a basis for defining the ABC.

What I am presenting to you here is what I presented to the council at the last council meeting, in terms of the recommendation from the SSC, and so that DLM toolbox has forty-seven different methods available, and I think all of those methods are published somewhere. They're all peer-reviewed methods. A lot of them are variations on a theme, like stock depletion or average catch and so on.

The methodology itself is a two-step process. It begins with a management strategy evaluation type analysis, using simulated data, to determine which methods meet certain performance criteria, which you can set ahead of time, and then, once you come up with a method or methods that seem to fit best for the performance criteria you're looking for, then you apply the method to real catch data to develop an ABC recommendation. In some cases, some of these methods will only give you an OFL and not an ABC, and so that eliminates a few of them right there.

What the working group did is they used life history data taken from blueline tilefish north of North Carolina, but they also used biological information that was contained in the last assessment, the SEDAR assessment, and also other biological information, like the steepness for the stock-

recruitment relationship and so on, information they couldn't glean from the data that were collected north of North Carolina.

The performance criteria that the working group used is the probability of overfishing less than 50 percent, probability of being overfished, which means one-half of BMSY, that less than 50 percent, and then a relative yield of 30 to 60 percent. It says "relative yield" because it's based on simulated data for the management strategy evaluation, and the working group felt that a relative yield of 30 to 60 percent is a sufficient yield to support a viable fishery.

If you have any questions about relative yield, hold off on them, because between then and now, unless Alexi or Amy can help me out, I am looking for the true definition. I don't want to tell you exactly what it is, but, at this point, it's just a yield sufficient to support a fishery that allows people to go out and actually try to catch these things.

The next slide is based on the outcome of the MSE. It was determined, eventually -- Initially, it's just two methods, but, on further examination and reflection about assumptions made in the management strategy evaluation, they decided to relax a few of the assumptions, because they felt they couldn't justify some assumptions, based on the lack of available information.

After performing the management strategy evaluation, they came up with four methods that met the performance criteria we talked about, and that's average catch over the entire time series, and that's from 1999 to the 2015, average catch in the last five years, then a simple catch depletion method, and then the simple catch depletion method that employs a 40/10 harvest rule, which is the rule that the Mid-Atlantic Council uses. We use a 40 percent rule here, but I'm not sure -- The 40/10 is probability of overfishing less than 40 percent. If it gets down to 10 percent, then the fishing is stopped altogether. That's a P*.

Parallel to this process of looking at the toolbox and running the management strategy evaluation, there was a parallel effort to look at developing a catch time series for the Mid-Atlantic region. For that, the fishery management council hired an outside firm to come in, and they essentially ran what's called a Delphi method, and they started out by conducting an anonymous survey of charter boat and headboat operators and private anglers and tackle shop owners with knowledge of the recreational blueline tilefish in the Mid-Atlantic region.

This enabled them to provide some detailed estimates of effort and catch for boats fishing from most ports in the region. It was quite an extensive analysis of a lot of folks that were involved in the fishery. This was followed up by an in-person meeting to combine the collective knowledge and use the Delphi approach in terms of refining the initial estimates and coming up -- They also had a follow-up survey after the meeting to come up with a time series that, at least the participants in the meeting, felt comfortable that they felt it was representative of the catch in the region.

Obviously there's a lot of uncertainty with this. This a new -- The Delphi technique is not a new technique, but applying it to develop a catch time series or effort time series, that's new, and so it was done. There is a comfort level of the participants in the fishery with the numbers, and so those were the numbers that were developed in parallel to the work done with the DLM toolbox.

Step 2 is to take the four selected methods and run 1,000 simulations using each method and then calculate the median ABC value for each of the methods. Based on that, these are the average

catches in kilograms, the outcome for the four methods. It gives you a sense of what the numbers look like, and then the ABC itself is just a simple arithmetic average of these four numbers, and that was the recommendation of the working group and later became the recommendation of the SSC.

One of our terms of reference is what is the overfishing level? It cannot be specified. This was just based on catch and biology, but it wasn't a true assessment in the Mid-Atlantic, and so the SSC couldn't specify what the OFL is. Obviously, because we couldn't specify an OFL, the level of catch couldn't be determined either.

Again, the third term of reference that we were given from the council is what is the ABC? For this, we recommended a simple arithmetic average of about 40,000 kilograms, which is roughly equivalent to the average catch in the last five years or so, but it's quite a bit lower than the 2014 catch that set off this whole time bomb called blueline tilefish in the Mid-Atlantic.

We also were asked about the most significant sources of scientific uncertainty, and, again, here, right off the bat, we recognize there's little information on exchange between how much exchange there is between fish in what we're calling the Northern Southern Sub-Units. The Northern Sub-Unit is that part of the coastal stock that's north of the North Carolina/Virginia border.

Again, the catch time series developed by using the Delphi method, there's a heck of a lot of uncertainty with that. We can't quantify it, but there has got to be. There is high uncertainty in the stock-recruitment relationship, and that's coming out of the SEDAR itself and how comfortable people are using the steepness, which the outcome of the MSE is -- The steepness is key. It's a strong influencing factor on the outcome.

Again, it's unclear if the productivity of the Northern Sub-Unit has increased, and that's why we're seeing more landings now in the Mid, or we're seeing a range expansion, either because of climate change or just a normal range expansion, just say if the stock is growing.

We did note there are different growth patterns in the Northern and Southern Sub-Units. The Northern Sub-Unit had much larger fish and larger fish at a given age, and the extent of the depletion of the Northern Sub-Unit is unknown, how depleted this stock is from its virgin state, and so those are the major significant sources. There are a whole list of others as well.

In terms of research recommendations, which is another term of reference, obviously more accurate time series, fishery-independent sampling. It would be nice to take the sampling that's done now in the Southeast with the chevron traps and so on and extend that up into the Mid maybe someday. Recommendation of the nature of the stock structure and the amount of exchange between the Northern and Southern Sub-Units, hopefully the stock ID workshop will at least start to address that point.

Again, the stock-recruitment relationship and also recognizing that there are growth differences and just trying to drill down to determine what the causes of those growth differences are. Is it because of a difference in natural mortality rate that the fish are allowed to get bigger in the north, or is it environment related or habitat related, or is it just could it be a genetic issue. I think that's in for slides.

As I said, the ABC was reluctantly accepted by the council, because they had no choice. They couldn't exceed the ABC, even though several council members thought it's much more restrictive now than the current fishery. It's going to cause for much more stringent regulations on the fisheries. In the Mid-Atlantic, where the blueline tilefish is an alternate species to black sea bass, that's already being restricted on a lot of these for-hire fisheries, it's going to cause some economic strain in the fishery, but that's our best biological shot at it.

We're going to -- We only gave a one-year recommendation. We're going to come back and revisit this again, and, also, obviously the Mid's eyes are on what's going to happen in the Southeast here and the South Atlantic with the stock ID workshop and the follow-up data workshops and so on. That's a wrap-up, and I would be glad to answer any questions, or try to answer any questions. Thanks. By the way, if Amy or Alexei -- They both participated in the SSC meeting. If you have anything to add or to change from what I said, feel free to speak up.

DR. BARBIERI: Thank you for that summary, John. It was very informative, and let's open up the floor for questions and/or comments from the committee, please, if any.

DR. REICHERT: A quick question, John. So this was a one-year recommendation, and so this would carry -- This recommendation would carry until mid-2017?

DR. BOREMAN: I'm trying to remember. I think the fishing year starts on November 1, and so it's for the fishing year 2017, which starts November 1 or December 1. Next year, at this time, our February meeting in 2017 or our April meeting, we're going to have to revisit blueline and come up with another recommendation.

DR. REICHERT: So it's my understanding that you will have to go through a similar exercise before the results of the stock assessment may be available.

DR. BOREMAN: Definitely. We probably will go back and revisit the toolbox and probably rerun the management strategy evaluation, maybe, hopefully, with better data.

DR. BARBIERI: Any other questions for John or Amy and/or Alexei?

DR. SHAROV: No, but I just wanted to say that I was impressed by the workgroup that put together that analysis, considering the amount of data that was available. I thought it was really good work.

DR. BARBIERI: Thank you so much, John, Alexei, and Amy for taking that up for us and John for the report.

DR. BOREMAN: Yes, and I just want to, again, thank Amy and Alexei. They both were right in the middle of the discussions and were very helpful and gave us some good guidance. We had to have a follow-up webinar, because we sent the working group back to do some additional work, basically to develop more clarification in the writing of the report on why certain assumptions were made and how things were done, but I think it was worth it and Amy and Alexei were key to that. Thank you.

DR. BARBIERI: Very good. Before we move on to our next agenda item, Number 7, which is Review of New Bag and Size Limit Analysis Methodology, I just want to get your input regarding -- We have about an hour left this afternoon. Then, tomorrow morning, we start with presentations and discussions of SEDAR 41. My point being, and I was getting some input here from Marcel on this, is that if we don't finish all the items that had been planned for this afternoon, I still would like to start fresh tomorrow morning with SEDAR 41 and basically adjust the agenda accordingly.

It's always good for us to start the day rested and with plenty of coffee with something that's more detailed and analytical and is going to generate, most likely, a lot of discussion. With your concurrence, we tried to go through the items in order, as we have them listed in our agenda, but, if we cannot complete some of them, we will continue the same order listed here after Item 11 as we start in the morning. Any opposition?

DR. SERCHUK: Of course, I have no opposition to the Chair. How foolish of me to think otherwise, but I was wondering -- I don't know which item you're going to do next, Mr. Chair, but since we've already had a number of discussions about the SEDAR process and so on and so forth coming up, whether you would entertain going to Item 10, which is Update on the Research and Operational Assessment Tracks. I think that might, again, be able to fit within the hour.

DR. BARBIERI: I think that's an excellent suggestion, Fred, and yes, we already had discussion of SEDAR items that generated a whole lot of input and questions from the committee, and so if Erik is ready, then we move to Agenda Item Number 10. It's an Update on Proposed Research and Operational Assessment Tracks. We want to thank Erik for coming over and giving us that overview.

In terms of briefing book documents, Attachment 12 in your briefing book is SEDAR Stock Assessment Categories. In terms of action items, is comment on and provide feedback for the proposed research and operational tracks, and so Erik is going to give us an overview of how far they've come with that process, but then he is requesting committee input and any suggestions for them to continue this process.

DR. WILLIAMS: Thank you, Luiz. I'm Erik Williams from the Southeast Fisheries Science Center. I strongly encourage you to read the attachment that came with this presentation, and I won't go over that. Hopefully everybody can just read that. It's only a quick two-pager, or two-and-a-half pages, and I will try to be brief with this. Hopefully it's kind of fairly straightforward, but we'll see.

The problem, if we would like to characterize it as such, that the SEDAR Steering Committee is trying to address and the Science Center is concerned about is that we're not getting enough assessments done, essentially, and this is an analysis that we did at the Center level. This is combined data from all three of the councils. We respond to the Caribbean, the Gulf, and the South Atlantic.

What we're focused on is that there is a lot of stocks that are still unassessed, and those are determined to be unassessed because it looks like there is sufficient data to do an assessment, even as much as a full age-structured assessment in some cases, but they just haven't -- We haven't had time to get to them. We're also short on staff, and so, when we're short on staff and we have a lot

of things to get done, we have to then look to the process, to try and modify it and make improvements in efficiency, is what we're after. We're trying to basically create more throughput.

The existing process, as you all are quite well aware of, consists of the current SEDAR process. It consists of a benchmark track, a standard track, and an update track, and hopefully you're all familiar with those. Some of the issues we have with those mostly arise from the fact that we're expecting our benchmarks to meet a deadline, but, at the same time, be a wide open process to address many different methods and consider the data basically for the first time. There is an inherent sort of crunch that gets created in that, because we can't necessarily anticipate all the issues that might come up by just looking at the data and choosing a model off the shelf and applying it. Invariably, that results in some issues that have to be readdressed.

In almost every case I can say, at least with the benchmarks that I've been involved in, we end up with a deadline crunch, of sorts, and this was mentioned earlier, that we end up pushing decisions later on into the process because we run out of time.

Standards aren't quite as bad, and they allow for some changes. Then, of course, updates are the fastest, and that's a good process there, and so what we're proposing is to, in true government fashion, rename the processes, which is always good, and then also make some tweaks to them. What we're proposing is to go to a research cycle and an operational assessment cycle, and so basically only two categories. Essentially, the research cycle would allow the time and the flexibility necessary to fully address all the issues that might come up and to consider many models than we often have to in the current benchmark process.

The downside to that is that it takes a little more time and we're proposing that we don't expect management advice to come out of that process, because, by expecting management advice, you are putting that deadline on it, and we're trying to avoid that, because that's what is causing some crunches. Frankly, it's probably causing some dissatisfaction in the results, when things aren't fully investigated to everybody's standards, and so hopefully the research cycle will allow that full exploration and that full vetting of all possible models and ways of looking at the data and the models.

On the flip side then is the operational assessment, and this is where we're hoping we would get most of our gains. After running an assessment through a research cycle and having fully vetted it, we would hopefully be able to gain some efficiency then by running it in an operational mode very quickly and be able to fit more of those within a year.

Again, this is sort of a quick bullet description. This is also, more or less, mirrored in the attachment document that came with this in your briefing book. It's basically describing what we're looking for with a research cycle. I'm not going to read through these. I think hopefully that I described it fairly effectively just now.

In operational assessments, the big key here is that it's going to produce timely management advice. We can actually schedule these based on when data sources become available too, and we can gain some efficiencies by lining things up that way, and so, again, in summary, sort of the advantages of the new approach -- Again, the research cycle, the main advantage is that we're hopefully going to get a more vetted, thorough process that's going to result in an operational model that everybody will be more satisfied with and we'll be able to -- We'll be able to rely on

that operational model for more cycles before we have to ever kick a stock back to a research cycle, perhaps, and so maybe we can stick with that operational model for maybe a decade or more for that particular species, thereby cutting down the number of times we run a stock through this research cycle. More often than now, we run it through the operational assessment.

Here is just a mock-up of what it might look like in an actual setting, where we had a hypothetical situation where we had five analysts working and how we might break that up into two years. We could get essentially about ten operational assessments that could be completed and three research tracks, would be just an example.

Basically, the long-term averages that we're hoping to get out of this system is that we could conduct about one to two research track assessments per year and four to six operational assessments per year with a staff of five, and so this is just a hypothetical. Of course, the timing of these might be adjusted and other things like that, and so this is where the SEDAR Committee is thinking about heading. This is where the Science Center would like to see things go. The overarching desire here is to basically increase throughput, because, right now, the SEDAR process doesn't seem to be meeting our needs in terms of total throughput of assessments and providing timely management advice. I think that was it, and so input is definitely desired, any questions or feedback.

DR. BARBIERI: Thank you for that, Erik.

DR. BOREMAN: Thanks, Erik. You know that the Northeast is going through a similar soul searching, I guess, but one recommendation -- Well, first of all, a comment, and Fred may want to chime in, too. We seem to be having a tough time getting into this research track mode, because there is always a desire to have an assessment coming out. There is a need for updated assessments and new assessments and new biological reference points and management reference points and so on, and so we really haven't I don't think, have launched into a true research mode yet, a research track mode, which we're still trying to do. I am saying we as the collective we of the whole Northeast science and management structure.

I do recommend that you don't call them research track assessments, because "assessment" implies that you're going to be doing an assessment. A research track could just take one issue and get into it in depth, and maybe that will feed into a future assessment, but calling them research track assessments I think might raise some false expectations of what comes out the other end. Thank you.

DR. BARBIERI: Good suggestion. Any other --

DR. BELCHER: Just out of curiosity, with the current assessments we have out there, how many would you label as operational?

DR. WILLIAMS: That's a good question, and I think John probably alluded to that, is how do you dive into this process and then get it up and running? I've thought about this, and I don't know how we could -- I mean I don't know how everybody would agree to do this, but I think one method would be to actually hold a research cycle to look at which stocks we would agree are already operational and which ones aren't, and just review them from that standpoint. That might help us make that decision, but you're right that diving into this new process is the tricky part.

DR. GRIMES: I was just going to say that perhaps an additional -- It's good, I think, and an additional advantage might be that research cycle work presumably would be published and peer reviewed, and this would lend credibility to the agency, to the quality of the assessments themselves, and no doubt make your stock assessment scientists a lot happier, too. I mean they were scientists, after all, and not just -- They didn't sign on to be pushing the stock assessment wheel of pain around and around.

DR. SERCHUK: Unfortunately, I've had a lot of experience with developing these concepts. In fact, I may have started with John was -- It goes back some time. Part of the issue will depend on the appetites of your clients. In the Northeast, the appetites of our clients, no one wants to go on a diet. They want to go to the buffet table, and so it's been, in my view, very difficult.

I think, presently, at least in the Northeast, we are caught up with all three types of assessments. I would liken the research track assessments to the current SAW/SARC assessments, because they are benchmarks, of a type, and then the Northeast also provides operational assessments, and they also provide update assessments.

There are a couple of good things and a couple of bad things. The idea behind doing a research endeavor is to free up some of the time for the staff to engage unmolested in using their creativity to think about a new way forward, think outside the box, bring in new models, think about new data and be creative. I think, without that, the entire assessment process is dead.

Unfortunately, that's the part of the process in the Northeast that has been the slowest to develop, and it still hasn't really developed. They still haven't weaned themselves away from a process in which benchmarks are done and used as soon as they are finished as the basis for subsequent management advice. When crafting the working papers for this endeavor, it was clear, just as you mentioned, Erik, that the scientists need to be off the schedule, off the advice-giving schedule, to think about it, and it was also contemplated that they wouldn't use the most recent year's worth of data, that it wasn't necessary if you're going to thinking about how to rethink things.

The minute a new assessment came out, a new model came out, particularly if the new model showed things to be more favorable than the existing model, the clients came in and said, no, we want to use that for advice, and so you were caught up in the treadmill again, and so that's one thing to think very carefully about.

I like the idea of the operational assessments, and I think this was brought forth in the most recent, I think it was last year, where the Northeast Center engaged in twenty operational assessments for all the species covered in the Northeast Groundfish Plan. That was a large undertaking, but the beauty of it was that every stock in the management plan was put on exactly the same timeframe. That is, you were bringing everything up to 2015.

You aren't dealing with some stocks in your management plan that were assessed three years ago and others four years ago and others last year, and so there was a disjoint in terms of those fisheries that were prosecuted on a number of different stocks, all with a different assessment timeframe, and so the beauty of the operational assessment is you can do a number of them and try to bring them all in sync with the management time certain, and I think there's a lot to be gained from that.

I still think that what the managers would like to have, and this is where they would like to have the updates. They would like to have the updates, just to see whether the management plan is performing and meeting the goals that they set forth from the assessment that might have been done two years ago. Is the stock rebuilding? Are we getting the gains and so on and so forth?

I encourage you to move forward, but I think you need to keep your eyes open, and I think it has to really be done with a complete understanding of the clients. The council really must know what's going on and appreciate that if you want a better product that you have to have an R&D section, and the R&D section is the same people -- It's like the cancer researcher that when he's not doing cancer research is you're a family doctor that has to be the practitioner, and you can't be the practitioner every day and dealing with the public's ails and still make the breakthroughs on cancer research. It's easy to say, but the fact is that the assessment scientists get squeezed doing both jobs. If they do both jobs, they're not going to do either job well.

DR. BARBIERI: Thank you for that, Fred. Any other questions or comments or advice or input for Erik regarding --

DR. REICHERT: I've got a couple of probably more practical questions, because it's my understanding that the operational assessments may be done by different staff than the research track, and you mentioned in your example, for instance the long-term average is five analysts, and how many analysts do you guys currently have, or is this --

DR. WILLIAMS: We currently have five lead analysts.

DR. REICHERT: Was there a lot of discussion in terms of when is going to start, or is that still in development?

DR. WILLIAMS: It's still in development, and I believe -- Again, I think John Carmichael mentioned it earlier, that the SEDAR Steering Committee is meeting next Monday, and they're looking for input on this topic from you guys, from this meeting, and I don't know where they are in terms of whether they're going to make a final decision about yes, we're going to embrace this fully or not. That's where I'm not sure.

DR. REICHERT: I think I discussed it before, but I was really happy to see that the consideration of different models are part of the research track, because I think sometimes we get caught in model selection or we may not have enough time to fully explore different models, based on the data, especially if we are under a time crunch, and so I was very happy to see that.

In addition to that, and that may be something that the SSC can pick up later, because I think it will also change the role of the SSC, in terms of the review and the involvement in this process, because it's my understanding that SSC members will be involved or will be part of the research track. Then that's where the CIE is going to be involved and all the operational assessments, it's the SSC is the sole reviewer of the operational assessments, and so I think that changes the role of the SSC a little bit, and I think we probably need to see -- I'm not sure if it may increase the workload of the SSC, because we are involved in the current SEDAR process, actually in all steps, data, assessment, and review workshops, and also in the standard assessments, but I think it's good for us, as an SSC, to kind of think a little bit about what our role would be and how would that potentially affect our workload if we would go to this assessment schedule or model.

DR. WILLIAMS: If it achieves one of the goals, which is the increase throughput, and you guys are the sole reviewers of operational assessments, then yes, it's going to increase your workload, in that sense.

DR. BARBIERI: That will be inevitable there.

DR. BELCHER: Any thoughts on who is going to determine what goes into the research tracks as you're getting ready to go into this, because obviously we have some that, from particular views, there are some pressing issues that people want to see done, and so how is the determination going to be made? Is it going to be through SEDAR or someone else?

DR. WILLIAMS: I would envision it occurring exactly the way the SEDAR schedule is being set now, which is an interplay between the council and you guys, the SSC, and the SEDAR Steering Committee, sort of making those decisions.

DR. BELCHER: These are questions that are just rattling around in my head. Then the other question would be is you have a species that enters in and goes into a research mode. When would be the next time it would cycle back through, or what would determine whether or not it's eligible to be back into a -- The idea of we keep revisiting a benchmark, at what point do you say it's already been through a research track and it's operational and it stays operational until what flip of the switch, however many number of years? Is there any thought on that?

DR. WILLIAMS: I think generally the thought has been that the year after, or as soon as you could fit it into an operational after a research cycle is completed, you would do that stock as an operational the next year, update it and get the latest data and run it through. I think that's the way it's been envisioned.

If you step back and look at what this process proposal is trying to do, we are trying to build in a governor of sorts, if you want to call it that, so that the research cycle is not triggered very often, because there comes a cost with it. It costs us an analyst for a year, it costs us a delay in management advice, potentially, but the advantage is then you've created this operational assessment that can be conducted multiple times and hopefully for a longer period of time. Yes, that is sort of one of the underlying themes of this process, is to try and get us shifted towards more of these operational assessments and less of spending our time on benchmarks and research cycles, except only as needed.

DR. REICHERT: Currently, we, in this region, have over sixty stocks. A lot of them are very data-poor, but I assume that part of this research track is maybe looking at -- Rather than looking at individual species, look at methods. That may reduce the number of species that need to be assessed, but have you guys looked at, if we're talking about four to six operational per year, what's the -- That may be kind of a follow-up of what Carolyn was asking, but have you guys looked at an operational assessment of a stock once every how many years, or does that depend on the SEDAR Steering Committee recommendations, or is this also part of providing a system where the sequence and the schedule is set over a longer period of time, rather than being determined on an annual basis?

DR. WILLIAMS: I think the schedule would still be determined on an annual basis, and I think this is actually leading into my presentation that I have to give to you guys tomorrow about stock

prioritization, because I think that's going to feed into this whole process as well, and I think there will be, hopefully, if we get this all worked out, a nice interplay of looking at stock prioritization results and then looking at the schedule and figuring out where to put things and when to put them.

DR. BARBIERI: We can hold that conversation until that presentation. Thank you. To that very point, Erik, we have about thirty minutes left. This seems to be the perfect segue. It fits so well into how these two things are going to intermix. I like the idea. I like this concept a lot, but I just wonder how realistic, and I think this is Fred's point, this is going to be, given the management needs and the realities that the council needs to address. The proof is in the pudding and we'll get there, but if you have your next presentation already queued up, with the committee's concurrence --

DR. SERCHUK: May I make another point, Mr. Chair? I really think that you have to engage the council and not only the SEDAR Steering Committee, but I think you have to engage the council as a group, because you really have to get out people's aspirations and you have to get out the difference between wants and needs.

That's very clear, and I think that, on behalf of the Service, they will have to basically point out what their limits are, what they can do with the team that they have and try to forge a way forward that is optimal for everyone and takes advantage of the creativity of the team and takes advantage of the needs of the council and tries to keep the operational assessments up on a timely basis, a multiyear basis, maybe every two years or every three years, and also puts in some flexibility into the system, maybe for updates or maybe for things that come up that are really unanticipated changes in assessments that need to be looked at that you really -- At any point, you can't plan for, but always happen, and that's that flexibility that you can't fill up people's schedules with 100 percent of their time.

You can put in 80 percent and try to go with that, and then leave the 20 percent buffer for -- We know there's going to be an exigency that's going to come up, we know there's something happening in the fishery and we know that something is not right. Thank you.

DR. SHAROV: I definitely see lots of advantages in what is proposed here in terms of the research stock assessment. Based on the discussion that we had right now, what we need to do is to engage into the research stock assessment, because that has not been done yet, and we just need to try it. What Erik has described here, that such an assessment should mostly focus on more principle things, new types of models and tests of new hypotheses, et cetera, et cetera, for which we don't have time through the regular assessments, but, at the same time, the value of the benchmarks, in my experience, was that that was the place when all the tensions were brought together. That's the pressure cooker, when finally you get the valve and the things are getting out of there.

My question to Erik is that he has definitely thought of this and can you give an example? Do you have an idea of the first research assessment that potentially your group could do and what it would focus on? Would it still try to address multiple conflicts or modeling issues or data issues with particular species or what -- What would be the first one to try?

DR. WILLIAMS: It's actually already on the proposed SEDAR Steering Committee's schedule, or the SEDAR schedule, I should say, and that is they have tentatively endorsed -- I don't know if

they have finalized the notion of addressing scamp, both in the Gulf and the Atlantic, as a research cycle, and so it's scheduled to start in January of 2017, I think. John could speak to that.

MR. CARMICHAEL: Erik is right. The Steering Committee's intention is to try the research track process there with the scamp, and the idea of bringing in a joint assessment is it gives both of the councils who are dealing with the bulk of the SEDAR things, the Gulf and South Atlantic, the opportunity to share in the experience together, I suppose, equally.

Then, as we've done with other changes of this magnitude, the Steering Committee would evaluate it, once it's done, and decide how to proceed. Perhaps the approach is modified or what have you, and so they're interested in general comments, and they're also interested in thinking about specifics as they'll apply to that assessment, and certainly when we come to you with things like the terms of reference and the schedule and all of that, thinking about how those particular documents will have to be modified to deal with this type of approach. It will be useful when they come to you, however long it is from now, at a future meeting.

DR. WILLIAMS: I appreciate Alexei's comment about the benchmark putting people into a pressure cooker, in a sense forcing them to make some decisions, and I agree that's one of the concerns that I have, but I think that we'll have a strict enough timeline, even under the research cycle, that it will still force decisions to be made, but just hopefully not on as a tight of a timeline and not forcing things to be left undone, in a sense, and pushed further down into the process. Then they're really hastily done, in my experience, in some of the current benchmark processes, and I think this will alleviate some of the pressure, but not completely, and so maybe that's the answer.

MR. CARMICHAEL: One of the biggest things, of course, will be the scheduling, because if you think about our discussions just now on SEDAR 50 and blueline, the review workshop is already scheduled, and so a lot of times these issues will come up in an assessment and they need time to be worked out and you bump up against having to have things done for the review, and so one way we've thought about implementing this is perhaps not scheduling that quite as rigidly as we do now and waiting.

Now, we can't wait forever, because there is a lead time required of the CIE in making those requests and setting up meetings in general, but I think we would try to perhaps maintain some flexibility in that endpoint. If things go faster, maybe we do it a little sooner. If something blows up that needs further evaluation, then we would have that opportunity, if we're not as locked in with CIE guys planning to be here on May 13. If we can maintain some flexibility there, that might help the process a lot.

DR. SERCHUK: Can I ask a related question? One of the things that is on the schedule is the MRIP revisions. Presumably, that is either the new estimates or consolidating the estimates. That will have a huge impact, and I'm just wondering -- That will affect many, many assessments down here, and I'm just wondering how that might best be handled when you undergo that, because we talked about something generic affecting many stocks, and that will.

It might also affect allocations and it might affect many, many things in the management process, and I think that could be a huge undertaking. I am just asking, how much thought has been put into that with respect to how to unfold those changes within the assessments and the council's

wishes to utilize that information, even going back historically, or how to best include that in the assessments. I'm sorry to raise it now, but I see that as a huge issue involving both the Center staff and having a huge interest by the management council.

MR. CARMICHAEL: There has been a lot of discussion of that, certainly at the Steering Committee and at the council, in terms of how we deal with those. It's kind of a two-pronged approach identified now. One is that there are likely some stocks where you need to do an update, as we commonly think of an update.

Then the idea of the revision is that that's a more specialized type of assessment and that could be more rapid, and it's been defined as being that, and so whatever the terminal year of the prior assessment is, that will stand. I think it will simply bring in only the new MRIP data and plug it into that model, based on the terminal year, and not bring in additional indices and age comps and length comps and all of that stuff.

That's intended to be a very rapid way of just bringing things up to speed with the MRIP data, and so where it was left at the Steering Committee last time was that the Science Center would do some evaluations of the stocks and provide some advice back to the councils on which species seem to be amenable or appropriate to use that type of assessment, versus which ones may require more of a full update, whether it's because of the time since the terminal year or other issues that might be going on, that that could be misleading. Perhaps recreational data is just such a huge component that it might have other effects.

We're hoping that we get some feedback at our meeting next week and then, by October, that we'll be in position to identify which stocks get sort of what type of assessment approach to deal with this revised MRIP data.

DR. SERCHUK: My concern is I don't think it's going to be that simple. It will affect the productivity of the stocks. If you're adding more catch into it, some way or another, it will change the OY and it will change the ABC. It could. That's my experience. It's just not going to be we're updating it this year and -- All of a sudden the whole playing field has changed. The goalposts are different now, or could be now, and that's the thing that I'm concerned about.

DR. BARBIERI: To that point, not to belabor it for too long, but, as you know, the MRIP Review Committee, this is one of the terms of reference that is being addressed, and I know that the NMFS Office of Science and Technology has been really focused on trying to address this, both in terms of how these adjustments are going to be done for the recent past as well as historically, in terms of getting some processes there evaluated to get that done, and so this is being thought about. I'm not sure that we are ready to provide a conclusive answer to that just as yet, but it's a very good point that's going to have to be taken into account.

DR. SERCHUK: I'm just saying that this could be a very large activity, taking a lot -- Not only in the assessments, but in interpretation of the assessments. It depends on the model you're using, of course, and I'm thinking if you're starting to think about research tracks and you know this is coming along, to the extent that you recognize that this could be a very large undertaking, you sort of plan for it, that's all.

DR. BARBIERI: Fred, to that point, because I think those are very good points, please, please, please -- I mean Chip is taking some notes there, but later, we're going to have -- This is the type of feedback to the Center and to the council, to the SEDAR Steering Committee, that's very valuable to present all of these issues that need to be considered and some thoughts on this, and so let's make sure that we kind of flesh out a lot of those points in our report, so we can provide helpful input to them.

DR. BELCHER: The other question that Laura and I were just talking about too is, obviously to Erik's example, I'm assuming that's just relative to your staff at Beaufort, right? The question becomes, when you start folding in ASMFC into the process and HMS into the process and the Gulf and the Caribbean, how many research tracks -- How are they determining research tracks at that point, because obviously that would be -- I mean is ASMFC going to have that option to submit for a research track?

MR. CARMICHAEL: The research tracks are coming out of the Center, and so, at this point, it's envisioned as largely applying to the South Atlantic and Gulf of Mexico. The Caribbean, because of its unique situation and the data challenges that exist down there, is taking a little bit different approach. They're focusing on a lot of data-limited methods right now, and they've been handled with a little more flexibility than the other two anyway, as a result of just their unique data challenges.

For the commission, SEDAR provides just a peer review, much as the Northeast provides another -- The SARC provides a peer review opportunity for them without necessarily being as involved in -- SEDAR is not involved in their assessment process, and so the commissions, both Atlantic States and Gulf States, have their own assessment procedures, and they follow those and we give them an opportunity to bring some things for peer review.

HMS is somewhat similar. They've operated, in a lot of ways, on a little different timing and schedule than other parts of SEDAR, but, depending on how a research track works out, that might be something which they decide to follow as well and fold into their process, particularly because there are a lot of challenges in many of those assessments, as you're well aware. I would say, for HMS, that's one that may very well, being that it runs through the Center, could pick up the research track approach and use it.

DR. BELCHER: Procedurally, what changes then relative to -- If HMS and ASMFC have been under that umbrella of SEDAR, are they now going to be outside of SEDAR?

MR. CARMICHAEL: No, they would still be within SEDAR, but we have a benchmark process and standards and updates defined for SEDAR, as done through the Southeast Center, but that's a little different than is done through ASMFC, and so they would exist -- ASMFC and Gulf States exist in the same way that they do now, as partners with SEDAR, and we provide a peer review.

The same is actually quite similar when we deal with assessments done by Florida. The Florida FWC, Luiz's guys, run that assessment and they prepare those models. They bring them to SEDAR as a peer review, and we provide the peer review opportunity for them through SEDAR, without getting into all of the details. The SEDAR coordinators aren't handling data and assessment workshops for Florida assessments, nor for Atlantic or Gulf States assessments.

DR. BELCHER: Just continuing on with that thought then, so, understanding the difference with the commissions, but HMS, and knowing that it's federal fisheries management and has all the same auspices that apply to it and how assessments are -- If somebody pushes for a research assessment, is that something that wouldn't apply to them then?

MR. CARMICHAEL: No, as I said, I think HMS may gravitate toward the research track process, if they think it's a good way to get through their assessments. I don't know that the other partners will do it. I mean they may consider that a lot of times -- You know I think Atlantic and Gulf States tend to go in with a much flexible assessment process, and, a lot of times, they, because we're providing a review, they may be working on the assessment for quite a while before they come to SEDAR and say, okay, we want to do a review, or they may tell us that we want to do a review sometime in 2018, but we're not exactly sure when and we have to run it through their process, and so sometimes they are doing a bit more flexible approach than what our standard SEDAR benchmark has always been.

DR. BOREMAN: Two points. Number one, on this research track, there should be research questions that are being answered, and I mean not just leave it open-ended, like we're going to move this species over to a research track, which means remove the timeline and just work on it until we feel we're finished. There should be specific research questions that should be answered so you know when to say when and pull the plug and get it back into the process.

Second is something that we're wrestling with in the Northeast too, that I don't think we've really thought about in-depth much yet, but how would you handle defining best scientific information available? If something comes out at the research track and it's a piece of information that's gone through peer review, there is going to be a lot of pressure to say that's now BSIA and so it has to be used in an assessment.

There should be some rules set up ahead of time, I think, on what will define BSIA. This is an issue we're having with MRIP, as Luiz knows. With the new methodology, the new effort survey, we had to be very careful to define BSIA as it's BSIA when we say BSIA and nobody else, and after we've had a few years to test it and make sure we can weave the new methodology into the current time series. Those types of rules should be thought about ahead of time, so it's very clear when this new information is going to be inserted into the assessment cycle. Thank you.

MR. CARMICHAEL: Considering that John made the comment earlier about a research track having like a topic that it addresses, as opposed to a general assessment tool, and maybe we should have some discussion of whether or not that's the best name, because I think the intent of the research track is just a more flexible way to build that robust benchmark assessment tool, which then gets updated, and "research track" may be somewhat misleading.

One of the comments of the Gulf SSC when they talked about it, as I recall, was saying, well, in a lot of ways, this is really not so much research as it is more or less what you're doing now, just a little more open-ended, or perhaps you give yourself another six months to get to the endpoint, and that may well be, and so maybe "research track" isn't the best name.

DR. WILLIAMS: I appreciate all of the comments, Fred's and John's, and certainly Fred's caution about keeping the clients in mind and what their perspective on this is going to be, but I think, hopefully, the one thing that we can all agree on, that is what the desire seems to be, is more

throughput. We would like to see more assessments coming to the table, more management advice coming to the managers more timely, and I think that's the ultimate goal that we're all after here, and so hopefully this is a step in that direction, and hopefully everybody would recognize that, and that's, I hope, where we're headed.

DR. REICHERT: I also recommend, and I'm putting on a different hat to involve the data providers in the scheduling and in this process, especially those and of course that's close to my heart, those providing data that take a little longer to provide, aging and reproductive parameters and the video index, and there is probably a variety of others.

The good news about the research track, or whatever terminology is going to be used, is that we don't need the most recent data. However, for the operational assessments, we do need the most recent data, and if there is an increasing number of operational assessments, that becomes a very critical issue, not only in terms of timing, but also in terms of Jennifer Pott's shop and others, in terms of available resources, personnel and funding, to provide the data necessary for the operational assessments, because we can plan all the operational assessments we want, but, if the data is not available, they're not going to happen.

I encourage to make that part of this process, because we are -- I think other labs are in a similar position, but we are kind of still catching up to provide age data and reproductive data on species that we haven't looked at in a while.

DR. WILLIAMS: That's a very good point, because all we're doing is potentially shifting the bottleneck, but hopefully we're shifting it in a direction that then is a little more manageable, or maybe we can open it up a little easier than where the bottleneck seems to be right now, which is the SEDAR process and the number of assessment scientists seems to be our limiting factor right now.

DR. BARBIERI: Very good. With that, I think it's the perfect segue, Erik, for us to go -- We might go a little bit over time, and I apologize to the Socioeconomic Panel that has been here faithfully since 8:30 in the morning, but, if they can bear with us just a little longer, I think we can get this item completed.

DR. WILLIAMS: I will dive right in and try to make this as brief as I can. You have already talked a little bit about stock assessment prioritization. I think Rick Methot gave a presentation to you, and so you're familiar with what we're trying to do here. Here is a little brief history of where we've gone with this. It started back in 2011, and now here we are. We have a final document that came out in August of 2015, which is in your briefing book.

That document is actually quite detailed, and it basically spells out how you should go about determining stock assessment prioritization. Sort of the steps involved are shown here. Again, I won't go into too many details, because part of the overall process is also to be flexible with the council that you're working with in terms of setting this whole system up. It's ultimately there to serve the council, to provide them some advice on what stocks should be priority, based on their decisions that they've made and their SSC's input and all of those things, and so it kind of gives them a tool to work with that they don't necessarily have to stick with, but hopefully it's a useful tool for them, and it hopefully highlights where there might be some issues with a stock that might

need to be addressed, because it hasn't been addressed for a long time, per se, or something along those lines.

Again, it's a complicated process, and having gone through the steps that I've gone through -- What I have done is put together a tool that basically follows the process that's outlined in the technical memo that follows these steps, using data that we can put into this tool, using weighting factors and some other things, to then come up with a list of prioritized stock assessments.

This is envisioned as being done on an annual basis, and so once the tool is set up, once we've established certain weighting factors, then all we have to do is update some of the input data and you get a new updated list every year, and so when the SEDAR Committee meets or when the council meets, they will see that here is your prioritized list for stock assessments, and now tell us which ones you want to assess.

The roles in this are sort of outlined here, and the role of my agency is basically to help collate the data from past assessments and provide the scores, with input from the councils, and then we also need input from the regional steering committee, and then they use the proposed list, in the end, to basically come up with the schedule for stock assessments.

Just a brief overview of what kind of information goes into this. I think Rick Methot probably covered this in some of his earlier presentations to you, but this is hopefully a quick, thumbnail sketch. We have a lot, of course, of fishery factors that go into this, including the importance of the commercial fishery, importance of the recreational fishery. Subsistence importance doesn't really apply to this region as much as it does to others. Non-catch value, constituent demand, and rebuilding status would be some of the fishery factors that are going to go in.

We have other factors based on the stock itself, the condition of the stock, what it's relative stock abundance is, what its relative fishing mortality status is. There is a factor for ecosystem considerations, or ecosystem importance, and then we also have other factors, and I guess they're calling them assessment factors, that account for things like unexpected changes in some of the leading indicators, and this is where information from Marcel's CPUE document might come in handy, because if we see a sudden change in that, we might want to increase the weight on that factor. As new information becomes available, that becomes an important factor, and how many years the assessment is overdue is also an important factor.

This all goes into a scoring system and basically, after some weights are assigned by both the SSC and the council, we basically have a scored list that comes out in the end. Here is just an example of some of those scores and where the information might come from, and so this is, again, a little small, but hopefully you can read it. It's those same factors we just went over, the fishery factors, the stock condition factors, the ecosystem and assessment factors. You will notice in the source, and just look down that column of source, that the ones that are labeled as "experts" is where we need input.

This is where the SSC is going to have to weigh in or the council is going to have to weigh in and basically come up with some weighting factors for those factors, and I will go over that in a little more detail when I actually show the tool that we've put together, which is an Excel spreadsheet. You can see more clearly where we need information input from the SSC or from the council.

Again, sort of the next steps after -- I think this was after Rick's presentation. He outlined that first we have to defined the list of stocks. The way we did that for the South Atlantic is I actually -- It was basically John Carmichael and myself. We sat down and had a little exchange going back and forth of, okay, what stocks do we want to put into this initial try of the prioritization tool? We didn't want to bite off all of them at once. We thought we would bite off a pretty good chunk and then maybe eventually fold in the remaining stocks at a later point, but the whole idea is really to get this tool up and running, so that then we can start to use it.

One of the other inputs we need is the target frequency, and that has to be scaled somewhat, because it's dependent on the resources we have. In the case of our region, as I mentioned, I only have five lead analysts working at the Beaufort Lab, and so our target frequency has to account for the fact that we only have so many warm bodies that can do these things.

One of the next steps is getting these importance scores, and that's where the expert opinion is going to come in from both the SSC, and potentially the council as well. It's mostly the SSC for these importance scores, and then the last step is getting input from the managers, or the council, on how they want to weigh these factors. How important are some of these factors in determining the stock assessment priorities? There is a lot of flexibility there, so that this tool will hopefully address what they consider to be the more important needs. Like I said, a glaring example is they have a factor for subsistence, which really doesn't have as much importance in our region as it would in others.

I thought the easiest way to explain all of this was to actually walk you through the tool that's been together, and so -- Where to start? Let's start with -- This is, unfortunately, a little more complicated than it needs to be, but hopefully, when I walk you through, you will see.

The first step, as I said, is John Carmichael and I worked on taking all of the existing stocks in the South Atlantic and whittling it down, and so there's a lot here. Obviously we're not going to deal with all of those in one bite, and so we whittled that list down to, and here is one way to look at it, down to thirty-one stocks, I think, thirty or thirty-one, and they're listed here. It includes pretty much every stock we've already assessed, obviously, and then it includes a few unassessed stocks, and it includes some important stocks.

The way we went about choosing those stocks were -- If you look over here, you can see the criteria we kind of used. If it's been included in a stock assessment before, obviously it probably needs to go into this list. If it was a representative of a complex, then we chose a representative, at least, from each of the complexes, and then also if it was an FSSI species, we figured we should include it, and so that got us to this magic thirty-one species that are now in the tool. As I said, this can be expanded to include more species, once we get this kind of fleshed out, the tool fleshed out, and up and running.

There is obviously assessment data that goes in, and that's sort of just quickly summarized. I will show you that there's a lot of information that goes into this. There is other data. Not all of this is being used. Some of this, we just assimilated because it was easy to put it all on one sheet, but, essentially, what we're trying to get to is -- If you look along these tabs, you will see that I have a tab that essentially corresponds to each of those categories. I have one for recreational importance, one for subsistence, constituent demand, all of these things where we actually need expert opinion

to weigh in on each stock, basically to determine what is the, for instance, recreational importance? How are we going to determine the recreational importance for each of these stocks?

I proposed three metrics here, and you can open this up for yourself and look at them. A couple of ideas that I had are shown here, but we're definitely not sold on any one of these. It's open for discussion, but the idea is that we're going to have to get input from the SSC on a bunch of these things, and so I think the idea is to possibly hold a workshop or two, and I don't know how many would be needed, of either all or some subset of SSC representatives, as well as maybe some other assessment folks, to just basically sit down and score all of these factors for all of these species, because that's what is going to be required.

Hopefully, once we get that scoring done, then this starts to become a very automatic tool, and so that all feeds into -- If you want to see the gory details, you go to the stock scores calculation sheet. What you will see there is all of these factors being fed into basically a giant matrix --

DR. REICHERT: Erik, can you remind us of the tab that you just --

DR. WILLIAMS: Yes, this tab is Stock Scores Calculations, and that is the gory details. That's where you can see all of the calculations that go in here. For instance, commercial fishery importance is taking the log of the data divided by the log times five. That's straight out of the technical memo on how to compute that importance factor.

Recreational importance, I just happened to use one of those metrics that I came up with, but, again, that's open for discussion on what type of metric we might want to use to determine recreational importance. Subsistence, I set to zero, because we don't really have a subsistence importance in our region. We can probably just drop that category altogether, potentially.

You can see that, essentially, up top here, the ones that I've highlighted in yellow here, which kind of show up as orange on that screen, are the categories where we need expert input. What I've done is set this spreadsheet up so that hopefully it's easy to provide that expert input. For instance, the constituent demand, this is a factor that is supposed to represent stocks that have a particularly high constituent demand, and it has a range of score from zero to five. It doesn't necessarily have to do that, but that's sort of dictated in the technical memo.

What I've done is set it up so that you can actually just slide -- You can go down the list and slide these little toolbars and give them whatever importance you want, or we can use some metric, or you can do it by the Delphi method, however you want, but, basically, we're going to have to basically go down and score each of these stocks on constituent demand, and we're going to have to do this for all these factors.

Once you've done that, as I said, that goes into this score sheet and gets adjusted. If you go to the very first tab, which is the results, which I just broke it, but the point is this will obviously be -- This is not the final tool. It is just a way to start exploring how we're going to do these weightings. Basically, what this exercise -- We're at the point now where the part we need for this exercise, to get to the point where we're going to have a final stock prioritization tool, is input from the SSC, lots of input, lots of expert advice on how we're going to score all of these factors for each of these species.

Once we get to that, the final step then is going to be where the council then actually weighs the factors themselves, and that's what this sheet is showing, is where the council can actually then weigh the factors and how important they want those to be. That will then result in this final broken score.

I guess the point of this presentation is to say that where we are now is we have a list of stocks that we want to work with. We have some proposed methods for how to compute some of these important scores, but then there's others that we just need to get more feedback on and fill those in for all these species, and then that will get us to the next step, which is then hopefully have that assimilated, so that we can then go to the council and say, okay, we have the factor scores. Now you tell us how you want to weight these factors, and then that should then end up with a final calculation of the priority stock assessments.

DR. BARBIERI: Thank you for that nice summary, Erik.

DR. SERCHUK: Clearly there's a lot of activity involved with filling out this matrix or going through this exercise, and I think you have to go through it to grow through it. On the other hand, I think there should be some economies of scale, at least recognizing that the service has put this forward and that all the councils and all the SSCs are dealing with it, and probably in their own way.

They're all struggling, and some may be more advanced than others. I suggest that, as we go forward with it, if there is a Council Chairs Meeting coming up, that the chairs discuss among themselves the utility of this tool and how well it's working, how it could work better, how they can draw upon each other's experiences, and as well as if there's a National SSC Workshop, and I know that there were such workshops in the past, to see whether, from a technical point of view, we can glean from each other's experiences, because there's no sense in any one council or any one SSC reinventing the wheel that someone else has found wobbles.

I think we should go through it, but I think we should draw upon the greater expertise that's out there by everybody going through this exercise independently and seeing whether there is some commonalities that can be useful. Thank you.

DR. WILLIAMS: That's a great idea, except we're one of the first councils to go through it, and so that's where we are and that's why this is -- We are the guinea pigs, in a sense. The Pacific Fisheries Management Council is, I think, at about the same stage that we are. They've had this tool put together and presented to them, and so it's us and the Pacific Fishery Management Council that -- We're the guinea pigs.

DR. SERCHUK: I understand that, but, presumably, Rick has made this presentation at every one of the councils, and so it's a matter of some people are going to be starting later and some people are starting earlier. If it's not done this year, certainly next year, when more of the councils come onboard and more of the SSCs come onboard, but I'm just thinking that this is plan on paper. After all is said and done, it will probably need some modifications. To the extent that it can be built better or some modifications can be brought to it, based on other people's experience, we take advantage of that, that's all. Thank you.

DR. BARBIERI: Just to that point, Fred, I agree with you, but one of the things that I like about this is the fact that it's developed now to the point that it can be road tested, and I think this is a big advantage. You can play with it, you can adjust things, and test it, and it's so much easier for folks to then understand how it works and be able to provide objective -- I think.

DR. SERCHUK: With all due deference, Mr. Chair, and maybe this is my background. Maybe it's because of the part of the country that I've spent most of my experience with, but priorities are priorities, irrespective of metrics. Someone will come in and say this is what we need, and there's a human element to all of this, and so my eyes have been open for a long time about this. Thank you.

DR. BARBIERI: Yes, good point.

DR. SHAROV: This is just thinking out loud. It's official the fisheries science has moved into artificial intelligence. I think what we're trying here is a very novel attempt, and it certainly would be an absolutely objective and clear and transparent method of describing as to how, in the end, we came up with the priority that we will end up with, but this is being a little bit maybe critical or cynical here, but having weight factors, in the end, where the customer or the council will be able to weigh in, how much different the end priorities would be from what we have now, because -- Seriously, that is the semi-chaotic process that we're going through currently. It is essentially just a less formalized, subjective/objective, weighing of all the same factors, which, in the end, brings us to very specific species that are at the top of the priority and, no matter how we will reshuffle, we will probably end up with the same answer. I am in support of this exercise, but, yet, I indeed wonder how much different the result would be.

DR. WILLIAMS: Yes, and I think that's -- It's actually we're intentionally setting it up that way, so that people can configure it in the way that it matches their expectations, but I think the advantage here is that it's forcing us all to then recognize how that decision is made. It spells it out a little more clearly, and then, hopefully, over time, it forces a little bit of consistency in that decision-making process, maybe just a minor, because, again, it can be adjusted every year, and, yes, there's no saying that, okay, here's your priority list and these will be the stocks you're going to assess.

No, that's the intent of this, but I think the intent is to just give us, again, a useful metric that can be used in that decision-making process that has been thoroughly thought out and is a metric that I hope the managers would find useful, because they contributed to it, and so it's their own metric, in a way, and they can look at it each year and decide what they're going to do with it in the end.

MS. LANGE: I think it could be very useful if our constituents, referring to them as the council members, look at it and really put the thought into it that can be done that they should, but, more important, the council's constituents, the stakeholders, I think it would give -- If the councils buy into it, I think it will be a very useful tool for the stakeholders, because they will see it -- It's all out there in black-and-white.

Everyone has had a chance to put in their things, and it comes out with a very specific ranking, a very specific number, so that hopefully we're not doing what used to be called benchmarks for the same stocks over and over again, that there are other species that are brought with an actual

quantitative value that they need to be assessed as well. Again, it's going to be a long, iterative process, but I think it's good that the process is starting.

DR. BARBIERI: Thank you for that, Anne. Any other comments or questions for Erik? Erik, thanks again for coming over and giving us this overview. Hopefully we will be able to provide you some good comments as part of our report. Unless there are any other comments or questions from the committee regarding today's topics, I think we are ready to adjourn for the day. Our agenda calls for 8:30 tomorrow morning, and we are going to start with Agenda Item 11, SEDAR 41, Presentations on the Review of Red Snapper and Gray Triggerfish Assessments. We will recess.

The Scientific and Statistical Committee of the South Atlantic Fishery Management Council reconvened at the Town and Country Inn, Charleston, South Carolina, May 4, 2016, and was called to order at 8:30 o'clock a.m. by Chairman Luiz Barbieri.

DR. BARBIERI: Good morning, everybody, and welcome to day two of the May 2016 South Atlantic Council SSC Meeting. We're going to continue going through our agenda. We have, today, two SSC members joining us through webinar. Anne Lange had to stay home today. She has a health issue with a family member that she had to tend to, but she is able to join us by webinar, and we continue to have Brian Irwin on the webinar today as well, and Mike Errigo just confirmed that they are both already logged in and ready to go.

We will start this morning, as we discussed at the end of the day yesterday, we're going to start with Agenda Item Number 11, Red Snapper Assessment Review. As part of SEDAR 41, there were two assessments, red snapper and gray triggerfish, and it turns out that the gray triggerfish had some data issues and some problems that couldn't be resolved and that assessment could not be completed as expected, and we will be discussing that after we discuss red snapper. We will discuss all of that in more detail.

Attachment 13a and 13b give you the SEDAR 41 stock assessment report for red snapper as well as some supplemental projections that were provided by the Center, and I will bring your attention to Item 11.4, our list of action items, which I am going to go through explicitly, because this is a very important assessment, and I want to make sure that our notes and our reports completely capture the range of topics that we are being asked to weigh in on.

Review assessment, does the assessment address the terms of reference to the SSC's satisfaction? Does the assessment represent best scientific information available? Does the assessment provide an adequate basis for determining stock status and supporting fishing level recommendations?

Another item has to do with identify and discuss assessment uncertainties. Are key uncertainties identified, and, if not, indicate additional uncertainties. Are risks and consequences of uncertainties identified and evaluated? Are methods of addressing uncertainty consistent with SSC expectations? List and comment on the effects of those uncertainties that most contribute to risk and impact status determinations and future yield predictions. Provide fishing level recommendations.

Apply the ABC control rule and complete the fishing level recommendations table, like we usually have for each one of our assessments. Provide advice on monitoring the stock until the next

assessment. What indicators or metrics should the council monitor and the SSC evaluate to keep tabs on the stock until the next assessment? Is there a recommended trigger level for these metrics?

Provide research recommendations and guidance on the next assessment. Review the included research recommendations, and those are listed for you at the end of the review workshop report, and indicate those which are most likely to reduce risk and uncertainty in the next assessment. Provide any additional research recommendations the SSC believes will improve future stock assessments, and, finally, provide guidance on the next assessment, addressing its timing and the assessment type.

With that, I am going to invite Dr. Kate Siegfried to join us here at the table and go through our assessment report presentation. Following Kate's presentation, after the committee has the opportunity to ask some questions, I have a few slides that I put together that summarize all the main points for us, to sort of guide our discussion through the outcome of the review panel report.

DR. SIEGFRIED: Thank you, Mr. Chairman. I'm going to try to speak right into the microphone, especially for those on the webinar, because I always have a hard time hearing. Good morning, everybody. This is a really long presentation. I wanted everybody to see what the reviewers saw.

Here is an outline for you. I hope you all have this available on your own computers to scroll through at your own speed, but I have the outline here for you. I'm going to go through the review of the data, some supplementary analyses, including the surplus production model, the catch-at-age model, which was reviewed thoroughly by the CIEs in March, and then the projections that we provided at the review and then the subsequent projections we can discuss, if the SSC desires.

First, our stock definition is a coast-wide stock. The Gulf conducts their own assessment, but here is the distribution. The red indicates a more dense population and the yellow is less dense. We're focusing on the four Southeast states, South Atlantic states.

For consideration for time blocking, we looked at the regulations and jurisdiction of the council. In 1983, there was a twelve-inch total length size limit implemented. In 1992, there was a twenty-inch size limit. Then, in 2010, a moratorium was put into place with intermittent mini-seasons, during which there was no size limit. You're probably all intimately familiar with this, but 2012 had two three-day weekends, 2013 had one three-day weekend, and 2014 was two three-day weekends and one two-day weekend. As you can imagine, this poses challenges for a stock assessment.

The next part of the presentation covers the life history data we had, and please slow me down if I go too fast. We looked at all of the data provided by the life history working group and decided on three growth curves. First, the population growth curve, which incorporated all the data available. Then we had a twenty-inch growth curve. This comprises fishery samples taken during the twenty-inch size limit time block.

Then the last one was a fishery growth curve, which is all of the samples taken from the fisheries, but outside of the twenty-inch size limit. Those growth curves were estimated external to the model and used as input, allowing variation around the mean, and so here's the means plotted. The green is the one of most interest. That's the twenty-inch size limit. You will notice, for the

earlier ages, that there is a larger average size, and that would seem appropriate, given there was a minimum size limit implemented in the fishery.

The next life history topic was natural mortality. We used a new estimation method for SEDAR 41. We actually did this for both species. A paper in 2013 was put out as a meta-analysis by Charnov et al. and then another meta-analysis by Then, et al., which incorporated Hoenig's work. He's a coauthor on the paper. That was put out in 2014.

We used this method. It's an age-based method. It shows age-dependent natural mortality, but it's not time variant. We used the Then, et al. estimate to scale it to that asymptote of age fifty-one, and so what I've plotted here for you, SEDAR 41 is the blue, SEDAR 24 is the red, and so there is a substantial increase in the natural mortality used in this assessment, as opposed to the last one.

The next life history topic that I want to cover is reproduction. We did change the way we model reproduction in this assessment as well. We still used a 50:50 sex ratio and a logistic model for female maturity. The spawning season was April through October, and the peak is in mid-summer, but we used an age-specific number of batches and batch fecundity, and that's the difference from the last benchmark. The spawning biomass in this assessment is modeled as population fecundity, the number of eggs rather than the mass of the mature female gonads. On the right here, I have just the maturity at age of females for you.

The next topic is discard mortality. This was a point of contention for the last benchmark. We put together -- Well, I wasn't part of the putting together, but I was part of the group, the ad hoc working group, for discard mortality, and we looked at pre-regulation and post-regulation, and this is the circle hook regulation, in both sectors, the recreational and the commercial sector. The group came up with a number, a sort of average, for j-hooks versus circle hooks and then a range, so that we could incorporate that range into our uncertainty analysis for both fishery sectors. The two time blocks we used for discard mortality were pre-2011 for commercial and pre-2007 for recreational, and we used this to calculate dead discards.

The next topic I wanted to cover is the removals that we were provided by the data workshop. They discussed hand line and diving landings at the data workshop. However, the plenary decision was to lump those. They were separated for the last assessment, but because diving made up such a small proportion of the total commercial landings, a little less than 7 percent, but the biological samples would not be lumped, because we didn't think that the compositions from the diving fleet was going to be similar to the hand line fleet.

The complete commercial landings start in 1950. Before 1950, the majority of the data are imputed, and the group, the commercial group, as well as the plenary decision, was that the best data were available from 1950 on. The discards are available beginning in 1992, and those estimates are generated using a discarding rate from 2002 to 2009, to inform that previous period, and they assume negligible discarding, due to that twelve-inch size limit in 1983, and so they weren't back calculated to 1983. They just started in 1992, when the twenty-inch size limit was implemented. The plot here shows by year. This is in number, so that they're on the same scale. The discards are in red and the commercial landings, in number, are in blue.

The recreational fleets, we were provided landings and discards for headboat, charter boat, and private boat modes from 1981 to the present, and then the historical landings were estimated by the recreational group, using a census method. They were not split out by mode, and so we were going to need to look to see how to group these fleets and whether we were going to need to model selectivity separately.

The differences that we saw were in the depth fished between the headboat and then all the other MRIP modes. What you see on the bottom right of the slide is the proportion of the samples taken by each fleet color, and so the green is commercial, and see that average depth fished is substantially different from the recreational fleets, but the main thing to see here is this red is the headboat, and there is very little fishing in the deeper depths, as opposed to the rest of the MRIP sampling, and so what we saw was that mode of fishing, that modal depth of fishing, is lower for headboat.

We wanted to look into this more, to see if we needed to separate those out, and so we looked at the length compositions provided. The blue is the -- Let me walk you through what this is. It starts in the bottom left-hand corner with 1981 and it goes 1982, 1983, and upwards to 1989 in the upper right-hand corner. This is length on the X and just proportion on the Y-axis, and we have the charter boat in blue, the recreational discards in pink, when they show up, the private boat mode in red, and the headboat mode in green.

We don't have a lot of coverage early on with discards, and we really don't have a lot of good charter boat data, and so here, we can't really tell yet. This is 1990 through 1998. We still don't have a lot of coverage. We see, in 2005, we start to get better data on the recreational discards, but we don't see substantial differences, reliably, between charter boat and headboat until we get to the mini-season period.

Here is 2008/2009, and we see overlapping of the private boat mode, the headboat mode, and the charter boat mode, and then the discard is smaller, as we would expect with the size limit, but then in the mini-seasons, and basically that's what this is, we see this divergence of the headboat and other MRIP modes. The charter boat and the private boat mode seem to be capturing larger individuals than the headboat mode does in the mini-season, and so we thought we needed to separate the headboat from the rest of the MRIP modes.

This just illustrates that there's a difference with the mini-season. This is by year and by age. These are nominal age comps, and we see that these three years of mini-season comps that we have are substantially different in the modes of each of the fleets back in time, and so this backed up using a separate time block for the moratorium and mini-season time period, as well as separating out the recreational fleets.

What we decided to do, as a panel, was to use the MRIP charter boat and private boat modes as one fleet and then let the headboats stand alone. That way, we could apply different selectivities to those two fleets during the moratorium time period.

The recreational landings that we have, like I said, before they were separated into sort of the estimated or back-calculated historical time period, that goes up to 1981. It starts in 1955, and that was not separated out by mode. The recreational landings by mode during the non-estimated time period, or the non-historical time period, are here on the right. Headboats are in blue and the other

MRIP modes are in red. The only time that we saw divergence was in that moratorium time period, and so we could separate out these historical recreational landings into those two modes without any concern about selectivity.

As far as discards go, there were a few zeroes in the time series that we were provided by the recreational working group, in 1982, 1986, and 1990. Those zeroes seemed unlikely to be true zeroes, given that the surrounding years' values were so high. There were -- Here, it was something like 80,000, and here it was 110,000, and it was zero in between.

The assessment panel agreed that it seemed unlikely that those were zero, and especially since no regulation occurred to cause a change that would lead to zero discarding. It seemed an estimation issue within MRIP, and so the years with zeroes, we decided to -- A number of options, and the one that we came to was to calculate the average of the year before and after the zero and then apply a third of that average for each of those three years.

We deemed this most appropriate, so that we wouldn't lose that year, or so that we wouldn't create data, but we would lose that year-to-year variation, and so weren't going to see the -- We're not saying that we think that it goes from 80,000 to zero to 110,000 as being accurate, but we didn't want to create landings that weren't provided by the recreational working group, and so these three years were averaged, and the average of those three years was provided for each of those years to the model.

The next data component I have is about composition data. We did the weighting a little differently for this benchmark. We used a thirty-fish minimum per region. The two regions that we have are the Carolinas and the Florida/Georgia region. We did this annually for length comps, and then we used a ten-fish cutoff annually for age comps. The reviewers did comment that that was a really tiny sample size compared to other assessments they've seen, but the composition data in the South Atlantic is very sparse.

These minimums prevent a very small comp sample size, in our relative sense, of a few samples to be scaled up to a large value of landings. This especially would occur in the Florida/Georgia region. If we had issues with this, additional minimum trip numbers would be explored during model specification, which I will show you later, and we used compositions from 1978 to the present, because we didn't have a lot of reason to believe that the sampling design pre-1978 was representative of the way that the fish were being caught by region.

This is a little bit hard to see. This is why it's good if you have it on your screen, but the sample sizes here are provided. This is also in the workshop report or the stock assessment report. We have it separated out for the length comps and the age comps by mode. For such an important species, it's a really small composition sample size.

DR. BARBIERI: Katie, let me just interrupt you for a second to give the committee the opportunity, just to let you know, that if you have questions -- This is probably such a long presentation that has a lot of details, and it's going to be difficult, perhaps, for folks to remember to go back and forth, and so if something comes up and if you have a question for Katie, if you don't mind, Katie, then while the slide is up there and discussion is taking place, ask your questions then. Thank you, Katie.

DR. SERCHUK: I have a question on the table that you put up on discard mortality. I'm just wondering -- I know this is way back, but you gave a pre-regulation and post-regulation. You also gave ranges associated with it. I'm wondering whether the range you gave for the commercial in post-regulation is the right range or if there's a typo, because all the other ranges include the mean value, presumably the average value. The range for the post-regulation on the commercial includes the upper value. In other words, you have a post-regulation of 38 percent and you have a range from 28 percent to 38 percent. Is that really the way it was performed?

DR. SIEGFRIED: No, I can find it for you. It's a typo in my presentation.

DR. SERCHUK: Thank you. I apologize.

DR. SIEGFRIED: It should be 28 to 48. Chip was at our discard mortality ad hoc working group and just told me that. Any other questions? Okay. This came up during the review workshop, as far as whether the headboat observer data were representative of the headboat fishery, and this is just for your illustration for discussions later. This is also in the report from the data workshop. This is the number of red snapper positive trips that were reported for the headboat program versus the number of at-sea observer trips positive for red snapper. It provides it by state and then for the whole South Atlantic.

The point here is that the sampling has been pretty consistent for the at-sea observer program, before and after the mini-season, and it was just requested because of the way that we use the headboat discard index, and so you have this in your presentation.

Modifications to the data made during the assessment workshop is something I wanted to cover. Each time we write these reports, we want to make sure that our thought process is followed. There were compositions, both length and age compositions, provided by the data workshop. However, the assessment panel reviewed all that data and perceived inconsistencies between the age and length compositions.

The length compositions were thought to be perhaps just adding noise to the model, and we're using this age-structured model, and we have high confidence in the age determination for this species. It's a very large otolith that's pulled from red snapper. There is aging validation underway and there is high reader agreement, and so the panel thought we should move forward removing all the length compositions after 1992, except for any discard length compositions, because we didn't have any other way to estimate selectivity for the discards.

We also had a review workshop modification. The review was pretty intensive. We had everybody at the table sort of looking at the data to sort of make sure that what we had there was all correct, and there was an error found in the age compositions for the chevron trap. The ones that were provided were based on annulus counts instead of calendar age. For calendar age, the life history group thought that the edge type and period of annulus formation are taken into account using calendar age, and that's what they recommended. However, it was oversight, and we were provided with annulus count, and so this correction was done at the review workshop, in the model rerun.

On the right here, what you have is what the correction looks like. The revised compositions provided are in blue and the previous compositions are in green, and I have -- It's five years of

composition data for the chevron trap. What the error had given us is a slightly younger age composition for the trap, particularly in 2011.

DR. SERCHUK: What is the perceived inconsistency between the age and the length compositions? I am unclear what that means. Could you explain it a little bit?

DR. SIEGFRIED: When I was in the assessment workshop, when I was trying to get the selectivities to estimate well, I would have to down rate the length comps in order to get the selectivity to fit well, as the age-based selectivity, and it seemed like it was sort of pushing back on what I was trying to do with the selectivities. We couldn't see any movement, age or size compositions moving through. We didn't see strong signal. It was a very jagged-looking length compositions, and when I removed them, especially for headboat, I was able to get a nice clean estimate of selectivity with just age compositions included.

DR. SERCHUK: But is it correct that the age compositions came from the length frequency samples that were collected to get the age compositions? How are the age compositions derived?

DR. SIEGFRIED: The age compositions, they're not the same -- The length compositions are not the same fish that were aged. That was the problem with the chevron trap. That was very specific from the MARMAP group, was these are exactly the same fish for the length and ages, and so you can't use both, because it's sort of double-counting those, but the headboat, we seem to just not be getting a good distribution of lengths to inform the model, and they were different fish, but it just didn't seem to be giving us any signal. There wasn't any sort of movement of sizes through year-by-year, like we saw in the ages.

It was recommended, for SEDAR 24, to not use length comps when age comps were available, and so that was something that the assessment panel discussed, but we did try it with the length comps in it first, because there's disagreement about that in the scientific community. Is there any questions about the data correction that was done at the review workshop? Okay.

We had this data correction, and we finished out the review workshop, and we held a subsequent webinar to finish up, because we had to rerun all of the uncertainty analysis and everything with this data included, but we did -- The changes that were made at the review workshop here seem to have little effect on the overall base model, and so we were able to continue on with the review workshop, unlike what happened with gray triggerfish.

Then I wanted to cover the indices of abundance that the data workshop provided. We were recommended three fishery-dependent indices of relative abundance: the headboat logbook data, the headboat discards, and then the commercial hand line logbooks.

The logbook indices were truncated in 2009, because of the change in fisher behavior due to the moratorium, and if they're changing where they fish and if they're changing how they fish, then that's not going to be consistent from year to year, and so those dependent indices, from logbooks at least, would not be consistent across that regulatory change.

Then we were given one fishery-independent -- Actually, we were given two fishery-independent indices of abundance, but we combined them at the assessment workshop. We had a chevron trap and a video index. The chevron trap and video indices separately were repetitive for red snapper,

because the video cameras are mounted on the traps, and so that means they're indexing the population at the same time in the same place on the same fish, and so there is efforts underway to incorporate both sources of data, perhaps in a hierarchical way.

There's a lot of discussion about that, to provide one index of abundance in the future, but, for now, what we did is combine them using the Conn Method. This is a published paper put out in 2009, and it's a hierarchical way to combine indices of abundance. The main effect of this is just that it adds the error that comes from process error, but, if you look here, what I have is all of them plotted.

The purple and the green are the individual indices that we combined and then the orange is the combined, and you'll see that it's just right in between. It looks like it's just averaged, but the CVs that come out of the Conn method are a little larger, due to that process error. This is the illustration of all of our indices of abundance by year, on a relative scale. We have headboat being the longest, our hand line index is in lighter blue, and the red is the headboat discards.

We also had an additional recommendation at the assessment workshop. The CVs of fishery-dependent indices are not really thought to reflect true variation in abundance in the way they're standardized now, and so the GLM has produced a CV, but that CV is usually very small, because it's based on the sample size, and, for dependent indices, it's a very large sample size compared to independent indices, and so they seem to be more certain if you just look at CVs. However, we didn't think that was true, and this published paper doesn't think that's true either, and so we have fixed the CVs to this literature value from Francis et al. in 2003 to 0.2 to start off, and then we further changed that, based on the iterative reweighting.

Here is a complete picture of data availability with the regulatory periods thrown in there, and so, by year on the top here. This is our data source here, and we have landings -- I tried to keep the color scheme consistent, and so commercial is blue, headboat is green, the MRIP -- I have them separated in the charter boat and private, but I put MRIP in red. Then we have discards in the same colors and length comps, age comps, and indices. We have our independent data in orange.

This first time block has no size regulations. This next one is the twelve-inch total length size limit, twenty-inch size limit, and then the last is the moratorium. What I have also done is highlight, in yellow, any place where is reconstructed data, very low sample sizes, or an uneven sampling design.

We see some of that still in the moratorium period, because it's very difficult to sample a fishery that's under a moratorium or a mini-season, but the states really came through and helped with that in the last three years, modeled in the assessment. Are there any questions about data before I go through the modeling approach?

DR. SERCHUK: Going back, four of the fishery-independent methods, the video, were you able to calculate error bars around the annual limits, the annual means? I am trying to find out whether is actually a trend in the data on the chart that you talked about all indices or whether we're talking about a large variability, interannual variability. It's not clear, because there are no error bars around that.

DR. SIEGFRIED: There is error bars provided in the data workshop report. I can pull that up, but it's the document provided to the index working group, but I just didn't plot it here.

DR. SERCHUK: Okay, but are they are large or are small? Are they tight or are they wide?

DR. SIEGFRIED: They're not large enough that there's not an obvious uptick at the end. I mean the CVID index that we used -- This last data point is not encompassed by the error bars of the first data point, and so there is an uptick trend, what we see. I mean it's definitely increasing from 2010.

DR. SERCHUK: That was my point. I was trying to interpret whether that was a trend or whether that was large interannual variability.

DR. SIEGFRIED: I appreciate that. I didn't plot those here, and if you want visual confirmation if that, I can pull up the data workshop report.

DR. SERCHUK: I believe you. If you say it is, it is. Just, typically, when you are able to do it, if you put it on visually, you can see how striking it is that the error bars overlap.

DR. SIEGFRIED: When I show the actual assessment results, you can see, with the likelihood weighting, what those error bars are. You will be able to see that in a little bit.

DR. REICHERT: I was just going to say that it's page 600 on the PDF of the assessment report. It gives the error bars.

MR. HARTIG: I think the problem I've had, reading through this assessment, was you have the indices and then you have the age comps and then there's been a lot of focus in this assessment on the age comps from the chevron trap survey, and so my question is were the data that were collected in the mini-seasons appropriate and were the data from FMRI used in this assessment? I mean I can't find where they were used, and I mean my question is were they used in the assessment and how did they figure into the age structure of the age comps of the assessment?

DR. SIEGFRIED: Are you just talking about comps, or are you talking about all data that they provided, because all of the data that they provided were essential to our removals timeline. We also had -- I mean Marcel can speak to this, as far as what the life history group took in from the Florida data in particular. It was used in the calculation of the age and growth, the von Bertalanffy curve, and then we do have some of that data represented in the recreational age comps that I showed earlier here, and so anything that's recreational private, recreational charter boat, that's all Florida. We don't have -- I mean we didn't get that from MRIP. That's all the state samples.

MR. HARTIG: Okay. I'm clear on that. Just one thing that would have been helpful, to me, would have been some kind of an age structure that showed everything put together from the entire assessment. What is the age structure of the population, based on everything we know about age? I don't see that in the assessment. Do you see what I'm saying? Combine everything together into one age structure for the population that we know now. What was the age structure that came out of the assessment from all the data that we have? That, I haven't been able to find, and that was a problem for me in trying to figure out where the focus was.

DR. BARBIERI: Ben, Figure 40 in the assessment report has all the age compositions for the different time periods relative to the equilibrium one, and so it would be Figure 40 in the assessment report.

DR. SIEGFRIED: I will go through all of that in detail when I show the assessment results, but the Florida data are used. We worked in person at the data workshop and tirelessly by email and on the phone in trying to make sure that we were getting it all down to twenty-inch versus all of it versus which ones are age comps and which ones are included for removals, because this assessment wouldn't have been able to have been done without those state data. The MRIP data were so lacking, because, as you know, the way that they sample is not sufficient if you have a mini-season or a moratorium, and so it wouldn't have possible to do this without the state data.

DR. BARBIERI: Ben, if you want to look at that up, it's page 630 on the PDF.

DR. ERRIGO: I just wanted to ask, for the discards in the mini-season, the moratorium period, I assume the discards outside of the mini-seasons are from MRIP. In the mini season, were there discard estimates from like Florida, their special sampling, that were used, or was it just MRIP discards for the whole time period? I just wasn't sure.

DR. SIEGFRIED: There is a decision tree that was put together at the data workshop that looked to see which of those data sources was most comprehensive for the mode, and so if MRIP is operating in that mode, but it's only two or three-day weekends, do we think that if MRIP sampled during that two-month mode, did they capture all the discards, or did the state capture it in those six days?

In cooperation with the state partners in that working group, and Erik Williams was part of putting that together each year, and he's here, but they would look at using this decision tree, whether the state data were less or more, and then whether it was comprehensive, because of the two-month mode, or it wasn't. Then everything outside of the mini-season mode was MRIP, because there wasn't state data outside, but that decision tree is available in all the documentation as well. Anything else before I start with the modeling approach? Okay.

We looked at a number of supplementary analyses and then a catch-at-age model. First, we looked at catch curves, and this was purely as a diagnostic tool for the mortalities used, both natural and fishing mortality, and the Beaufort Lab usually does multiple models to sort of check how each of the data sources influence our statistical catch-at-age models, and one of them that we used was a surplus production model called ASPIC.   Then we used our catch-age model, which is just a statistical catch-at-age model implemented in AD Model Builder, and that was meant to provide a more comprehensive stock status.

First, I will go over catch curves. This is a little bit rough, because, as you saw, the age data are a bit lacking for the full age composition of the species in each fleet. We have two estimators, the regression estimator and Chapman-Robson, and then we looked at the commercial hand line, the headboat, and then the general recreational, which is charter boat and private, and then SERFS, which is meant to describe the video and the chevron trap, although only chevron trap provides any compositions. We looked at mostly synthetic cohorts within year and then some limited data on the true cohort.

These are a bit busy, but what I have is each of these plots has the regression estimator in green and the Chapman-Robson in blue. It's by year, and this is Z. We see it's pretty noisy. This is for headboat synthetic age comps and headboat true age comps, but we have a pretty -- It's noisy, but we have a pretty large estimate of pre-1999. Then, barring this outlier, it's a little bit lower from 2005 on. Then, for hand line, we see it's a little less noisy and, in general, lower, and so we're looking at magnitude here. We want to get an idea of what Z is for each of these samples from the different fleets.

Here, we have MRIP. We have 0.5 to 1.5 for headboat and we have 0.25 to 0.75 for hand line, and then MRIP is sort of in between. It's a little less variable, 0.5 to 0.75 or so, and then SERFS is pretty consistently 0.5.

What we were looking for, and this is when we were still considering a first time period, but we wanted to separate it out as much as possible. This is the aggregated. This is the estimate including all ages, and we see headboat is, in general, just larger. What we're looking at here is if we see a large overall value in comparison with catch curves, it may be some evidence of doming in the selectivity, and we see, just in general, usually headboat is a little higher.

We wanted to, in general, show that headboat probably was domed. Looking at this, this is evidence for doming in headboat, but it's not a lot of support for anything else in the other fleets. Then this other supplementary analysis that we did is the ASPIC.

This is updated software and was used for -- This is the ASPIC software of Prager. This model, in general, is conditioned on yield. It's really a look at the rate of indices and the catch correspond. It's non-equilibrium logistic formulation. There is uncertainty from a bootstrap. There is no age structure in this. Recruitment variability is not included. There is no time-varying selectivity, and so all those time blocks aren't able to be implemented in this production model. There is no M, let alone age-specific M, and there is no age-specific contributions to population fecundity, and so all those data improvements we can't include in this, and so this is really meant to look at the catch versus the indices.

The way this was set up, this was done by Nikolai Klibansky in our lab. The commercial hand line, headboat, headboat discards, and CVID indices were used, as they were for the statistical catch-at-age model. The landings and the indices were incorporated the same as they were for the catch-at-age model. He did upweight the CVID index by three, which is something that happened during the assessment workshop.

The headboat discard index was lagged forward a year, and it used the extended CVID index, which goes back to 2005. What you have here at the bottom, these are the parameter estimates for each of these ASPIC runs that he found most informative, and so 318, Run 318, is the continuity case. That's from SEDAR 24. 319 is updated continuity, and so this is with the new catches, and so that shows you the importance of those new catches. 320 is the best configuration, which is meant to be most similar inputs as to what I was using for catches in the indices, and then the 323, that bottom one, is that best configuration, but with the B1 over K fixed, which is sort of the initial depletion.

There was some question as to whether R, the intrinsic rate of increase, was approximately correct, and it looked like it was sort of an average of a meta-analysis, and that should be in the

documentation, but, in general, what this shows is that the changes in catches seem to have a big effect just on the continuity for ASPIC.

Here, it shows the fits of ASPIC to each of the indices. This is commercial hand line, the headboat index, the at-sea, and the CVID index. With these production models, there is not a lot of wiggle room in it to do anything interannually, and so it just sort of fits an average through each of these indices. Then the removals are plotted there as well, in red or pink, whatever your vision says it is.

This shows the bootstrap estimates. The blue-shaded areas are the distributions from those 1,000 bootstrap runs. This is each of those benchmarks, the F over FMSY, B over BMSY, the R intrinsic rate of increase, the FMSY, the K, and the B1, and so the initial biomass, the carrying capacity, and the BMSY and then the initial depletion.

Each of these lines represents the best fitted parameter value, and then the dashed lines are sort of the 95 percent confidence interval from the bootstrapping. These are the benchmark time series that are output from the model, and so this does show a different status, what it showed for SEDAR 24, then what ASPIC estimated for SEDAR 24, and that's likely due to the difference in the index time series and the magnitude of the catches, which I'll go over in a bit.

MR. HARTIG: Katie, I'm going to ask this question now, because it came up in the combination of the chevron trap and the video indices and the way the selectivity of the ages were structured based on the chevron trap index. Was there any attempt to go back and look at the ages that were collected in other surveys and compare them to the chevron trap, to see if there was some possible population differences in the age structure within the chevron trap video index and the other information that was incorporated in the assessment?

DR. SIEGFRIED: Which other surveys are you talking about?

MR. HARTIG: I'm talking about the FWRI information and the mini-season information, the age structures for those combined, and then if you look at that with the trap index, there are some substantial differences that occur when you compare those two. One of the things the industry has brought forward is that the chevron trap index may not be giving you the age structure of the population.

The selectivity of the trap itself may not be collecting the number of older individuals in proportion to what they occur in the other data, and so that's -- The problem is magnified when you combine those two indices and you use the trap index for the age structure of the video index as well, and so that was my only point, that there is a difference between the age structure of the samples that were collected outside of the chevron trap indices, and I just wondered if you had looked at that in the assessment, to see if those were representative.

DR. SIEGFRIED: I thought you meant maybe another survey, like independent survey, but I'll be able to show that for you with the catch-at-age model output, but what the age comps look like from any of the MRIP age comps in that moratorium time period compared to anything we got from the chevron trap is the comparison that you're looking for, and we can look at that in detail when I pull it up, like what the distribution is, but comparing those is all that we can do. There is not some other survey that we haven't looked at.

When I go through the BAM results, then we will look at that in detail, because that's the only comparison that we can do that addresses your question, and we can look at the likelihood weighting, how those are different and why they might be different, and we can look at whether they are fit well.

MR. HARTIG: I appreciate that, and I will wait until we get there.

DR. BELCHER: If you combine the CVID and the chevron in the overall age model run, why would the CVID be handled independently going into the ASPIC model, is the first part of the question I have. Then, secondly, is there any background or discussion of why that one received a weighting of three?

DR. SIEGFRIED: To answer your last question first, it was left in from the assessment workshop, and there wasn't any need to get this model fit better or differently. We had talked about upweighting the CVID index for the BAM model at the assessment workshop, but then it turned out that we didn't need to once we made data alternations, like the length comp removals and things like that.

As for your first question, maybe I didn't say it clearly, but the CVID index was used the same way it was for BAM. It wasn't a chevron trap and CVID. CVID is the chevron trap mixed with the video index. They're combined, and that was used the same in both of these models. I can't see your face, and so I'm not sure if I answered your question.

DR. BARBIERI: Yes, and thank you, Katie.

DR. SIEGFRIED: Everybody is anxious to see this, because every time that I say that I'm going to show that in a minute, here it comes. Here is how we configured the catch-age model. Our start year was 1950, and that was based on the fact that first year of our reliable commercial landings was 1950. The next input began in 1955, and that was the historical recreational landings. SEDAR 24 started in 1955, I believe.

We used a prior with a mean of 0.03, which is informed by SEDAR 24, to estimate an initial F. We used that prior after much investigation of the effect of the prior on the estimate of initial F. That was all done during the assessment workshop.

We used three time blocks for the selectivity and for growth. The first time block was 1950 to 1992. There is a size regulation put in place at the end of 1983, but we did a lot of investigation into the length comp to see if that had an effect, and it didn't seem to have an effect, and so the panel recommended only one time block, 1950 to 1992, for that first time period. The second time block was 1992 through 2009, and that reflected that second size regulation, the twenty-inch size limit, and the third block is 2010 through the terminal year of the assessment, which is 2014. There weren't any size regulations during the mini-season, and all other fish are discarded outside of the mini season, due to the moratorium.

We iteratively reweighted the likelihood components, a la Francis 2011, and that's so that we could SDNRs, or the standard deviations, of our normalized residuals close to one. This provides some objective way to weight our data components. I think SEDAR 24 was the first time it was used

for red snapper, but, in the past, it was a subjective decision, and this really allows us to use the statistics instead of make judgments ad hoc.

We used a constant catchability. We have a plus group for our compositions that's set to age thirteen. We didn't have a lot of age comps over age thirteen. It was less than 5 percent of the data, but we did model ages one through twenty for the population dynamics, and twenty was used as a plus group. That's based on the saturation of our life history parameters.

We made several considerations for functions to describe selectivities, and we used a number of criteria to decide on those functions, and I will go through that discussion here. We looked at the depth fished of the fleet, we looked at the way the gear catch the fish, we looked at the age compositions we had available, we looked at the availability of each size class in the region the fleet fished, or the depth that they they fished, and then we looked at our catch curves.

The depth fished, we discussed this. First of all, we just wanted to see where the fleets were, and so this was actually provided for the industry for the last benchmark. We have the size of the fish, the depth fished in feet, the charter boat, commercial, headboat, and private boat. There just wasn't a lot of data for private, and this shows that commercial tend to catch, on average, a little bit larger, but also the range is a lot wider of the size of the fish caught. They also fish deeper. Headboat tend to fish exclusively, barring a few outliers, exclusively shallower. Charter boat seems like a combination of the two, somewhere in between, and private, there just wasn't a lot of data, but it seemed to be a shallower depth.

Then I showed this before, but the commercial here, this is depth, and the proportion of the samples taken at each depth, and this green shows the commercial, and they just tend to fish deeper. The mode of their fishing is deeper, and also the range is wider. Headboat tends to fish exclusively shallower.

The recreational charter boat we have here fishes deeper, potentially, although their mode is similar to headboat, and then we looked at mini-season samples. This is all from Florida, actually. This purple line shows that it matches pretty well, this charter boat depth fished, and so we wanted to see if the behavior in the charter boat during the recreational mini season was similar to previous years, and it looked like the depth fished, at least, was similar.

MR. HARTIG: This brings me to a question that I had earlier that I will go ahead and pose now, because this is pretty instructive for what I'm going to present. If you look at the discard mortality estimates from the AW report, the recreational discard mortality in 2009, you had the highest landings, the highest levels of discards. In 2011, you had the lowest. It was similar for the headboat, which is highest levels and then 2011 had the lowest levels, but then, if you look at commercial in 2011, where both the headboat and the recreational were the lowest, they had the highest level of discards.

Then, if you look at when they had the lowest levels for the recreational and for the headboat, it was just the opposite. I'm getting a little bit confused here, but, anyway, I see this -- This was instructive, and that other graph you showed with the depth fished on the bottom there. However, you would think there would be some -- There is some overlap in both the sizes and the depths fished, and you would think that there would be some relative combination of those that would give you a similar commercial level of discards in those years that are so high for the recreational

and the headboat. I just wondered if you guys had looked at that and had any questions about that as you went through the assessment.

DR. SIEGFRIED: Are you talking about the actual magnitude of the commercial discards versus recreational discards?

MR. HARTIG: Yes.

DR. SIEGFRIED: I think it might be two separate points, unless I misunderstood you. I think that they do overlap in where they fish, in the depth that I'm showing here, during part of the time that they're fishing, and so some of the commercial fishing here overlaps with some of the headboat fishing, and it overlaps quite a bit with charter boat fishing.

However, the commercial fishery, I mean they have really dropped off more than the recreational fishery, due to the moratorium, and so the rate of discarding is a separate issue from where they're discarding, and so the magnitude of that -- They're also covered better. I mean we get logbooks from them. It's harder to get the estimates of discarding from the recreational fishing than it is from the commercial guys.

I agree that they do tend to fish similarly, and so their discard mortality should overlap as well, and so I guess that's even three separate issues, and we do show that in our discard mortality estimates, that there is some overlap in our uncertainty range. A good point you brought up during the discard mortality discussion was the skill of the average commercial fisherman versus the average recreational fisherman in reducing discard mortality.

The depth fished is just one component of this discussion about selectivity. What we've shown here is headboat tends to fish the shallowest, commercial tends to fish the deepest, and that the other MRIP modes are somewhere in between. Also, commercial tends to capture the larger -- This doesn't necessarily mean oldest, but this just means larger individuals.

Then we moved our discussion on to what the gear selectivity would be, because that's just one component of overall selectivity. We did look at a literature review, and a lot of the literature were provided by a panel or industry or informed by the Gulf assessment. The hook type doesn't seem to have an effect on the selectivity, though it does on the discard mortality, and so there wasn't a need to have an additional time block for selectivity based on hook type, but we have the time block for discard mortality pre and post-circle hook requirement.

The hook size does matter, but we don't have hook size reported in the logbook, and so one thing that we had was public comment during the assessment panel, and it seems the fishermen would be willing to report this. It would be very, very useful for future assessments if we had hook size, the number and the manufacturer, because the size of the hook is not -- The number is not universal across manufacturers, and so we would be able to take the manufacturer and the hook size and get an idea of what the actual physical size of the hook is, if it was reported. They said they were willing to do that, and so that might be a research recommendation. In general, it's likely that hook sizes are smaller for headboat than for the commercial fleet, and that's the distinction that we could draw for the two ends of the spectrum.

Then we looked at the availability of size classes to the fleets. This is where the rest of the literature review came in. We have a paper written by Mitchell et al. This came from a cooperative research program project. This was I think the cryptic biomass project. The title of the paper is "Depth-Related Distribution of Post-Juvenile Red Snapper in Southeastern U.S. Atlantic Ocean Waters: Ontogenic Patterns and Implications for Management" and *Marine and Coastal Fisheries* is where it was published.

The points of this paper are that older, larger red snapper were generally distributed throughout all depths, whereas the younger and smaller red snapper occurred disproportionately in the shallower waters. Some species that we assess, the larger individuals stay in the deeper water. That doesn't seem to be the case for red snapper.

For Red Snapper equal to or larger than fifty centimeters fork length, they found no evidence of a positive relationship between depth and age or length. Then the age and length distributions greater than fifty centimeters fork length did not differ between the fishery-independent surveys and the commercial hook and line fishery.

MR. HARTIG: In the assessments for mutton snapper, black grouper, and red snapper in the Gulf, the age structure -- The largest and oldest animals come from commercial longline gear, in each of those assessments. You have a specific gear that fishes in deeper water, anchoring the age structure of the oldest animals for those three assessments.

Then we have this study that comes out and says that it doesn't happen in the South Atlantic. I don't know that that's particularly the case yet. I mean we don't have an age structure that's filled out enough yet to even tell if the older fish migrate to the deeper water with time. The overall observations from the fishery, and my individual observations are, that there are much larger, older fish in, relatively small numbers, in deeper water. Now, I don't know that they're older. I can't say that, because I didn't age them, but they are much bigger than anything that I see inshore.

Going forward into this assessment, I think if we don't have some way to sample a longline sample in those deeper waters, not just for red snapper, but for gag grouper and animals of that nature, where you see an ontogenetic shift in animals and the largest males staying in the deeper water, for gags in particular, that we're never going to really know the age structure of these animals in the future, and so it's just a plug for the Center to be able to be thinking about some way in the future to be able to sample those deeper areas and access those fish.

DR. BUCKEL: Katie, I guess, to Ben's earlier question about the comparison between the Florida fishery-dependent data and the SERFS data, this was SERFS versus longline data, but it was observers -- I guess it was fishery-dependent, but there was observers on the longline, and so was there a comparison made between the SERFS -- The total age structure and the longline data, to get at Ben's earlier question, I guess. I see you have here that it's greater than -- I'm not sure why it was truncated to the greater than fifty and not the whole -- If you had traps in the same area as the longline, that would be a nice way to get that selectivity of the traps, I would think, unless I'm missing something.

DR. SIEGFRIED: I would have to look in more detail of what you're saying, but I think the larger than fifty centimeters was trying to look at the larger, older individuals, as far as where they're distributed, and it was directly comparing the trap versus this commercial hook and line. It was a

cooperative effort for the fishermen to go out there and just find the oldest ones that you could find, the biggest, oldest ones that you could find, and they didn't find a difference between the traps and the hook and line.

DR. BUCKEL: Yes, I understand that point, but I was just -- Back to Ben, so that -- I understand the purpose of this study was set up for a different question, but it may be able to inform the earlier question that Ben had about a way to maybe empirically estimate the selectivity of the chevron trap.

DR. SIEGFRIED: One thing, to what Ben said before, was I think there is also a component in the Gulf to distance from shore fish, because of the way that the shelf is in the Gulf, and so the depth fished is pretty consistent out quite a ways, versus on the South Atlantic coast it's not, and so the depth fished by each fleet also depends on distance from shore in the Gulf. Isn't that true? I mean I didn't assess that species, but the time I had down there, the commercial fishermen would go farther offshore than the recreational guys would.

DR. BARBIERI: Fred, before I get to you, just, to this very point, just to clarify to you and Jeff, Ben, is that we found actually this same result. We had two separate CRP projects funded that looked at experimental drop hand lines, timed hand line drops, and this is exactly what we found, that the younger are oriented more towards the shallower areas, but the older ones seem to be across the whole range, and so that agrees with this result, just to point that out.

DR. SERCHUK: Based on the growth curves that you put up way back at the beginning, my interpretation is that a fifty-centimeter fish is around three years old, and is that correct, first?

DR. SIEGFRIED: I can pull it back up, but it can be, yes.

DR. SERCHUK: That's what the growth curve shows, the average growth curve, and so I'm a little bit taken aback when we start talking about old fish being three-plus for an animal that lives at least twenty years. I would seem, to me, that -- A know a three-year, again, according to the maturity information, those are all mostly mature fish, because they mature very early.

On the other hand, to talk about older fish that begins at age three, where you have at least seventeen age groups that you're considering, and you're cutting it off at age twenty, and they grow much larger than that, I think it's a little bit misleading. I wonder whether, had you done a different cutoff, instead of fifty centimeters, maybe sixty centimeters or seventy centimeters, whether you might have seen some differences. I am just saying, typically, for a long-lived fish to include ages three and above as an indicator of spatial segregation of age groups, normally I wouldn't have gone that way, and so have you looked at other cutoffs is my question.

DR. SIEGFRIED: I didn't do this work.

DR. SERCHUK: I'm not impugning you. Let me put it another way. Has the work been done?

DR. SIEGFRIED: I haven't seen this exact comparison done for sixty, seventy, eighty-centimeter fish, and we do tend to try -- I mean it's difficult in these sorts of meetings, but we try not to go back and forth between length and age. Like, for instance, here, this doesn't mean that they necessarily catch older fish just because they have larger ones, because of exactly what you're

talking about. We're trying to amass as much evidence as we can towards some idea of selectivity, but there isn't one source that gives us everything we need to know.

DR. SERCHUK: But you understand that there was a size regulation that went into effect that was on length and not age. Then you're presenting age information on an age-structured assessment, and then you're trying to look at differences in distribution of the animal, either by size or age, and I'm just trying to get a better understanding, because typically one would often see spatial segregation of animals, either by size or age, and you may have a very variable situation, but I am not convinced that looking at it simply by fifty centimeters and above is the most illuminating way to get at this issue. Thank you.

DR. SIEGFRIED: I agree. It's meant to be one part of this picture. I mean one of the things that we -- The reason an age-structured model is important here is the disproportionate effect of older individuals on reproduction. This is trying to get at this age-based selectivity, but we do need to model it based on age, because of what we found for the reproduction.

DR. SERCHUK: One other point, Mr. Chair, if I could. Another reason why I bring this up is one aspect that is not contained in the status determination criteria, when we start looking at BMSY and where we are in terms of the stock, is whether the age composition or the size composition of the stock -- You can actually be above a BMSY and not really have a very restored age composition or size composition to the stock, and so I think for animals that are long-lived, or moderately long-lived, and this certainly qualifies, that's another dimension.

Even though you may say we are not overfishing and the stock is not overfished, there may still need to be some attention paid to whether you have a population distribution in terms of sizes or ages that represents the natural potential of the population. Thank you.

DR. BARBIERI: To that point specifically, that same figure -- If you look at Figure 40 in the assessment report, where they do have -- Katie did point out what the equilibrium age composition is at the expected MSY or proxy value, and so basically we're trying to rebuild the population to that expected age composition at equilibrium. You're right that, explicitly, this is not taken into account in the reference points, per se, but it is integrated into that process.

DR. SIEGFRIED: I will talk to Mitchell et al. and ask them to do this for a research recommendation at different cutoffs. To Ben's point, actually, this would be an interesting study to redo when the age structure -- When we think we're closer to something where the age structure is filled out. I agree that that would be a very good study, to see if it changed. It would be research on the way of population dynamics change based on a rebuilding scenario. That would be a good study.

I'm going to talk more about the way we configured this model. Based on the depth fished that we saw, headboat being the shallowest and commercial being the deepest, based on the -- Hook size matters, but we don't have it in the logbooks. In general, we know that the size is smaller for headboat than for the commercial fleet. Then, based on the fact that the size distribution, at least, didn't seem like it varied by depth, we looked at these assumptions for selectivity, and so commercial hand line, the trap video, the commercial hand line discards for the two blocks without a size limit, and then the MRIP Block 3, we would do logistic. MRIP Block 3 came after -- That was not an initial assumption. That's a final configuration issue.

We attempted MRIP Block 3 as a dome-shaped selectivity to start, and the model kept wanting it to go logistic. After discussing this with the assessment panel and also doing another sensitivity for the review panel, it's still logistic. That seems to be because of the way that the MRIP modes, the charter boat and private boat mode, fished during the mini-seasons. It seems like they were selecting for larger and older individuals, which would make sense if they had a limited number that they could catch.

Then headboat, headboat discards, and then MRIP Block 2, which is subject to that twenty-inch size limit. MRIP discards and the commercial hand line discards during the size limit would all be dome-shaped. We tried multiple functions for a dome-shaped selectivity, including the double logistic exponential, but we came to this double logistic function.

The reason that we chose that is so that we didn't have to make an assumption of an age of full selectivity and that the model would estimate that. The logistic exponential requires us to fix that age. Then the double logistic was an unwieldy function that just we couldn't get it -- It's six parameters, and we just weren't able to get it to work for this many different dome-shaped selectivities. If you will keep in mind that we have ten selectivities for this model, we have three time blocks, we have three different growth curves. I mean it's a highly-parameterized model.

Our assumptions are that MRIP mirrors headboat in Block 1, because we didn't have any comps for MRIP in the early time period, and it seems appropriate, given the way that we've seen the fleets fish in any sort of compositions we did have, that would overlap in that first time period and that MRIP discards would also mirror headboat discards, because we didn't have comps for the MRIP discards, but we did for the headboat discards, because of the at-sea observer program.

Then the next thing we needed to decide was steepness, and so would we estimate it or would we not estimate it? We attempted to estimate it. We were able to estimate it, but the estimation was not stable, and it seemed to want to go to an upper bound of 0.99. We then ran a likelihood profile to see if we were just missing some likelihood minimum, but we were not. All of the profiles show that it's going to the upper bound.

The only thing we got from this is that we didn't think that steepness would be low, that we were missing something in the data that way, and we weren't able to estimate it. We saw that it wasn't low. The likelihood profile shows that we weren't missing anything, and so we'll go over what we did in a second.

We had this issue with selectivity, the dome-shaped selectivity for headboats in particular, and then MRIP as well, going to zero when we actually had age composition data that show that there was selectivity in these older age classes, and so the assessment panel recommended a plus group of age ten for headboat, meaning that we would fix the selectivity after age ten to what it was estimated for at age ten, to allow for a non-zero value of selectivity for older age classes. Because we have that comp data in dome-shaped selectivity, especially this many are just inherently difficult to estimate.

On our way to the base run, we fixed steepness at 0.99. That's consistent with what was done in the Gulf and what is being done more in the Northeast. The model estimates, when I was able to estimate steepness, were all high, 0.2, 0.95, 0.96, but they were not stable. It changed, any little value, and it would not converge.

This is not meant to say that there is perfect compensation in the stock and that we believe steepness is actually 0.99. What this is meant to do, as it is in the Gulf, is to model average recruitment with deviation, and so we're assuming an average level of recruitment with deviations, and that, mathematically, the steepness is 0.99. When I show you the recruitment residuals, you will see whether you agree with that. The assessment panel, the reviewers, seem to.

We fixed the headboat discards also, the selectivity, at age ten-plus. Then, when I did all of these data corrections, we fixed steepness, we removed the length comps, we were able to estimate the selectivities, no upweighting was needed. Upweighting was considered for the CVID index, because it's an independent index that we wanted to make sure that the trend was captured. It was a survey that was put together somewhat specifically to an index species like red snapper. It seemed like we were just weren't able to get the signal before, when we had the model configured as it was initially, and so, after all of these corrections to the assessment panel, we didn't need to upweight it any more. The base run fit the CVID index very well without any upweighting. The weighting just got all the SDNRs to near one.

We found an issue after we had our base run going. It looked like a potential instability in the selectivity parameters. Now, remember there is ten or so selectivities, and they are double logistic, which means that slight changes in the parameter values could result in almost identical double logistic curves.

Something that SS does is called jitter. It takes a very tiny value and moves the initial value that the model uses to estimate parameters and just jitters it a little. We ran like a mega jitter to address this problem, because we wanted to make sure that the starting values weren't causing a problem with our estimation.

It just wasn't finding the minimum in the likelihood surface, and so we call this our starting value analysis, and it was used to come up with our new configuration. We also changed the estimation phases, which is purely a computational issue, to come to a better solution. The phases are when each of the parameters estimated start to be pulled into the entire likelihood.

This mega jitter, for each estimated parameter, we would draw a random uniform value from a distribution that's plus or minus 25 percent of the current starting value. Then we would run 400 bootstraps and keep track of that and compare it to the total likelihood. We ran that multiple times. It took a lot of time, but it seemed to be worth it, because what we came up with was this is our 400 bootstraps. This is our total weighted likelihood. Remember that we want the minimum, and this run right here in red is our base run.

You can see there are multiple solutions to this model if we don't start with these correct starting values, and so it was sort of like our own optimization that we did. We did this in order to achieve that global minimum. It, incidentally, didn't have a big effect on the status that was put out, but it is more statistically sound, and we used these starting values in our base run.

DR. BARBIERI: Katie, I think you're going to be going into the description of the base run right now, and I think we've been here for a solid hour-and-a-half, and perhaps we should take a five-minute break and then we will come back. It seems like a good place for us to pause. Just try to make this really short, because we still have a lot to go through this morning, please.

DR. BARBIERI: Apologies for the short break, but we do have a lot to go through today, and we don't want to lose continuity in Katie's presentation. If everybody can please return to their seats, Katie can go through her base model, base run, description.

DR. SIEGFRIED: I am going to go ahead and go through the base run as it was configured by the end of the assessment panel contributions. The estimated parameters I have are the growth curve CV, and so the error around each of those three growth curves, the deviations around the initial age structure, and so those nineteen ages that have the deviations. The stock recruit parameters are zero and sigma-R. The steepness is fixed. I have the annual recruitment deviations, the forty selectivity parameters, four catchabilities for each of the indices, and then the fishing mortality by year by fleet and then our initial F.

We assumed constant CVs, at the recommendation of the data workshop, and we estimated one CV for each of our growth curves. This is one example, just to show you what that assumption looks like. As Fred was saying, there's just a lot of different ages that a certain size fish can be. There's a lot of variability in growth.

I am going to go through the fits to the data, and please stop me if you need further explanation. These are going to look like we just drew it right through the lines, because, for this type of model, we're fitting the landings and discards exactly. On the left, I've got commercial hand line landings. On the right, it's commercial hand line discards. We see the highest takes were in the late 1950s and late 1960s, and then it has declined. We had an increase in 2008 and 2009, and then this is the effect of the moratorium and then the mini-season catch.

The landings are in thousands of pounds and the discards are in number, thousands of dead fish. On the left is headboat landings and on the right is headboat discards. This here is just because the model didn't start until 1955. These look perfect, because they are the historical reconstruction. The data start in -- 1981 is the first year of our sort of data-rich period. We have no estimates until 1992 of discards, and then they slowly ramp up until the mid-2000s, when they reach their peak in 2008.

General recreational landings and discards, on the left are the landings and on the right are the discards. We have these two very high values in the mid-1980s, which we've talked about quite a bit. Otherwise, 2008 and 2009 were similar to the mid-1980s, and then we've seen this uptick in the moratorium mini-season take for general recreational. Here is the discards. The discards are also at as high a level in the terminal year as they were in the early 2000s.

I want to go through these composition fits slowly, to help address some of Ben's questions. First, we have the length comps. The line is the fit to the data. The purple circles are the data. N is the sample size provided to the model. The effective N is the sample size computed after the likelihood weighting. You can see the length comps for the commercial hand line are downweighted using the iterative reweighting and not that we did it arbitrarily.

These are going to all look similar. You have the proportion, the length bins. When it's ages, it will be the ages, but it's 200 millimeters to 1,000 millimeters, and it starts with the commercial hand line here in 1984 and goes through to 1992. In general, we're not seeing sort of a movement of a modal size class consistently moving through. If you've seen some of the Northwest or

Northeast stocks, they tend to see more of that moving through in the length comps, and these are the best-looking length comps we had.

Here is the length comps from the commercial hand line discards. You can see these are downweighted quite a bit, and they also are very noisy. Here is the during the moratorium period and then pre-moratorium. We only had two years reflected here, but it's really six years of pooled data, and so it's pre-moratorium and post-moratorium.

Here is the headboat discards. We just had trouble fitting these data in general, but they were also downweighted, but not as much as the hand line discards, and so this starts in 2005. Then it moves through 2006 to 2014, and the fits were pretty good in a few of the years, 2006, 2014, 2011, but that's the end of the length comps.

The age comps come in here. This is hand line age comps starting in 1990, and the sample size cutoff, if you remember, is ten. We had two years where we had eleven samples. Here it's fourteen, and then we start getting into better sample sizes in 1996 and 1997, where you see the fit starting to improve. Here is 1998, 2004 to 2008, and 2009 to 2014.

We can go back and forth with this to answer Ben's question, or to address it. If you will focus here on 2012, because we didn't have 2010 or 2011 data, because of the moratorium, but 2012, 2013, and 2014, the data are fit fairly well, and we have a peak of age fives in 2012, age threes, fours and sixes in 2013, and fours and sevens, and so we do see an age class moving through.

Here is the age comps for headboat, and we have pretty good sample sizes back in 1978. This is 1978 through 2006. There's a few years here where you can see an age class moving through, but it's all the younger individuals. We have here in 1981 an age-two peak, an age-three peak in 1982, but then we don't have an age-four peak in 1983, probably because they're just not capturing many older individuals in this relatively shallow fishery and the size of the hooks and the way they fish and all that, at least early in the time period.

Then we start to see some of those older individuals in the mid-1990s. Here is 2007 through 2014. If you will notice, this is headboat, and we don't see a real movement to a lot older individuals during the mini-season, which holds true to what we looked at as the assessment panel. We see age twos and age fives and age threes and age sixes and age twos, again, in 2014, but we see those ages and we see those modes in previous years as well.

Here we have our age comp for the chevron trap, and this is meant to reflect CVID, but, again, we don't have any comps from the video, and so this is just chevron trap comps. These are the corrected comps, and they're fit quite well. They are actually, by the iterative reweighting, upweighted in the base model. We see age threes moving to fours and moving to fives. We have sixes and sevens here. We do have signal coming through with this chevron trap composition. We also have a peak at age-two here, like we have a peak at age-two for headboat. We do discuss the recruitment signal that we get at the end of this base run, and so this is some support for that.

Here is the age composition for the general recreational fleet, starting in 2001. We have a peak of age threes. In general, it moves to fours in 2006 and 2009. Then, in 2012, the comps shift over to older age classes than they've ever had previous to the mini-seasons, and so this supports the idea of a separate selectivity for this fleet during that last time block.

DR. SERCHUK: I guess I'm struck, one, by the relatively low numbers in many years, maybe thirty or maybe twenty or maybe forty individuals in here. Some of the years don't have many. You have twelve age groups that you're looking for. If you only sampled twenty fish, you're not going to get a very good representation of the span of things with such a low sample size, and I realize that this is what's available.

Those years, for example in this one, where you're starting to see some changes, you're really having some very large sample sizes, 315, 130, 121, and I think we've got to be a little bit careful about this. I realize that you start your initial discussion talking about a minimum sample size, but we should keep in mind that the low sample sizes, in many cases, may not give us a representative picture of composition in these fisheries, particularly if they're taken either at one point in time or if they're not taken across trips or so on and so forth.

I realize you've done the best that you have with the data, and so I'm not critiquing that, but we should consider that low sample sizes are likely, in many cases, not to give, generally, a representative pattern of the age composition of the catches of that particular fleet, and I think we've got to keep that in mind. Thank you.

DR. SIEGFRIED: I completely agree with that. I absolutely think that the comp sample sizes need to increase.

MR. HARTIG: It's kind of hard to go back in time and do that though, but this is where my question that I asked earlier should have been asked, and I'm sorry for that, but, to me, I see these relatively low sample sizes in the later years -- This is where my question about was the information that we got from FMRI -- Is that in these age comps here?

DR. SIEGFRIED: Yes, it is. These sample size increases that Fred was just talking about in 2012, 2013, 2014, they come from that data. Those are that data. We don't have that from MRIP. I know that's been a common misconception that those data aren't used, but the sample sizes from MRIP are in the single digits for those years or zero. One of the years, 2012 or 2013, one of those two years, there were no comps taken from MRIP during a mini-season, and so all of that -- I mean Florida has been instrumental in this assessment being able to be done.

MR. HARTIG: No, and I saw that in the assessment, where you guys talked about FMRI and how important those studies were, but I still had questions about where it was actually used.

DR. SIEGFRIED: Just as a reminder, our sample size cutoff is trips instead of fish, and this is to remind you that it's ten trips capturing any number of fish, but it's sampling of those trips. It's still a very small sample size, and so it's not 315 fish for 2014. I wanted to comment on what Fred said, too. I mean I understand and appreciate your point.

One of the things that I guess you all can consider is when the model is allowed to go logistic for the general recreational fleet, it does so with that larger sample size, these last three years of higher sample size. We have a logistic function modeling selectivity before that, and we did not discuss what happens if we had more data in the earlier time period and what that would look like, whether it would go logistic, but that would substantially change the result, but, like Ben said, we can't create that filled in comp.

DR. BARBIERI: Go ahead, Kate.

DR. SIEGFRIED: Okay. Here is to Fred's comment before, as far as what these error bars look like. The error bars cover the error bars, but the mean of each of these years for CVID is not -- Like the last year is outside of this mean, but it's not outside of the error bars. I'm not sure if that answers his question, but here is the fit to the CVID and the commercial hand line index, and remember this is 2009. This is 2010 to 2014.

This is with an iterative reweighting, just to get SDNRs to one. We see a good fit in both of these instances, with the residuals not having a noticeable pattern. We do under fit this terminal year for 2009 for the commercial hand line, but we over fit 2011 here.

DR. SERCHUK: I'm glad to see this in there, because this was my concern. My interpretation of the figure on the left is, apart from the last data point, that suggests, to me, that there has been no trend. All the error bars overlap. They overlap considerably. It's only that last data point, and the fact that that overlaps three of the other data -- Perhaps in the last year things have gotten better, but I'm withholding judgment about whether there's a trend. We saw that blip in 2012 or 2013, and then it came down again. It may be something positive going on here, but I just think we have a very short time series and a lot of variability. We're hopeful. I would be hopeful, but I wouldn't stake my salary on it. Thank you.

MR. HARTIG: While I'm thinking about it, I mean when we were talking about cohorts moving through the fishery, and I can't remember what table that was that I looked at. Maybe it was proportion of ages through time, but if you look at the years of the biggest year classes, you can actually track the sampling through that time. You can track cohorts all the way to the end of the time series, which I found pretty interesting, but then it doesn't really show up in what you presented before, but you can see that year to year to year to year, and sometimes more years than I would have thought, for some of those larger year classes.

DR. SIEGFRIED: You're talking about the cohort plots that was in the addendum? We can pull that up if you want, but the thing to note about that is the oldest individual cohorts and how the twos, three, and fours sort of start to disappear. What we really want to see, for say SSB to be strong and for there to be a rebuilt stock, is for those cohorts to move all the way through those twenty ages, in reduced proportion to M and F and all of that, but we don't see them past age ten or age eight in some of the fleets, and so those are nice plots to show that, and we show those separate from those age comp fits so that we could track the cohorts.

DR. BARBIERI: Let's save that, perhaps, just so we don't disrupt the flow here of going through the presentation.

DR. SIEGFRIED: Also, to Fred's point, I'm sure you all have this in your documentation, but you may not have been able to get to it. The 2010 year for the CVID index was hotly contested. There was a workshop about it and all that, and so if you want to decide whether you think a trend is here, an increasing trend, the 2010 year has been thoroughly vetted as to whether it's reflective of the red snapper stock, but you can see why this has been an issue, whether 2010 should be in this index.

On the left here, we have the headboat index, and on the right is the headboat discard index. The headboat index, particularly in the earliest time period, the ASPIC model couldn't fit that and the SEDAR 24 benchmark couldn't fit that. It's just a very difficult time period to fit, as well as these last two years, and that's because of the magnitude of the change, it seems like, for these last two years. This last base run actually came up a little more, but you can see the error bars are quite large, and that's because the headboat index is actually downweighted a bit, I believe, from one. SDNRs are still at one, but the index weighting. It seems to be fit best during the data-intensive period, and then it just can't do this -- I think it's a 250 percent increase in that last year.

The headboat discard index, this was thoroughly discussed at the review workshop as well. There was mixed opinion with the reviewers as to its usefulness, whether it should be truncated after 2009. We see that it over fits that terminal year. It under fits 2012. If you only include 2009, up to 2009, the final base run results are not different. If you cut it out entirely, the final base run results are not different.

What it seems to do -- I have it later, but it affects the weighting on the age comps from the chevron trap, because that's where the likelihood push/pull comes from. I mean you all have the reviewer report. The individual reports, I think, are still coming, but they did not ask for me to remove it entirely for the final base run, but they wanted to see every iteration of it being removed or truncated, in order to be sure that it was okay to leave it in there.

What we found for numbers at age and biomass at age is here for you. This is by year. It's color-coded by age, and so the red up the purpose is age ones, up to age twenties. This is in number of fish. Here is the same color scheme, except for it's in biomass, metric tons, and what we see is these -- This is basically showing you the effect of recruitment on the numbers at age, and so we have a high terminal recruitment.

We have other years where we know that we had high recruitment in the mid-1980s and the mid-2000s, and this jump-up that we see is as large total numbers as we had in the 1950s, is not actually reflective of total biomass, and I will show you later, of spawning stock biomass. This is reflective of the extremely large recruitment event that the model is estimating, and so the majority of these peaks are these age one.

For the biomass, we do see an increasing number of age fives to age tens to age twelves coming back into the population, and so we do see this recovery of the older age classes, but we don't see the distribution of the ages that we would expect if it had recovered to historical values, but the total biomass is similar to what it was in the late 1970s, and so high numbers, high total biomass, but the spawning stock just is still not recovered and the age structure is still not recovered.

Here is the illustration of the spawning stock. This is the spawning stock in eggs and this is showing that until we are able to get -- Until the population, according to the model, gets more of these mid-age and older-aged individuals to contribute, and they contribute more to the spawning stock than the younger individuals, then we wont actually be able to recover values that would allow us to consider the stock to be rebuilt under the current rebuilding plan.

MR. HARTIG: Thank you, Katie, but what's really happening in the stock? I mean we had that large year class right before we closed the fishery, and that, in my estimation, was probably

precipitated by some of the management that the council had put in way back. Then we have some continued rebuilding in the stock.

Based on what we see here, you wouldn't think the stock would be able to continue to put out these giant year classes, but it is, and so the last one in the time series is the highest we've seen since I don't know when, but it is one of the highest we've seen, that last year of recruitment. Something, to me, isn't quite working with what we're looking at with these numbers. The stock seems to be pretty resilient, once you get it up past some minimum level, to overfishing, and so that's all. I'm not going to go anymore.

DR. SIEGFRIED: I do think that the stock is capable of putting out the recruitment that we're modeling, obviously. Otherwise, the model wouldn't allow it to do that, but the issue with it is whether those recruits survive to be able to contribute to the reproductive stock later in their lives, and so with the way that we're assuming selectivity and the way that the age and growth is coming back and the way the reproduction is dependent on age, we have this huge year class coming through, but the discarding rates that we see being reported, the discard values, the landings in the general rec, and all these things are taking those smaller individuals, is what would prevent them from later contributing to the spawning stock biomass in their lives. This spawning stock biomass we have out there can create these big recruitment events, but those recruits have to be able to survive to then create and spawn themselves.

DR. BOREMAN: Kate, the values of what we're looking at, from about 1978 back, those age comps for those years, that's model-generated. It's not based on actual aging data, and so the model is assuming that we must have had fish out to age twenty? Because, if you look, it's pretty regular from year to year. The only changes are just because of the total biomass changes, but everything else looks fairly even.

The model is assuming this is the way it must have been, even though the age composition might have been radically different than this, where back then you might have had a lot more of the younger age groups in the biomass, but this is just a -- It's basically a manufacturing based on the model itself and what the model says this is what the age composition must have looked like to produce that level of biomass or what?

DR. SIEGFRIED: You're asking me that or --

DR. BOREMAN: Yes.

DR. SIEGFRIED: Yes, it's assuming an equilibrium age structure, and that's assuming that we're back to 1950. We do have minimal uncertain commercial landings prior to that, and so we know it's not a zero F situation then, but we do estimate that initial F and assume this equilibrium age structure.

We've done sensitivities to look at a later starting year, in 1978, which would remove that requirement to assume equilibrium age structure. We needed to decide on an F, an initial F for 1978, but we get the same stock status later in time, which I will show when I go through the sensitivities. Based on that, I think that our assumption of an equilibrium age structure is pretty sound, and we do get the same results later, if we start in 1978.

DR. SERCHUK: When I looked at the previous slide with the numbers, I am struck that there doesn't seem to be really any consistency between the age ones and what you see at age two and age three, and maybe it's because of this discarding and maybe it's because at age one we have a very high mortality rate, natural mortality rate, and it's still high at age two. It's unclear to me whether it's the high natural mortality rates that were being assumed at those first two ages or it's the impact of the fishery, in terms of taking significant numbers of mortality, whether it's discards or catches, or whether it's the combination.

One doesn't see the progression, even at those very early ages, of seeing very high ones and then seeing, two years later, high threes. I guess what's the implication of that? Is it, as you said, because there is still a significant amount of fishing mortality added to very high natural mortality at those ages? Is that the issue of why we're not seeing the progression of these year classes, even at very early ages, from age one to age three?

DR. SIEGFRIED: I would say the increase of M has been a big difference between these two assessments, but certainly that's something I didn't mention when I was answering Ben's question, but yes, the gauntlet that these recruits have to run through includes that higher natural mortality. In SEDAR 24, the input, particularly public comment we were getting, is that M can't be that low for them.

We have these new meta-analyses that show, to some extent, that M wasn't as low for the older age classes, but we're also assuming this age-dependent natural mortality, and so when we use this new meta-analysis, those M estimates are higher, and so they go through that gauntlet. Then they go through the discarding gauntlet, because of the way it's set up for that size limit and for no size limit during the mini-season, and then the intense discarding. It seems like the gauntlet is thrown down for these recruits.

DR. SERCHUK: Would it be fair to say, looking at some of the age compositions, that the age at recruitment, in most of the fisheries, is age two, rather than age one?

DR. SIEGFRIED: We do see some age ones in headboat, but it does seem to be the modal age of recruitment is age twos.

DR. BARBIERI: Katie, I'm not sure I have a question, but more of an observation. If you go to Slide 77, my impression, looking at this, is that this egg production, the fecundity at age, actually is very poorly informed for most of the older ages, because they haven't been available, really, historically, to be sampled, and so I think, to add to something John Boreman mentioned earlier, in terms of how this is being constructed, I think that -- I can understand the choice of going to egg production as perhaps a better metric of spawning potential, but I cannot see how those either batch fecundities or spawning frequencies -- I mean this data is pretty difficult to get, usually, within even very, very well-sampled populations.

I cannot see how we would be able to come up with that in a way that is informed by real data, and I know that this is not really the part that you handled. I think Nikolai was mostly handling the egg production thing, but I think two things here. One is that using egg production as a metric is really disproportionately weighing this towards the older ages, one. Two is I cannot see how we were able to have real data to inform the estimation process for the egg production.

DR. SIEGFRIED: The studies that the life history group did, and Nikolai and Dave Wyanski, I believe, worked on those, but they fit functions that would assume that the -- I will show it in a little bit, but like the batch fecundity, they were able to fit the important part right, the part that is increasing with age, and then they assumed that the function continued the way it was continuing, and so it was a logistic relationship.

Most of the action actually happened in less than twenty-year-olds, and so the changes in egg production is less than twenty, and so that was well informed, actually, pretty well with the sample sizes, and so there is no reason the life history group thought, and Marcel might be able to weigh in on this, that the functions should decrease with those ages after twenty, because it seems like -- Last time, they used gonad mass of mature females.

That's not easy to inform either, and so this batch fecundity by age looked at actually the ability of those individuals at the action point, like between fifteen and eight, and how they differentially contributed to fecundity, and there are good data for that. We did, during preparations between the assessment workshop and the review workshop, look at whether pulling back and using gonad mass gave a substantially different answer, and it's not substantially different. It was actually more pessimistic than this is, and this is informed much better by data.

DR. BARBIERI: My point is not really in terms of direction, but it's just that I think there are lots of assumptions of extrapolating beyond the data that had to be made to be able to construct this. Marcel, unless you have --

DR. REICHERT: No, I agree with Katie. We looked at that, and I can look it up on the data workshop report, but I thought we have fairly decent sample sizes for the fecundity estimates.

DR. BARBIERI: Beyond age ten?

DR. REICHERT: I can look it up, but if you look at the growth curve, that's, I think, what Katie was also referring to. They grow really fast, and then you get the leveling off, and so they grow in weight a little more, but, even if you look at that growth in weight, that levels off at some point.

DR. BARBIERI: Right, and so that relationship between fecundity and size would be easier to capture. As we rebuild this to age composition, I mean that's the -- If we have the truncated age comp that we have had historically, how would we have the proper sample sizes to actually estimate those fecundities in a way that's -- Because these fish haven't been there in a highly juvenesced stock, or that's my point. I mean I can see the size being one thing, but, in terms of the ages, it's puzzling, to me, how that relationship was established.

DR. SERCHUK: I'm a little bit puzzled about the discussion, and I'm a little bit puzzled, because, it seems, to me, apart from -- We remove this early part of the series up here, this 1980 and older, which I guess was John's question. There is a disjunct here between the dynamics of the population beforehand and afterwards, but, afterwards, clearly, no matter how you look at reproduction, whether you do it by mean weight or whether you do it by egg production or whether you do it by ovary weight or anything else like that, you only have a few groups in the population.

There is only three or four or five groups in the population, and they're mostly dominated by young, immature animals, one and two. They don't really mature, as I understand it, fully until

age three. Then you only have three or four age groups of considerable size. After that, you have nothing, and so whether you weight one-half of 1 percent by egg productivity or whether you do it by mean weight or you do it by gonad size, it ain't going to be a lot, and so that's where I'm a little bit confused here. If you're talking from a theoretical point of view about what do we do to try to promote that, that's a different issue, and maybe I'm misunderstanding what the discussion is about.

DR. BARBIERI: I don't think you're misunderstanding. I think it's because on the Gulf side we've had a longer history of assessments, and we've had, since the oil spill, a ramp-up of sampling, and we have actually compared differences in egg production that are the result of batch fecundity and spawning frequency, and we actually have metrics to compare what the outcomes of the assessment -- How those metrics actually compare to each other.

It's interesting, because, as you go to that earlier age, even twenty, I mean there has to be some calculation that was made to come up with those -- Those are expressed, in spawning biomass, as numbers of eggs for that earlier time series, and that must have been informed by data, right?

DR. SERCHUK: This gets back to my question, Mr. Chair, before about are we really sampling the populations that are out there? It all gets back to the size-based understanding of the different fisheries, and if we're not getting a true representative picture of what's being caught, we're certainly not going to get a true representative picture of what's in the population, and that's where I really think we need to think about if we really wanted to improve this.

We have to step back and say, are we getting a representative picture, not only from the fishery-dependent, but I'm not convinced that the fishery-independent, certainly the chevron traps -- I don't think they're actually giving -- I think they're giving a very narrow picture of what's going on for a certain segment, for the earlier age groups, but I think we're missing a large part of the picture here in terms of the larger and possibly older segment of what might be out there. Thank you.

DR. REICHERT: You mentioned the maturity. The 100 percent maturity isn't until age three or four, but age one, there is about 80 percent, age two is 93 percent, and age three is 96 percent mature, and so they mature very, very early, which is kind of interesting for a species that lives forty or fifty years, plus.

There is a variability in the spawning season duration, and that was the modeling that Nikolai did. In terms of the -- I would be very carful saying that the chevron trap captures a very narrow range, because we do capture the older ages. The selectivity, obviously, is an issue, but we do catch a fairly broad range of ages, and that data was used in the fecundity estimates, and so whether or not that, as with any of the fisheries, whether that's representative of the true population, obviously we don't know until we know the true population, but we do catch the older ages.

MR. HARTIG: To your point, Marcel, yes, you guys do catch a broad range of ages. I just don't know that they're in the proportion, that your ages are in proportion, to what has been collected in the FWRI information. The information that I've looked at, the proportion of eight-year-olds and nine-year-olds are much higher in the hook and line surveys than they are in the trap surveys, and so that's the question we have. They're four times higher, as a matter of a fact.

DR. REICHERT: I agree, and that's where the selectivity of the different gears comes into play.

DR. BUCKEL: Katie, on one of the webinars, I had asked, if you could go back to the previous slide, where you have the numbers of fish estimated from the model, the number at age, if there was a way to maybe have that same type of plot for the observed data. I know the selectivities are an issue, to make it not completely apples-to-apples, but like the data on Slide 19, where you had the age comps, age by year, for the different fishery-dependent and independent -- The observed age structure. If that was in a stacked bar, like what's estimated from the model, then maybe you would be able to see that once you get over eight, it's a really small part of that bar or not. It would help, I think, with the conversation that's going on about the selectivity.

DR. SIEGFRIED: I don't have that available right now. You're saying that the sample sizes -- I can't see which number this slide is.

DR. BUCKEL: Slide 19 was a bubble plot that had the proportion by age.

DR. SIEGFRIED: Yes.

DR. BUCKEL: It was for each year, and so it's similar to --

DR. SIEGFRIED: Just look away while I do this. It always makes me dizzy. Is it this one?

DR. BUCKEL: Yes, and so this question that maybe if you put these selectivities in and the model is underestimating the number of older fish that are out there, and so if we had some kind of stacked bar showing the observed data for numbers at age by year, that may lend some credence to what the model is saying, or maybe not.

DR. SIEGFRIED: People don't like this plot, do they? I thought it was awesome, but nobody likes it.

DR. BUCKEL: I like it. You do see the build-up of some older fish, and that's -- When you look at the model, you have to squint at right at the top of the bars for the numbers at age to see the same trend, and so, maybe if this was plotted in a similar way, then we would see a match or not.

DR. SIEGFRIED: I'm not sure if I would be able to get that for you, but I see what you mean. It would just have to be -- It would just be exactly, I guess, what Ben is asking, like is this representative of what the model is outputting as total numbers, is the composition sampling representative of what the total numbers are coming out? I see, and I think that if -- Maybe we can look at this together separately or something, but I think that this actually shows quite a bit of that, especially here, where we see this big age-two bump, and it's just this is not going to be as easy to draw out, because of what you were saying with the selectivity and all of the assumptions that go into building that N, and we're not going to see recruits using this, like we do in the N plot.

DR. REICHERT: Katie, just for clarification, the SEFIS is the chevron traps, right?

DR. SIEGFRIED: Yes, and we're still getting used to calling it SERFS. It sounds like you're schlepping us up a mountain or something. Any other discussion? Okay. Let's look at some more of the estimates from the model. The top-left corner here, we have the recruitment time series.

We started estimation in 1978, and this is where we're still estimating it in the terminal year. We did go through sensitivities of not estimating it in the terminal year.

Here is the recruitment residuals, the deviations, and what you can look for here is our assumption of a steepness of 0.99 -- What we're assuming is that we have some sort of average recruitment and then deviations around it. This doesn't show any pattern, any sort of oscillation. This is meant to show if we could find one, but it's really kind of a gunshot blast, which is what we would want to see if we're assuming average recruitment.

In the upper right-hand corner, we've got our total biomass by year, and we do see this uptick at the end, and then our SSB through the years with this MSST line and then the SSB F 30, and so we used F 30 instead of FMSY once steepness was fixed at 0.99. That was what was used for the rebuilding plan, I believe, as well, and so this green line would be the rebuilding target. We do see an increase here, although the increase is slowing down a bit.

Then on to selectivities. We've got a logistic selectivity assumed for SERFS, and this is for the chevron trap, and what this will show is that it will be by age, the selectivity at age, and then I will have a legend here that will show the first year that that selectivity curve goes into effect. That will matter when we start having time blocks. Here, the blue is the 1950, and then the last year is 1991, and so then the next curve starts in 1992, and the green curve starts in 2010.

This is for commercial hand line landings on the left and discards on the right. The blue is before any size limit implementation, and assuming that the 1983 size limit didn't have a big effect. We got stakeholder input saying that that didn't really affect the way that they fish, because they didn't collect many smaller individuals anyway, but the 1992 change, the twenty-inch size limit, did. The model estimated that pretty well.

There is just a slight difference between that second time block and a third time block for those logistic selectivities. Then, for the discards, the red is the moratorium period, which would be much more like landings, because everything that's caught is discarded. However, this curve here is meant to reflect the twenty-inch size limit, and so only twenty-inch fish were discarded, which the model had to convert into age and create this estimate of selectivity.

The headboat landings and discards are shown here, and we assumed a double logistic dome-shaped selectivity for all three time blocks for the landings, with a fixed age of ten, to avoid that zeroing issue. The first and third time block are very similar, which is interesting, because the only landings that occur in the last time block had no size limit, and so that did follow pretty well with what we were assuming for the first time block. The second one, you can see the mode shifted by gear. That reflects the change in avoiding those twenty-inch or discarding those twenty-inch fish, less than twenty-inch fish.

On the right, you will see the discard selectivities for 2010. That's the moratorium period, and so everything is discarded outside of the mini-season, and so we do have a much broader range of ages that are discarded during that time period, but this blue line is just the under twenty-inch fish.

For general recreational, the discards mirrored headboat, but, in -- The first time period, it did mirror headboat. The second time period is 1992. We estimated, and we fixed this at age thirteen, because there were more data in the older ages for the general recreational comps during that

second time period, but this is mirroring headboat for the first time period. The last time period was during the moratorium, or this is all mini-season landings, and I've mentioned this a few times, but this kept wanting to go logistic, even when we had it modeled with the dome-shaped function, and so then we went ahead and used a logistic function. The assessment workshop panel recommended this change, and then we did further exploration during the review workshop to test that assumption.

Here is the fishing mortality by fleet, by year. The commercial hand line is really the fleet that has declined the most as the result of the moratorium. You can see their general F, which is analogous to effort or analogous to their catch, although it's not equivalent. It just shows their contribution to the F of the stock, and it's nearly gone, really. The green is the general recreational fleet, which has similar levels of F to the stock as it did before the moratorium. The pink is the general recreational discards. The light blue is the commercial hand line discards.

DR. BUCKEL: Is there a mislabel? The general recreational discards are -- There is more dead discards from general recreational than there is in landings, but this would suggest that -- Unless there is something that I'm not following with F, but I would think the F from the general recreational discards would be higher than the F from the general recreational landings.

DR. SIEGFRIED: The fishing mortality here takes into account selectivity, and so the general recreational fleet has the logistic selectivity, which captures older individuals, which are in lower abundance, which increases the F estimate on them, and so the F by fleet is still higher, but the total take of discards is higher.

Here is the landings in weight and discards in number by fleet by year. The commercial hand line, again, is the red, the blue is headboat, and then pink is general recreational. This is all the landings in weight by fishery. Commercial hand line is usually measured in weight. However, the recreational fleets are usually measured in numbers, and so this had to be converted. Here, the discards in the commercial hand line -- The weight of those is different from the weight of the recreational discards, but you will see that the largest component of discards is general recreational fleet, followed by headboat, and then the smallest overall, in the most modern period of the model, is the hand line fleet.

The references I have here, L F30 KLB is meant to be an MSY proxy, and so you'll see that it's under an MSY for the landings, but it's over the MSY for discards. Here is the yield per recruit plot with F30 as a measure. There has been discussion about different -- You know, F30, F40, Fmax, all of those, and you would see the peak of this is at about 0.21. That would be the maximum F of this yield per recruit curve, for your discussions later, and then the spawning stock biomass in eggs, by recruit curve here, is plotted, with 2014 being fixed to that line. A bias correction is in there, and you will see it's just kind of all over the place.

This is the thing that I think Luiz was mentioning earlier. This is the equilibrium age structure. Age one through ten are approaching the equilibrium values, but the older ages are still below what is expected for a rebuilt age structure, according to F 30 equilibrium. This is purely based on what the assumptions were last time.

I know you all have to decide on the F, again, but here is the F30 equilibrium. Each decade seems to have moved away from this equilibrium. However, the 2014 value is approaching that

equilibrium for those younger ages, and it's getting better than it was since the 1990s and 2000 for certain ages, and actually doing quite well between ages 7 and 10, and so we're getting there. That's the base run results. I have the sensitivities next. Are there any other questions about the base run before I move on to sensitivities? Okay.

We had some assessment panel-recommended sensitivities, but then we also had some reviewer requests for sensitivities, and so I wanted to go over the reviewer's requests first. After that data correction that I mentioned with the chevron trap age compositions, the reviewers didn't ask for all of these sensitivities to be rerun. They cited the fact that the changes to the base were relatively minor, and that the changes to sensitivities would be minor as well.

Really, the point of the sensitivities is to determine whether the model is behaving as we would expect, given the dynamics, we're assuming. The two additional analyses they did request were to look at the effect of the headboat discard index with the new base of the corrected data, rerun that truncated and dropped headboat discard index sensitivities, to make sure that it was the same, and then to look at the effect of the logistic selectivity assumption on the general recreational landings, and so I used a dome-shaped selectivity and mirrored the headboat to see if that made a difference on status.

This is very small. It's in your report and it's in your presentation, but we ran around twenty-five recommended by the assessment panel. That covered the gamut of assumptions, anywhere from all the assumptions about the dependent indices, the values of M, the discard mortality range, a lower steepness, differences in landings, high and low, differences in discards, high and low, and then a continuity run.

I will go through these, but they all have a similar format, and so I have the F benchmark here by year, and so that time series, and the biomass benchmark, SSB benchmark. The base will either be black or red, depending on how many lines I've had to plot, and then all of the sensitivities will be plotted along with that base. This line is the one-to-one line, for reference, and so this first one is looking at the sensitivity of the base run to changes in the fishery-dependent indices.

Using only the fishery-dependent indices was one of the sensitivities. I upweighted them relative to the CVID index, and those two, those created the most optimistic status, and so the only fishery-dependent indices, and so ignoring the SERFS data altogether, including the comps, brought the status to a more optimistic place for the years prior to the terminal year.

I mean that's ignoring that independent data, and then also upweighting, by threefold, all the dependent indices sort of made them trump the, and pardon the reference, all of the independent data. Then, at the bottom here, you can see that that green curve is the one that has the highest estimate of SSB. Removing the last two years of the fishery-dependent indices, or using a time-varying catchability for the headboat index didn't have any effect. The point is here that the CVID index is definitely important in the age comps from the CVID index. They're contributing data that the dependent age comps don't provide.

The next one was looking at the sensitivity of the base to the fishery-independent index changes. I used a longer CVID index, which is the chevron trap had data back into the 1990s, but there wasn't enough red snapper to do a standardization back into the 1990s. They did do sort of a revamped GLM to try to get it as far back as they could in 2005. That was the first year. However,

the index working group still didn't think that the sample size was large enough to use for the base run, but it was recommended for sensitivity, and so we used 2005 to 2014.

We upweighted the CVID index tenfold, and we separated the video and chevron trap index and included them separately. Then we only used the CVID index and no fishery-dependent indices. The one that seemed to matter most is when you use the video index and chevron trap indices separately, which is basically double-counting that index. This is the one where the video is mounted on the chevron trap, and so it gave that double optimism, is what I was calling it, and so it's this light blue line.

The other ones have some changes in the middle, but it seems to me like there's not a -- The CVID only was just I was shocked it ran. It's a five-year index and no other indices, and so I wouldn't take that long time trend to heart.

The next one was the sensitivity to natural mortality, and I looked at the upper and lower asymptotic Ms to get this upper and lower, and so I used 0.11 as a lower and 0.16 as a higher, and the base run asymptotic M was 0.134. This is a standard result when you use a lower and higher M, that you sort of envelope your assumption of M for the base run, the lower M offering this more pessimistic view and the higher M offering a more optimistic view. The next is the sensitivity to discard mortality. This is also a predictable effect, that sort of enveloping effect that I mentioned. The lower discarding mortality is more optimistic, although it doesn't seem to have a big effect on the spawning stock biomass benchmark.

We had some concern with the 1984 and 1985 MRIP landings values expressed during the data workshop, the assessment workshop, and the review workshop, and so we smoothed that value using a geomean of surrounding years, and it didn't cause a big difference in the terminal status, although it did cause a difference in the weight of the fishing mortality that was estimated during those years and the surrounding years.

The next one was the sensitivity to steepness. We used a lower steepness, 0.84, which was used for the -- I can't remember if it was 0.84 or 0.85, but one of them was the value from the meta-analysis and one was used in SEDAR 24, but it causes a large effect on the terminal F status, but it has relatively little effect on the terminal biomass benchmark, and so lower -- That's if we were to interpret it -- This is also using the F30 instead of FMSY, and so it's not exactly comparable, but it does cause the effect you would think, with a lower steepness on the model.

We used the aging error matrix that was provided. It increased the overall variability, but it didn't have a set bias across the time series. It's a slightly more optimistic outcome in the terminal years, but it wasn't consistently higher or lower during the second time block, the 1990s to 2009.

I looked at the sensitivity to batch number. I also looked at whether a constant batch number would have any different effect. I don't show it here, but it looks the same. There's just almost no discernable effect, which is what Fred was saying before, and so there's a lower and higher batch number recommended by the life history working group, and there is no change.

Then we looked at the sensitivity to the headboat discard index. This is the one where the reviewers asked this to be redone with the new data. They wanted to consider whether this index was appropriate. There was concern over fisher behavior changes due to no size limit and the

moratorium in 2010 on. It was recommended by the reviewers to leave in, though it was one of their points of contention. It seemed to -- The justification to leave it in seemed to be because there wasn't a big change when it was taken out, and that's reflected here.

The next one is the sensitivity of the base run to landings and discards uncertainty, which we do more comprehensively in our MCB analysis. For this, we used the 10th and 90th quintile from the MCB bootstrap, which I will describe when I get to that section, to create these alternative landings and discard streams, a higher and lower for each, and, as we would expect, the lower landings and lower discards bracket the base run. It has very little effect on the SSB status in the modern period, but you can see big changes as the fishing mortality was ramping up.

We also looked at whether the commercial hand line fishery had a dome-shaped selectivity, and this didn't seem to have much of an effect on the overall biomass or benchmark of spawning stock biomass. It did change a bit as the fishery was ramping up, which we would expect, when there were more large, old individuals predicted in the model, but, overall, the change is not qualitatively different, or the results are not quantitatively different.

There was also almost no discernable effect when we looked at the sensitivity to that plus group specification, and so remember where we had the headboat and general recreational selectivities that we fixed it at age ten or we fixed it at age thirteen, so that we would have a non-zero selectivity for those older ages. That didn't matter for the final result.

Then this is what we were talking about before, sensitivity to a later start year. We started in 1978. We iteratively estimated an F_{init}. We went through several permutations of how to do this and what the model would estimate, and so the base run F_{init} for 1950 is 0.03. F_{init} estimated in 1978 is 0.2. There is just not much difference in the terminal status. We do see a little bit of a lower F estimated during the first few years of the model, when it starts in 1978, and a slightly higher level of SSB when it starts in 1978, but we don't see any difference in the remainder of the time series.

The next thing I want to show you is the continuity. Now, I don't really think a continuity is possible, exactly as it's defined, but I use that word here loosely. The changes that we implemented were trying to get back to SEDAR 24 values used, and so natural mortality, steepness, the recruit standard deviation, our SSB measure, where we use gonad weight instead of total eggs, the time of year for spawning, our maximum age was a little different, and the discard mortalities.

We did not change all of the landings to what they were before. We didn't do all of those changes. We just made these changes, and we can see the continuity run has a much more pessimistic result for the overfishing status versus overfished status.

DR. BARBIERI: Katie, we have a question.

DR. SERCHUK: I had one question about the sensitivity that you put up for natural mortality. As I understand it, you used the asymptotic value for M, which sort of comes into play in age groups seven, eight, and nine, but that's -- I know that between this assessment and the previous assessment that there was a significant change in natural mortality for the younger age groups. They're almost twice as high now as they were previously, and I wonder whether you did any sensitivity analysis on those values.

DR. SIEGFRIED: I should have said it better. I did do the entire -- The entire curve was recalculated, and the asymptotic M was done the way the life history group recommended, to 0.11 versus 0.16, but the entire curve was recalculated over all ages.

DR. SERCHUK: One other question. You had a plot there of a stock-recruitment plot, which was very noisy, and I wonder whether, and I mentioned this point before -- Because the natural mortality is so high at age one, that's where most of the action is, in terms of controlling the age one dynamics, and whether you could have plotted that with age two as the age of recruitment to the fishery, because that's pretty much what it is.

DR. SIEGFRIED: I don't have a plot of that, but that would be a good idea. I do have that for the MCB, to look at the difference in age two on versus all of the ages.

DR. SERCHUK: My point is, if you start looking at that stock-recruitment plot, you find out you get very high recruitment at very low stock sizes and get very high recruitment at high stock sizes, and that's because most of that recruitment is being manipulated by M, a very high M value. It will come down. It will still be high, about 0.4, at age two, but that might be useful to look at as well. Thank you.

DR. SIEGFRIED: I have for you here the phase plot of all of the sensitivities we ran. They all qualitatively agree with the base run, in that they're in this quadrant, less than one for the SSB, 2014 over SSB F30, and then over one for the fishing benchmark. The most optimistic one is the fishery-dependent data only, and the least optimistic one is the continuity case.

MR. HARTIG: Were there any investigations in the sensitivity run using the entire time series with the chevron trap data?

DR. SIEGFRIED: No, because there wasn't enough -- It was in the single digits for the amount of red snapper they encountered before 2005. Here is the additional sensitivity requested by the reviewers about the third time block for the MRIP landings. It was set equal to the third time block headboat, which is a dome-shaped selectivity, and you can see a slight little difference here, but it's, in general, very similar and they just wanted to see if this would have a big effect, because of the logistic versus dome-shaped, but we talked about it, and that MRIP may be logistic for that last time period, but there are relatively few large individuals out there for them to take, and so that's why we thought we didn't see a big effect here.

This is some of the work that we did during the review workshop to look at how the headboat discard index was affecting the model, and so we saw this -- The fit, if you recall, and I can bring it up if you need me to, but it was over fit in the terminal year. It was pretty well fit the rest of the time, and it seemed like it was not being fit that last year, but having a level of recruitment, and the reviewers were a little bit puzzled as to where the model was getting the information it was getting for this high recruitment.

An iterative reweighting accounted for this effect of the headboat discard index. The status was nearly identical when the headboat discard was included, truncated, or removed, and what you will see here is the likelihood relative to the base run, and so this is the age comps from the chevron trap. A negative value indicates that it's a worse fit. When the headboat discard index was dropped, the fit, or the likelihood weight I had to use was -- It was improved. The fit and the

likelihood weight was reduced for these age comps for chevron traps, and so that seemed to be the push/pull that was happening with the headboat discard index, and so having that iterative reweighting seemed to account for that headboat discard index effect.

I've got retrospectives to show you next. These are kind of like sensitivities, I suppose. We're removing one year at a time. There is a large change in the F status. You will see here this red dot is the base run, and it goes down to 2013, up through 2013-only data, 2012, 2011, and 2010. We didn't go farther back, because we already were pushing it a bit to go to 2010, because we were entirely removing the time series, the CVID index and the age comps, and so there was a lot of discussion among the reviewers about why this would happen and how F was calculated and whether this pattern was a problem, and I don't -- There wasn't any further work required at the review workshop, and I think that you all have the group report, although not the individual review reports. That effect was not seen, or that pattern was not seen, for recruits or for spawning stock biomass.

The status, when we did the retrospectives, was similar to what you saw before, and so we have this. This is the base case, and the status change sort of oscillates around that one value for F over F30 in the terminal four years of the retrospective, and here's the spawning stock biomass benchmark. A lot of this uncertainty that we see here is encapsulated in our MCB analysis, which I think I go into next, and the removal of those age comps in that terminal year, that CVID uptick, all of that seemed to have a strong effect on the status in the terminal year.

DR. SERCHUK: Just a rather simple question, and I apologize, because I haven't actually read through the entire document, but when we're talking about the fishing mortality value, is that an average across all age groups in the population or is restricted to a certain fully recruited -- I am not really quite sure what the metric was, and do you know or can you explain it?

DR. SIEGFRIED: Here, specifically, it's apical F, and so that's -- Is that what you're asking?

DR. SERCHUK: I am asking how -- There are many ways to calculate what fishing mortality is. Some, you just used the fully-exploited groups and some you average over the groups and some are weighted by population size. I'm just wondering what the metric was, and I apologize for not going through the report, but because it can be expressed in many different ways, I am just wondering how it was expressed here.

DR. SIEGFRIED: Can I come back to that after, just to check on it, because I wouldn't want to say the wrong thing. Do you want me to do that like after the next break or something? Okay. I don't see how you guys could possibly read everything you're given.

DR. SERCHUK: While we're on this retrospective pattern, apart from trying to understand why it exists, why it occurs, this is a situation we often see in many other stocks. When a retrospective pattern comes, there's sort of a systematic bias, or you can call it a systematic error here, where you seem to underestimate F and overestimate SSB. This is what I'm getting from these plots, which suggests that where the stock -- The pattern suggests that, if it continues in the future, that we have underestimated F in this year, because next year, if the pattern continues, we'll find that F was higher this year than we thought and SSB was lower. I'm just wondering whether there was any discussion about how to interpret the stock assessment then, based on the retrospective pattern.

DR. SIEGFRIED: There was actually a lot of talk amongst the reviewers of how to calculate F in different selectivities, assuming certain age classes were subject to fishing and the different fleets. We traditionally use a geometric mean of the terminal three years to account for that concern of the terminal year estimate alone being used, and you would see that here if these two years were there, the actual two years estimated, that you would still have a qualitatively similar stock status, but I think the concern was just this jump down.

Otherwise, there's not really a pattern. There is no other retrospective pattern previous to that drop of that terminal year. Other patterns I've seen are much, much stronger and more severe than this, but I guess the geometric mean and also what the reviewers came up with for how to parse out F by age would be something the SSC could consider.

DR. BARBIERI: Fred, we do have a plan, and I was talking to Church and Steve Cadrin as well, that we're going to be discussing this in more detail as we discuss the review panel report.

DR. SIEGFRIED: Let me tell you about our uncertainty analysis. We have this Monte Carlo bootstrapping uncertainty analysis that we've done for stocks in the South Atlantic. You guys have probably all heard of it, but there's details about it that we need to go over for this specific stock.

First, the bootstrapping step. We create new time series of landings, discards, and the CPUE by assuming the mean value we were given from the data workshop and then the lognormal error around that is the CV, like from the standardization or from the CVs provided by the commercial group or the recreational group. New length comps and age comps are created each year by drawing the sample size in numbers of fish. Each fish is placed into a bin with probability equal to the original data, and so this is the way that we bootstrap new datasets to use for each of these runs. It's like doing 3,200 sensitivities at one time.

The uncertainty in the historic landings was an issue. The commercial group provided estimates. A decrease in value meant an increased precision over time, and so the historical time period had a 25 percent CV, 61 1961 to 1977 had a 20 percent, and 1978 to 1985 had a 10 percent, and then we assume a 5 percent, just for computational purposes, in the model.

Where there were state-specific values, we used Florida. It seemed to be what the commercial group deemed the best available information, the CVs I mean. The recreational group provided a CV on historical recreational catch, just all of the years, as 0.59. We applied a random scalar that's plus or minus one standard deviation to that whole time period, rather than annually, and I will show you what this looks like in a minute.

For recreational landings, we did the same lognormal error with the mean provided, the mean being the base point estimates provided by the working group. For headboat landings, we asked the headboat program leads to give us an idea of how uncertain those values were of landings, and so, in 1981 to 1995, we used a CV of 15 percent, and that indicates their feeling that there was better certainty than in the historic time period and than in MRIP, but before the mandatory reporting and full compliance for headboat. 1996 to 2007 had a 10 percent CV, and the improvement from 15 percent was due to mandatory reporting. Then 2008, they thought that a 5 percent CV was appropriate, because they thought they had full compliance.

For discards, neither the recreational nor the commercial group provided CVs for the discards. There was a CV provided for the discarding rate, but it wasn't exactly on the discards available, and so we used a CV of 20 percent, which is larger than landings, but smaller than MRIP discard uncertainty, because it seemed like commercial and headboat had more certain discarding information, because of logbooks.

The recreational group provided CVs for MRIP discards that were provided by MRIP, and we assumed that if a CV of -- We assumed a CV of one if there was a missing value or that just wasn't provided, and then we applied this, similarly, to the CVs described for landings, and so a lognormal with a mean of the provided value and the variation of these CVs.

This is it, by year. In blue is the commercial. This is what they recommended from 1981 on. All of the yellow is what we had to get opinions from the people who run the programs, people who have been in the program for a long time, to use that guess, basically. Everything that's white is either not included or an actual estimated value from MRIP.

The highest error comes from MRIP discarding, then MRIP landings, and then the discards from the commercial hand line and headboat fleet, and then headboat landings and then hand line landings. What this looks like, this is normally filled in, but I wanted to just do a few so you could see. This is the scaling that I was talking about in the historical time period. We didn't do any interannual variation for this time period. We just scaled the whole time period, but we did do interannual variability for the more data-rich time period. This is for the recreational.

For hand line, we did do that variability based on the CVs in each year. Here is what discards look like. They are more uncertain if you look here. This is the historical period for hand line landings. It's more uncertain than the modern period, and all of the time period for discarding is uncertain, and so all those lines show you a different draw of that bootstrap to use for the sensitivity run.

The Monte Carlo sampling, we had to decide on distributions of values of parameters that we thought needed to have uncertainty calculated here, and so we decided on natural mortality, discard mortality, and fecundity. The range provided by the life history working group at the data workshop was very small. This is, again, the asymptotic value, but the entire range was affected, all ages, and so it was really small, 0.12 to 0.14, around 0.134.

The assessment panel recommended an approach that would incorporate more uncertainty, and so what we did was the following. We used that Charnov age-dependent growth curve with the Then et al. estimator with the maximum age. The Then et al. data to estimate a and b were pulled off the paper and then drawn with replacement to calculate this equation here with the maximum age, and then Tmax was drawn from a uniform distribution provided by the life history working group, and we ended up with this. Instead of it being between 0.11 and 0.14, we have this nice distribution of biologically-reasonable Ms, but still using that meta-analysis to inform our uncertainty. This line is the value used in the base run.

For discard mortality, we had a range provided by the ad hoc working group before and after circle hooks were implemented, and so the idea was to draw from that range for period one, but then draw from a truncated range using the mean and range drawn in the first time period, the idea being that we didn't want discard mortality to increase with implementation of circle hooks, and so potentially we could have drawn a smaller value after circle hooks were implemented if we didn't

truncate those distributions, and so we put in this check to make sure we were actually decreasing discard mortality. This is examples of what the two curves would look like around that base run value, and so we were getting good draws and good distributions during our Monte Carlo step.

Then, for batch fecundity, we had this power equation that we wanted to sample from, and so we ran 10,000 estimates of a and b and bootstrapped the fits to those. The parameters are correlated though, and so they were drawn together with replacement, instead of separately, so we would get a reasonable fecundity estimate, and then any fits that were outside of the 95 percent confidence interval were trimmed, and this was similarly done for batches at age, and so we had a batch fecundity and then batches at age. There was a slight difference for batches at age, in that we applied it to fish length, the day of year, and spawning indicator presence.

This is what the batch number looks like, and so what I meant by all the action happens early in the ages. This is asymptotic after about age twelve or thirteen, and this is where we have most of the data. This is to address Luiz's comment earlier. The fit that we used in the base run is blue and the boot median used for the MCBs and the 95 percent confidence intervals are in red. The results came out, and we had pretty good fits to everything, and so I was just going to start going through those results, unless anybody has questions about how we constructed the MCB.

The top panel is all ages. The line with the filled-in dots in between is the median, and the gray is the 95 percent confidence interval from the uncertainty analysis. The top is all ages, and the bottom is just age-two-plus, and so we see there is uncertainty around that terminal year estimate of abundance, which is the majority of which are age ones, but we do get support of that from this MCB analysis.

The solid line in all of these plots is from the base run and the dashed is the MCB median. All of these are probability densities by whatever metric we have, and so we have F30, which is the proxy to FMSY, our proxy to MSY, our proxy to BMSY, and our proxy to SSB MSY. Our status plots are here, and so the dashed line, again, is the median, which is slightly higher for our SSB benchmark, but the F benchmarks are pretty much on top of each other for the base run versus the MCB median, and here is the annual plots with the corresponding uncertainty.

Here is the phase plot of all of the runs. We have 98.7 percent of the runs are in the overfished with overfishing occurring zone, and then 1.3 are in the no overfishing occurring, but still overfished zone. There is zero percent of those runs that say that the population is not overfished.

The last thing I had to go over were projections. These are the projections provided to the reviewers and the review workshop. There is also supplemental projections that you guys received from the Science Center at the request of the council after the review workshop. The projection scenarios that were in the terms of reference are the first five: F equals zero; F current, which the F current is the geometric mean of the last three years; F equals FMSY, or the proxy for FMSY, which is F30; F target, which I think that's 98 percent of F30; and then F equals F rebuild, and that's the max exploitation that rebuilds in the greatest allowable time, which, according to the rebuilding plan, was 2044. We added in another one, which is F from discarding only, and so what would happen if only discards were the source of fishing mortality for the stock.

The projections were run from 2015 through 2044. The structure of the projection model was the same as that for the assessment model, and we used the same parameter estimates. Any time-

varying quantities, such as the selectivities, were fixed to the most recent values of the assessment period, and so this would be all of the moratorium period selectivities, and then, for this set of projections, which is different from the most recent projections, a single selectivity curve was applied to calculate removals. It's averaged across all fleets using the geometric mean from the most recent time period. In the supplemental projections, we actually use the selectivity from the most recent time period for each fleet, independent of each fleet individually, instead of a single selectivity curve.

DR. BARBIERI: Katie, we have a question from Steve Cadrin.

DR. CADRIN: Katie, thanks. Just to clarify your last point, for scenarios such as 6, F from discards only, the flat-top assumption of the general recreational is not assumed for that and it goes back to a dome-shaped?

DR. SIEGFRIED: This is all the terminal block, and so it would be the flat-top, but, for this set of projections, it's an average selectivity for all the fleets.

DR. CADRIN: Right, and this is something that you may remember that we discussed at the review panel, is that, for some management scenarios, I think we need to carefully match the selectivity assumption to that scenario. As I understand it, the flat-top selectivity of the general recreational fleet was justified by targeting larger fish during the mini-seasons. If the mini-seasons are being considered -- If there is a management scenario that doesn't include mini-seasons, then the justification for that flat-top really goes away, and so it becomes complicated. I just wanted to put this out, that these are example projections, and I'm not sure that all of the complexity of the management scenario are fully considered in these.

DR. SIEGFRIED: Yes, I completely agree, and I anticipate more projections needing to be done specific to what the council is considering doing. I do think it's an improvement for us to not use a single selectivity curve in the most recent set of projections, and so we've already started that, because there may be differences in the way that the different fleets are managed, and so that's helpful. We can also look at this assumption of which selectivity curve is used, like you say, if there are no mini-seasons, or if they want to only look at discards in one fleet but not another. We have that capability and flexibility in this projection model, but yes, this is like the first stab at what it could be. This is also with fishing rates beginning -- That we were defining as beginning in 2017, and, in the most recent set of projections that the council requested, they started in 2016 and 2017, and so we can do all those different variations.

The initialization, for 2015, which is a moratorium year and there was no mini-season, the landings selectivity was set to zero, and the discard selectivity was rescaled to be the only form of take during that year. We solved the model for the F that matched the current dead discards in numbers, and so that's the mean of the most recent three years.

In 2016, it was similar, and that's assuming a mini-season would not occur, but this is purely because of things that we've been told by the council or council staff as to how to do a first stab at this, because of what the magnitude of the 2015 take was, but that can also be changed. The discards-only scenario treated the initialization year, 2016, the same as 2015 and then applied the mean F from 2015 and 2016 forward, starting in 2017, and so that's another assumption.

All these plots are the same, and so I just wanted to run through it with you real quick. The expected values from the base run are represented by the solid lines and solid circles. The medians are dashed line with open circles, and so you can see solid versus dashed, and so base versus medians. Then uncertainty is the thin lines around it, and it's the 95 percent confidence interval. The solid horizontal lines here are the F30 metric. The dashed horizontal lines are the corresponding median of that metric, and so this first one is the F equals zero, assuming all fishing completely stopped, and this is just sort of a theoretical thing to look at, what would happen and how fast would it rebuild if nothing else killed it besides M?

We see that the stock would recover, with 50 percent probability, in 2021, and then it would actually recover, with 75 percent probability, in a couple of years after that, but we're under a rebuilding plan and so we continue to go through the TOR projections, and so this is with F current, which is the geometric mean of the last three years, and the stock remains overfished throughout the entire projection period, and so this is saying with all the assumptions in this projection, if F stayed the same, it would remain overfished and remain at an overfishing level.

If we reduced that to F30, the stock still remains overfished throughout the projection time period, meaning that it never reaches 50 percent probability of recovery, and it doesn't with 2 percent less of that either, and so 98 percent of F30 is what F target is, and so it gets a little closer, but it's still not rebuilt.

The F rebuild, we're a little bit conservative here with the numbers. It rebuilds with like 0.49 percent probability in 2040, but then doesn't cross over the 50 percent line until 2044, and this F value is around 0.14, which is -- I think it's only a tenth of a -- It's smaller than F30.

The discards-only scenario is interesting, because it rebuilds much more quickly. It's a 50 percent probability of being rebuilt by 2025, and 70 percent probability by 2041. As Steve said, this was just a first stab to take a look at this methodology, and we fully expected more requests, and so that's the end of the main presentation.

DR. BARBIERI: Thank you so much, Katie. This was a long presentation and a lot of questions and discussion, and so we appreciate you bearing with us. It's twelve noon, and we've had a long and productive morning. Unless there are some burning questions from the committee right now for Katie, I would suggest we break for lunch, and then I think we were scheduled to return at 1:30, but my preference, until there is a phenomenal mutiny, is we return at one, because we really have a lot of work ahead of us, and this discussion is very, very important for us. First, any questions, burning questions, from the committee? If not, thank you and I will see you back here at one o'clock.

DR. BARBIERI: Welcome back to our afternoon session, and either Mike E. or Chip actually posted up there on the board our action items. Just as a reminder, we're going to revisit this pretty much explicitly as we go through our discussion this afternoon, and so I would say we get started by opening the floor to any discussion points or specific questions for Katie that you may have thought about during the lunch break, if any.

DR. SHAROV: I know we still have a full schedule, but very quickly, a couple of things. We tried, or the assessment tried, catch curves just for general orientation checkups and then the production model and the age-structured model. With respect to the use of the production model,

it provides quite different outcomes, or the conclusions that are made based on the production model are different.

There is a description or the assessment team characterizes the results by saying that this is an oversimplified model that doesn't account for variability in the recruitment, et cetera, et cetera. Have we learned anything, or is there a need to apply alternative models of a simpler structure in the case of red snapper? Do we ever need to do this? What's your take on the utility of it? Is there anything that we could learn from the use of the production model, in the case of red snapper?

DR. SIEGFRIED: I think that the production model shows how the catches and indices correspond, and so the place that -- The reason that I would say, for a species like this, that it's not would be because of time-varying aspects of the biology, of the regulations that have been put in place that affect our ability to model the species. Those size limits, we can't account for in an ASPIC model.

We do know that the recruitment is not the same value over time, which ASPIC assumes it is. It also doesn't take into account any sort of age-specific values of M, which all of the literature seems to indicate that we should expect smaller individuals to have more natural predation or lack of resources available, and so I would say the production model here is, as we build up more and more data availability -- This was a first-step model. There's other data-poor methods that could be investigated, but I think, when you have age data that you think are reliable, that this should be seen as a first step towards getting that fully age-structured model implemented.

I'm not sure what value it provides here exactly, but I've seen its value in the past, particularly when the age data were really sparse or uncertain. It would help us know whether the age-structured model was on the right track, even though the age data were questionable.

DR. SHAROV: I guess maybe the answer would be it tells us how wrong we could have been, had we not had the age structure and we said this is our best available information and we're using the biomass production model, and we would have been wrong. I don't know for sure, but, at this point, I guess that's probably the only utility, but we certainly should be considering alternative models, alternative hypotheses, and challenge our preferred models of choice, like the one we've used, because it might show up in the future that we've been wrong with this one as well.

The reason I ask only is that the model results were in the assessment package. They were part of the assessment, but, in the end, we didn't seem to use it, and so I was wondering if it was worth the effort of doing this or doing it in the future. Second, if I could, it's more general on the assessment results.

If we are correct, if we appropriately estimated biological parameters, then we have an idea what the stock should have looked like in the 1950s and the 1960s, given the natural mortality and maturity, et cetera, et cetera. We are estimating that, currently, the spawning stock biomass is very low, well below the target level, but it all gets to the point of whether the stock recruitment is dependent on the spawning stock size, and so it's a question of the reference point and whether the current reference point is appropriate for red snapper.

Given the range of these stock sizes, historical, that comes out of the model and the recruitment, we may conclude that the recruitment is largely independent of the spawning stock, which is not

surprising, because that's how we modeled it. Nonetheless, that's how we modeled it, but if the data were indicative of the relationship, we probably would have come up with the stock recruitment that would indicate that to get more recruits we need a larger stock size.

If the current stock-recruitment relationship that comes out of this model is indeed representative of its nature, then, therefore, we may conclude that we don't need that much of the spawning stock to have a level of recruitment and the level of variability that we've had in the course of forty or fifty years. In that case, maybe the SSB 30 percent requirement is too ambitious, and so that's the challenge, because we obviously would not know the true answer unless we double or triple or quadruple the SSB and see if any increase in recruitment will follow. Nonetheless, that's probably the open part, the inconclusive element, of the assessment that probably comes out of the results. Would you agree with that or not?

DR. SIEGFRIED: As far as how much of the spawning stock biomass needs to be there, the plot that I showed doesn't -- Obviously we're not arguing that it needs to be back up the, quote, unquote, virgin conditions, and that 30 percent was something that the SPR at 30 percent was what the SSC recommended last time, and so it's not really something that the assessment states what we should be trying to get at, but it does model -- It shows how much -- Maybe I'm bouncing around your question, but it shows how much of each of those age classes need to get there, given what will survive to those age classes.

I'm not sure if we need less, based on the reproduction now. I'm not sure. I mean we ran with it those benchmarks that have been provided by the SSC. I have had people ask me about F40 and Fmax and F20. I know the Gulf is F26 percent or something like that, and I'm not sure how much of that the assessment can truly answer. It seems like it's a level of risk for the SSC to consider, unless I'm missing part of your question.

DR. SHAROV: No, that's the essence of the question, and I accept your answer. Thank you.

DR. BARBIERI: Thank you. Fred, before we go to you, we have Anne Lange who is on the webinar, and she had already sent an email.

DR. ERRIGO: Anne sent me an email, and, Anne, hopefully you're on the webinar. You should be unmuted, if you need to make a comment. She's not. I wonder why. She is unmuted now, and so you should be just muted on your end.

MS. LANGE: Thank you. This may be not the right time at this point, but my recollection, from my days back working with ASMFC, was that there was a significant impact of the shrimp discards on red snapper. I noticed in the stock assessment report that the commercial working group indicated that there was no significant impact in that fishery, but the dates or years that it's discussing are close to 2007, and so I'm wondering if some of the not fit and the uncertainties in the earlier years may have been because of bycatch of the juveniles as discard in the shrimp fishery.

DR. SIEGFRIED: I was actually part of the shrimp bycatch estimation in the Gulf, as it happens, and the reason that they decided that it wasn't a big deal was a couple of things. It was very few occurrences in the shrimp bycatch history of red snapper in the South Atlantic. They hypothesize that that's because there is relatively little shrimping effort on the shelf, as opposed to in the Gulf,

where it's all shrimping effort on the shelf, and so we didn't have any data that showed that this was a big problem, contrary to having lots of data in the Gulf.

MS. LANGE: Again, I'm just -- My recollections may be wrong, but I remember fairly significant discussions and concerns, again back in the 1990s, and I don't know if Carolyn may have some recollection. I think Spud was involved in those discussions back then, but, again, I may be wrong. I just thought I would raise that, because there is some question about whether there was higher natural mortality back then or not, and it could have been from discards, but I'm not sure.

DR. SHAROV: Anne, I think you're thinking of the Gulf. That was a very significant issue for the Gulf of Mexico red snapper, but not for the South Atlantic.

MS. LANGE: Again, this was relative to my work with the Atlantic States Marine Fisheries Commission, and specifically working with people from Georgia. This was prior to my working with the Gulf Commission.

DR. BELCHER: Anne, I would have to check with Spud on that, because that was pre my time there, but I can fire an email and see if I can find information on it.

MS. LANGE: Okay. If no one else is concerned about it, it was just, again, something that, reading through the document, it noted 2007, and I just wanted to make sure that if there had been impact prior to that that it was considered. I know it was very important on the Gulf coast, but I thought there was also some impact in the South Atlantic. Again, I could very well be wrong. It's been a while.

DR. SIEGFRIED: There are other species, Anne, in the South Atlantic -- I have actually calculated shrimp bycatch for vermilion snapper, and so there are other species for which there are enough data to calculate shrimp bycatch, but there weren't enough occurrences for red snapper.

MS. LANGE: Okay. Again, I just wanted to raise that point, because it was my recollection. Thank you.

DR. BARBIERI: Thank you, Anne.

DR. SERCHUK: I share the concern that was raised by Alexei about the spawning stock recruitment curve. It's very variable, and I would think that when we discuss appropriate reference points -- I think we should think about this. There was also a yield per recruit curve that was presented, which indicates sort of an Fmax at 0.2. I'm not saying that would be an appropriate point. You might want to be a little bit -- Taking F 0.1, or you might want to take another point off of that.

There's nothing sacrosanct about the reference point based on a S-R curve this uncertain, quite frankly, and so I would think that -- I have nothing wrong with what's been done, but I think it's not a very precise way of going about it, and so I would urge the committee to think about not putting all of our eggs in one basket and thinking about things like F equals M as an approach, yield per recruit and other ways of looking at it, so the totality of the evidence that we have, if we get to that point, is buffered by a number of different lines of evidence. Thank you.

DR. BARBIERI: Thank you, Fred. Good points for us to generate some discussion. Actually, both your point and Alexei's is handled through the little summary that myself, Church, and Steve, you know we kind of put together a brief -- I think we are ready to go there, unless there are other specific questions for Katie. We can go into discussion of the --

DR. BELCHER: Kind of going off of some of the earlier comments, and I know Fred hit a lot on it with the ages, I understand that the reliable age estimates -- However, at what point is the sample size sufficient? I am kind of still in that category with Fred that only 5 percent in that plus group, whether it's thirteen-plus, ten-plus, twenty-plus. Only 5 percent of the observations are above that, yet they represent more than fifteen ages. It's great that they're valid, but is it sufficient enough to categorize the age groupings?

DR. BARBIERI: Good point. Again, this is just to guide our discussion this afternoon, and there is nothing new here that is not already in the review panel report. This is basically a copy and paste from the review panel report, just to bring up some of these things and help us go through our terms of reference, I mean our action items there, for the report. I am going to impose on Steve and Church and ask them to jump in whenever they feel like they have something to add.

There are many uncertainties in this assessment. We've talked about there is a number of parameters being fitted. I looked here at Slide 63 from Katie's presentation, and there are 365 parameters being fitted, including 259 values of F, and the data quality is questionable along the way, and so there is a balance there that it's tough to be met, and it's bringing some high degree of uncertainty into the picture.

As Alexei, Fred, and others brought up, the stock-recruitment relationship is unknown at this point, and probably it's very poorly informed. Selectivities for the different fishery fleets, you know you remember, during Katie's presentation, that she was very explicit about that, how difficult it had been, especially since 2010, as implementation of the moratorium sort of reduced the data available and the informational content to inform estimation of those functions.

Composition and magnitude of recreational discards that we know is really very highly uncertain, and the strong retrospective pattern that, especially for fishing mortality, that kind of brings some uncertainty, especially on the exploitation status. I am going to discuss some of those. The stock-recruitment relationship couldn't be estimated, and so we're using proxy reference points. The bottom line is we have poor or equivocal information on stock productivity, and so this most recent discussion there, regarding the reference points, we can see the sequence of three benchmark assessments for red snapper have come up with very different estimates of MSY and its proxy.

It's not necessary unexpected, but it really, in my opinion, highlights the difficulties in estimating that productivity of the stock and what that value of MSY should be. Depending on how you configure the model, depending on how some of those parameters are estimated, you're going to end up with very different outcomes. Steve and Church, anything to add there?

DR. CADRIN: I think that's a fair summary. The things that were most important to me were the assumed shift in selectivity of the general recreational fleet during the moratorium and mini-seasons. I thought that that -- Even though the sensitivity analysis suggests that there was just that one aspect and all the other parameters weren't freed up, and so I'm still concerned about that. The other was the use of age and length at the same time. That had come up several times.

Despite all these things, we did agree that this was an acceptable assessment, and probably the best that we can do. We really wrestled with interpreting how fishing mortality could be still overfishing during a moratorium with minimal mini-seasons, and so there are sources of uncertainty, as you said, but we did accept it.

DR. GRIMES: I thought your summarization of it was fine. There were a couple of weaknesses, more biological things, that I thought were there that I would have added to the list. I mean I guess the estimate of M by that methodology is -- The assessment is sensitive to that, but they all are. I mean that's just pretty typical, I guess.

Early maturation of two-years-old seemed odd, weird, to me, as Fred mentioned, but maybe that was a compensatory thing, as a response to extreme juvenation of the stock. I would like to see -- I wish we had better information about the depth distribution of large fish, which we had a lot of testimony that that wasn't really captured, and the small age sample sizes also I thought were an issue, but, all in all, it's very well done and carefully done, and I think we had to accept that it was the best available science.

DR. BARBIERI: Thank you. Selectivities were also an issue. Poorly-informed assumptions regarding recent fishing behavior, that discussion came up repeatedly during the review workshop, as it's very difficult to measure how the fleets were adapting their fishing behavior to the new conditions started since the moratorium.

There was that shift from a sharp-dome to flat-top in 2010, which, of course, is having a major, apparently, impact on the outcome of the assessment, and the information content for that is, of course, much reduced compared to previous years, and this is one of the comments that came up that's actually explicitly in the review workshop report. Older and larger fish sampled in the recent period couldn't be explained by dome selectivity, and the greater reductions in fishing mortality, the increasing catch of older ages, could only be explained with a shift in selectivity, and that is sort of pushing that sort of fait accompli of how the selectivity is turning out.

SSC MEMBER: Could that be explained by, during these mini-seasons, people high-grading the recreational catch? Is that one of the potential explanations that you had discussed, since they only have one fish per person per day during the mini-season?

DR. BARBIERI: Yes. So then --

DR. JOHNSON: Just one more thing along the lines with that. I think Steve brought it up earlier, but not only does that reflect what's actually going on, but in the projections, moving that forward, if the management style changes and those sorts of things. As the stock rebuilds and gets older, then that selectivity is going to have big impacts.

DR. BARBIERI: A question that Fred has asked before on the fishing mortality metrics and how comparable they are in regard to each other along that time series, we can see there some results that are kind of difficult to reconcile with the observations, in that since the moratorium, there is this very high exploitation on the older age classes. I can see how, size-wise, this would be the case, but there is very little evidence, from the age composition of the actual harvest, that that's the case. You can see how much that fishing mortality for ages five and higher, how much that increased, and, to me, that's a major source of uncertainty as well.

DR. CADRIN: This was fairly complex. I think, as Katie correctly characterized, with different fleets, different selectivity blocks, there is a lot of dynamics going on with fishing mortality, and apical F is one measure of fishing mortality. That's the basis of how the model is estimating fishing mortality, but what we are recommending, in the review panel report, is that the council consider alternative metrics for fishing mortality status, because, as this plot shows, and this was one of many, there is one pattern of fishing for the young fish, and especially in recent years, a completely different pattern.

This really came out of us wrestling how to interpret this increase in apical F, and we were able to determine that the moratorium was relatively successful for decreasing fishing mortality of young fish, but the move to older selectivity has really increased the fishing mortality of older ages, and so this is a complexity of the model that I think needs to be considered by the SSC and the council. It's not necessarily a source of uncertainty. It may be a reality that there is changing selectivity among the different fleets.

DR. SERCHUK: I guess when I look at this representation, I see a different picture than what is described. I am seeing that since 1989 -- In both cases, except for the most recent two years, that both the average for the older fish and the average for the younger fish are fluctuating without trend. It's only within the most recent year, couple of years, that we see a decline in one and maybe an increase in the other, but, apart from that, for the last twenty-five years, from 1990 to 2015 or 2014, I'm seeing just fluctuations going on here. I see some dip here, because of the moratorium, but, beyond that, I don't see a pattern. Would that be a fair interpretation?

DR. BARBIERI: I think so. The question, to me, is the magnitude of increase in the estimate of F, and you think about this as an exploitation rate relative to the previous years, and that becomes very difficult to be compared, because you have different selectivities and you have different catchabilities as well, but it's a jump in fishing mortality for those older ages that, for such a poorly informed selectivity function since 2010, in my opinion, it's really not credible.

DR. SERCHUK: My point here is you try to reconcile this with sort of the natural history of the fish, we have a yield per recruit curve that suggests that 0.2 is the maximum yield per recruit. We have Fs that are fluctuating from twice or higher on that, and that is not good for a stock. You can talk about the scaling, but I'm not surprised that we're not getting a lot of recovery here, based on that.

If you just take it at face value, what the natural mortality is, what the yield per recruit is, and what we know about an animal that lives at least until twenty years, and, according to the report, can live as long as fifty years, generally you need, for sustainability, Fs around, on average the natural mortality or below, to sustain that longevity, and so I'm not surprised at that.

DR. BARBIERI: Right, and I'm thinking about -- I mean we have two stock status from the biomass, and there's a long rebuilding in place, and there is an F rebuild report that will cause that stock to rebuild and the age composition to refill. I'm just thinking about, since a moratorium, as we discussed the exploitation status, not thinking our rebuilding trajectory, because that's a twenty-year or thirty-year to rebuild all those age classes, but it's the current exploitation status, in terms of the fishing mortality rates. That's the point.

Recreational discards during the most recent years of the stock assessment series, our recreational discards are one of the most important, most uncertain sources of information. It is troubling how the PSEs of recreational discards from MRIP are much, much higher than what they were to be assumed for the assessment, because of the proportion, the fraction, of total removals that is actually coming out of discards.

There wasn't really a way, as we can handle the uncertainty about recreational discards, the way that we handle that, to be more in line with the true uncertainty in those estimates for other assessments that basically, by setting a CV at 5 percent for the recreational discards, we're telling the model that this is a fairly reliable source of information that we know very well when the data indicate otherwise, and I understand that there is a practical component of this to be able to get model convergence and to get a solution, given the structure of the model, but, to me, this adds to that uncertainty and the equivocal nature of the exploitation status. Steve and Church, anything to add or discuss further?

Then the retrospective pattern, and Katie went through that in a fair amount of detail, a substantial upward adjustment of recent Fs, with the addition of the 2014 data, and one of the reviewers, the CIE reviewers, pointed out explicitly that if you remove 2014 and the recent Fs are down to around the F30 reference point, and so it's indicating, despite the use of that geometric mean, there is a fairly high degree of uncertainty in the exploitation status of the stock, and that's demonstrated clearly by the strong retrospective pattern there. Unless Steve or Church --

DR. CADRIN: Just one on the retrospective pattern. The one thing we discussed was that, with such a short video trap survey, the retrospective analysis is not as stable -- It shouldn't be expected to be stable, because we're really making it a three-point series, a two-point series, and those aren't really comparable, but the survey doesn't have the information content that it has in the terminal assessment, and so that's one caveat to the retrospective, that there does seem to be a jump in the last year, but that's probably because that survey reaches the critical period needed to be informative.

DR. BARBIERI: Good point. Church, anything to add? With that, if we can go back to our action items. We have three main points, three main bullets there, in terms of review the assessment, identify and discuss assessment uncertainties, and then we go to providing fishing level recommendations and going through those sub-bullets. Chip I think is going to help us collect some notes on your comments and/or recommendations on these topics. With that, I will open the floor for discussion of our action items.

With no major discussion points then, let me go bullet-by-bullet and see if I can get some feedback. Does the assessment address the terms of reference to the SSC's satisfaction? You saw the basic set of terms of reference for the review workshop. I am hearing yes from a couple of members, and so that will be yes. Now let's make sure, as we build our report, that we add a little bit more, as I circulate the draft report, so we can kind of flesh this out a little bit more. Does the assessment represent the best scientific information available? That was the conclusion of the review panel and the recommendation of the review panel, despite the uncertainties.

DR. BOREMAN: I'm still wrestling with Number 1. Define "satisfaction". I mean was it good for you? Is it one of those things or what?

DR. BARBIERI: I think it's a qualitative application of your sort of professional judgment. I mean there's things that we provide input and we, as a group of scientific advisors, have a few things we can quantify and some we can't, and the composite of the assessment workshop report has a general outcome, and each one of us will have our own different sense of satisfaction, but I think this hasn't really addressed what we need to have in terms of the scientific validity of the results, the metrics of uncertainty, and whether we have what we need to provide fishing level recommendations to the council and scientific advice to management.

DR. CADRIN: I agree with John that that's a bit subjective, but I can speak for myself, from being on the review panel, and possibly the review panel, that we were satisfied that the data workshop, assessment workshop, and the leads had given full attention and any of the limitations of the assessment were not for lack of trying. It's more a data situation, complexities with the fishery and the biology, that present the problems. I think that this is the best assessment we can ask for with the information we have.

DR. BOREMAN: What I was looking for, obviously, is something more than just yes, to put some qualifiers on that.

DR. BARBIERI: Right, and this is why I was asking, John. I mean even if we cannot come up with those right now, it will be good for our report to have that yes sort of fleshed out a bit and add some stuff, just because this report becomes then available to everybody, including the council, as far as understanding our review of the assessment. For Bullet Number 2 there, I heard yes for the best scientific information available. Does the assessment provide an adequate basis for determining stock status and supporting fishing level recommendations?

DR. BELCHER: Obviously it's after the fact that the review and all has been done, but just back to when Alexei was talking about the ASPIC. Understanding that the reason ASPIC was run is part of the data diagnostics, I think with the fact that a lot of people out there recognize that as an alternative model and that it was done for complementary purposes versus comparison purposes, but there really wasn't a compare and contrast of results there, it's going to be very difficult for people to look at that and see two models with both very opposing outcomes, and yet there is very little discussion of how that reconciliation was made.

I mean that was kind of -- I was looking for more dialogue and discussion other than we had ages available and so we went with age structured. I didn't feel that -- I think a little bit more in-depth would be helpful, because that really is going to be a hard selling point with those two methods showing that much of a difference.

DR. BARBIERI: Right, and a point that John Carmichael has made in the past, and I guess is for us to discuss here, is this thing that we've got to be attentive to our terms of reference, and maybe this is a suggestion that we could make, because I think the review panel could have provided more information contrasting those two, and so it's just something for us to be attentive as we get new assessment TORs in front of us, to ask for those things more explicitly, and then I think we get a more detailed compare and contrast.

DR. BELCHER: I know Fred has got his hand up too, but, to that point, I think that's harder, because it's almost like after the fact you have to look at the -- You have to look at it after. It's not something that's like ahead of time we can say alternative models, because then what will

happen is if somebody puts due diligence to them -- Like in the case of ASPIC, there are other things you can do to the ASPIC model to change it up. It's just not that cut-and-dry. You can add indices to it and you can change out some other parameters. I mean it's more flexible than what was used, but to what level are you going to investigate that time into it?

That's the question, is the more competing models that are there, the more time that has to be put into both models. You can't just have one that's kind of done as a -- Again, I understood, from what Katie's presentation was, it was kind of a diagnostic for them, in certain senses of it, but that's not what most people are going to see that as. They're going to see that as, like we said, is this a competing model or not, but it wasn't as adequately fleshed out, nor was the attention to detail given, to say, well, did you think about ASPIC and adding the following components into it. That was never given that option, and so I think, without the after effect of looking at it, it's not something the TORs is necessarily going to be reflective of, unless we can start forecasting what we think is going to go wrong.

DR. BARBIERI: So any suggestion on how we address this for any future assessments?

DR. BELCHER: I think the clarity of, again, if it's for comparison purposes, and, to me, there's a compare and contrast that I'm expecting to see, and not just that it's a complementary data source, giving us some degree of information that we can talk about what are the take-homes you get from running it in that elementary form, to help inform to go to the next step. I mean that's where I kept felt -- Either, one, it's part of the data process, or, if it's done in the assessment workshop, to me, that's saying it's a competing model and we need that compare and contrast of why one is more acceptable than the other.

DR. SERCHUK: To a certain extent, Mr. Chairman, I share the concerns that have been just raised. On the other hand, if the ASPIC model gave a similar result, my response would be to stop all data collection, because, quite frankly, the assumptions behind the ASPIC model, which are very large and very numerous, apply to this stock, and I think we have reason to believe that they don't, that the reason we get different results is because the assumptions behind the surplus production model are not being met, in reality, because the population is much more dynamic than that.

I think some of that has to go in, some of that reasoning has to go in. As I said, if I were a manager and said, gee whiz, you got the same results with an age-structured model that you did with an ASPIC model, I would say no more data collection, because the catch will tell us what the dynamics of the stock are. That would be very short-sighted, and I hope someone doesn't come to that conclusion, because I think the data collections are providing some insights into the tomography of the population and on the impacts of the exploitation. Some language needs to be crafted along those lines, I think, to buttress the decision to use an age-structured or a length-structured model. Thank you.

DR. BARBIERI: Thank you, both of you, for bringing that up. I am still waiting on some committee input for does the assessment provide an adequate basis for determining stock status and supporting fishing level recommendations?

DR. ERRIGO: Maybe we can just break it down, if it helps. Does the committee feel that the assessment can give stock status? Do we believe the stock status from the model? Then we can

address fishing level recommendations perhaps separately, since they could be two different things. Does that help?

DR. SCHUELLER: Given all the uncertainties and sensitivities that were done and the fact that the stock status was the same regardless, it seems to me that it's adequate.

DR. REICHERT: We can turn it around. If anyone believes that it's not adequate, what's the basis of the fact that we feel that it's not adequate?

DR. BARBIERI: In that case, I have to say that I do not believe that this assessment can provide exploitation status for the stock. I think that there's too much uncertainty on that exploitation status. I think this is one of those situations where -- Erik brought this up yesterday, regarding the research track. This is an assessment that is facing so many challenges. I mean Katie did a great job going through in very much detail a lot of what was handled, and I don't think we should interpret this as the assessment was not well done.

I told Katie and the rest of the Beaufort team at the review workshop that I thought they did a phenomenal job, a fantastic job, given all the limitations that exist, but the whole reason, in my opinion, for the SSC to have this role, despite the fact that we have a technical review workshop that takes place and has CIE reviewers, is that we contextualize, really, the assessment results with the realities that we see of how the fishery is conducted in our area, and we contextualize this for management advice.

In this case, I feel that the biomass status is unequivocal. Obviously, this is a long-lived species that will require a long rebuilding plan, and we've seen this happening for the last ten-plus years in the Gulf, and how the biomass is increasing and the age composition is rebuilding over time. To me, my problem is really with the exploitation status, because I can't reconcile, in my mind, that we had, during the moratorium, that we ended up with fishing mortality rates that are two-and-a-half times what is sustainable, given all the uncertainty that we have about the actual magnitude of discards.

Not reflecting on what the assessment team did or the quality of the science by any means. It's really a phenomenal challenge to get to the bottom of this, but I can't reconcile that in my mind and stand before the council and tell them that I believe this to be true, because I honestly do not, and so that's my own personal opinion, but I can't accept the exploitation status, as determined by this assessment. I just don't see it being plausible.

DR. SCHUELLER: I guess that's under the presumption that -- There wasn't a moratorium for the entire time, and so I guess my thought about it is there's a lot of discarding that accounts for it and there's a lot of recreational effort, and those mini-seasons perhaps weren't as well controlled or controlled at all, and you don't really have a good idea of what was out there, and so it's possible -- I was waiting for you to give your reason on why you don't believe the stock status with respect to the fishing mortality rate. Does anyone else want to weigh in?

MR. CARMICHAEL: Could you drill into that a bit and say -- Can you draw a distinction between qualitative versus quantitative determination? Does the SSC perhaps believe overfishing could have been occurring, though you can't determine the magnitude, and you're maybe not accepting the 2.5 times estimate, but do you think the majority of the information suggests that overfishing

was likely occurring in 2014? Perhaps it wasn't occurring in 2013 or 2012, but was it likely occurring in 2014?

DR. BARBIERI: I think, to Amy's point, I mean is it possible -- I don't think it's impossible. I don't think it's likely, given all the conditions, and if we are talking about providing scientific advice, based on those metrics, the magnitude really matters, because we are actually assigning a value to the degree of overfishing that I don't really have any confidence on, and I don't think it's very likely.

DR. SCHUELLER: I don't know that I agree. I feel like the weight of evidence is this fish can live a long time. The maturity, as Church said, it's very young right now, and that may be because the population is so truncated. If that's the case, then even a small amount of effort can be providing a fishing mortality rate well above what it should be. I mean the weight of evidence of all the life history data and everything suggests that overfishing is occurring.

DR. SERCHUK: I think we have to be very careful here, and I also think that probably should have been a term of reference for the review group, Number 3, because they put forth an assessment model, and if they accepted the assessment model, then the next question would be, do you think this is adequate for characterizing the status of the stock? That shouldn't be a question that we should have to answer in vacuo here, but I don't know whether they did or not.

If they came out with an answer and the answer was yes, we think the model can do that, then we have to have a really compelling argument to go against that, because they spent a lot of time doing what they did, and they did a lot of sensitivity analyses and so on and so forth. My point is, though, that really we should be seeing something in terms of the age composition expansion. If you believe that exploitation has been low enough, we ought to be seeing significant increases in the older age groups. From what I'm seeing, we haven't seen that yet, and so we're faced with an issue, and one could say, well, we don't see it because we're not sampling in the right way. Well, I don't know. Or we could basically say it's too early to see it, because this has only happened the last few years.

When I look at the diagram that you put up, I don't see any change in fishing mortality in either of these groups, and so that's a guess that either there is mortality that can't be explained by the sampling. It can be explained by maybe natural mortality rates have increased for all the age groups, if we're assuming a constant natural mortality, or we can assume that even on the twos and threes and fours that you're still having a very high M. Maybe it's not 0.6 anymore, but it's down to 0.3, and so any additional F, even if it's 0.1, is going to mitigate in a significant way against seeing really pronounced increases in the older age groups.

I am very reluctant to say that we don't know what the status of exploitation is in this case or the status of the population, and that's my feeling, but I would say to the committee, if you feel otherwise, we better have a really watertight, compelling argument to go against what the review group has said. Thank you.

DR. BARBIERI: Before I go to you, if we can go back to Katie's presentation and put that diagram that we have of the biomass and the numbers, and all I'm thinking about here is a rebuilding plan and a rebuilding trajectory -- I'm not discussing that this stock doesn't need management measures or rebuilding over time, by any means. We have rebuilding plans and we apply regulation to a

whole variety of stocks, but if you look at that graph on the left, you can see that from 2010, or thereabouts, you do have additional ages.

The biomass is increasing, and additional ages are being added, and so my interpretation of that -- I mean even on the graph on the right there that's much more truncated, you see those pieces of green and light blue, and so my interpretation is if we do fish at F rebuild, are we compromising the rebuilding of the stock? This stock has a rebuilding that's to be completed in 2044.

I don't see there, by that graph, and maybe I am misinterpreting that, any indication that right now the fishing mortality rate that's being applied on this stock is preventing biomass from increasing, one, and, two, additional ages to escape or survive and the age composition to rebuild over a long period of time, because we have so many ages in the population.

DR. SERCHUK: Can we go back to the figure before this, because fishing mortality doesn't operate on biomass. It operates on numbers. It's the numbers that I'm concerned about, quite frankly, because I don't see the translation, in any one year, of very good recruitment to a comparable -- I'm not saying the exact size, but I'm saying if you have excellent recruitment, you expect the next year that you're going to see a significant increase prior to that in the two group and then the three group, and I don't really see those things coming along in this graph, and that's what concerns me more than anything else.

The biomass is little bit misleading, because it brings in growth, but if you're fishing on -- You don't fish on biomass. You fish on numbers. It's a mortality rate on numbers in the population, and I see these big recruitments coming in, and I don't see the next year, the next few years, seeing this big recruitment being translated into sizeable increases in the next older age groups, and that's what really concerns me about this.

DR. BARBIERI: Fred, not to generate a back-and-forth, but I disagree. I mean if you look there from 2010 to 2014, these are only four years of data, but I do see additional colors on the top of those bars.

DR. SERCHUK: Sorry, Mr. Chair, but I just see it differently. I see that it's very large numbers of ones, and I don't see them coming in in comparable ways to the twos and the threes. Now, again, beauty is in the eyes of the beholder, and so I'm not going to go on ad infinitum on this, but I just see it differently than you do, Chair. Thank you.

DR. BARBIERI: I appreciate that. I mean I think this is the value of this committee, is for us to bring these things to make sure that by the time that this goes to the council that it's properly vetted and discussed.

DR. CADRIN: I want to both answer Fred's question and also clarify what I think you're proposing here. To answer Fred's question, the review workshop did have similar terms of reference. Term of Reference 3 was to evaluate assessment findings and consider the following. 3a is are abundance exploitation biomass estimates reliable and consistent with input data and population biological characteristics and useful to support status inferences?

What we concluded was that we accepted the new base model with the corrected age compositions, with the video survey index, as the best available model to provide advice to the South Atlantic

red snapper fishery. However, the review panel did have some concerns, and so the text there, on page 495, goes into those concerns,

Term of Reference 3b is about the overfished status. I don't think that's really what's being considered here. Going into 3c, is the stock undergoing overfishing and what information helps you reach this conclusion, the review panel could not find any evidence against the overfishing determination in the assessment, but it did have a number of concerns that are discussed below, and that is on page 496.

To answer Fred's question, they were addressed. We accepted the base model and both the overfishing status and the overfished status. Now, as we can see on the right, we as an SSC have similar terms of reference, and, as I understand the process and National Standard 1 Guidelines, the SSC can deviate from the peer review if it provides justification for that deviation, and so I think that's what is being proposed, is that the SSC has the flexibility to conclude something different than the review workshop did, but just to -- I was seeing nods from Luiz, and so I think that's what he is proposing.

While I've got the floor, I will just mention, to the left, there has been quite a bit of discussion and view of this figure, which really I think might be somewhat misleading, because the first twenty years of that are at very, very low Fs, and so that's more of an unfished age structure, and Katie did provide the age structure that the council is aiming at with the reference point, and we're actually much closer to the equilibrium age structure of the reference point than we are to those unfished age structures.

MR. CARMICHAEL: I guess then the question is the magnitude to which overfishing may be occurring is a number in a table, but it's not a factor in your control rule. It is it is or it isn't overfishing, and it's not a factor in how management will proceed, because the projections are based on a particular F level, which is based on whatever it takes to rebuild the stock. You know you're not in one of these situations where we may have been years ago, where you can have some reduction in catch based on how bad you thought you were overfishing at this point in time.

To some extent, the magnitude -- It's a number in the table, and that's one of the reasons we ask for that, to fill out the table which comes after this, and so it has some value there, but, in terms of what you do next, it may not be that critical to know the magnitude, the actual quantitative F over FMSY, whatever it may be.

Sort of back to the core question here, as Steve said, you're asked to do these things, and so is the proposal that overfishing is not occurring, that it's unknown, or that it could be occurring, but we don't know the extent? I sort of see those as the three options here, and so it would be nice to maybe hone in on where your proposal might be headed, in terms of an actual recommendation for the council on status.

DR. BARBIERI: Steve, before I go to you, just to clarify. Basically, I am looking at this, trying to look at this, as objectively as possible. I mean this is quantitative assessment in a very well-developed and very complex model. It's generating metrics for us, and I have to look at them and I cannot just, on one side of my brain, take them quantitatively and on the other say, well, but I won't take into account the quantitative results, because, as an SSC, I am here as a fisheries scientist that is looking at this assessment as it is presented.

That probably represents my main problem here, and I'm not saying I know where we are, but I'm saying there is enough problems there, I think, that I'm finding it difficult to have this sort of pass the red-face test and say, okay, we are at 2.5 times the sustainable and this is a well-estimated quantity that --

DR. CADRIN: Thanks, John. I initially thought you threw us a life preserver, and, as soon as I grabbed for it, I found myself sinking again, but I think there is something there. Primarily, we're being asked for an ABC recommendation, but the part of it that we still need from this is that is management needed to end overfishing, and I think this gets more at your magnitude. We're two-and-a-half times overfishing, and so, again, I'm just trying to move forward on this, because I think, really, the more important determination that we all accept is that it's overfished and that we're in a rebuilding plan and our ABC needs to be consistent with meeting our rebuilding targets.

However, I think the council will need some recommendation from us about the overfishing status, to determine if status quo management is enough or not. In my mind, it's not, that reductions are needed to reduce F further. Whether we're at two-and-a-half times FMSY now, that's the part I think that we do have concerns about, and so perhaps we do need to put a little bit more finer point on it. We can't just shirk this term of reference.

We do need to say that -- We've already said we agree that it's overfished. We may want to say that we're also concerned that fishing mortality may be excessive. However, the magnitude of F from the assessment may not be reliable enough to determine the magnitude of reductions that are needed to end overfishing.

DR. BARBIERI: May I just address that point real quickly? John, and hopefully I'm wrong about this, but that was exactly my point, Steve, is because, in all other examples that I've been involved in, I mean that number actually means something as far as the amount of reduction that has to be accomplished, has to be achieved, for us to be in agreement, in compliance, with not just the Guidelines, but the Act as well.

MR. CARMICHAEL: You have projections at F rebuild, and so there is a difference there between whatever F rebuild is for 2017 and whatever F was for 2014. Yes, it's in there, but I mean I don't know that whether you decided it's two times overfishing or one time overfishing -- I don't see that, at this point, that changes the F rebuild for 2017 that's in the projections. Is that correct?

DR. BARBIERI: I don't know how the attorneys are going to respond to this, as far as the Regional Office and compliance with the Act, and so no, I don't think that this would influence F rebuild, and I'm satisfied, really, with fishing at F rebuild over the next thirty years and let's rebuild this stock. My concern is when they ask me, if we are undergoing overfishing, how much do you have to reduce then? What metrics are put in place to actually bring you into compliance, because ending overfishing is supposed to be immediately, as far as I understand, and so that's my issue with the exploitation status.

MR. CARMICHAEL: Just to that, I mean the reduction is to the yield at whatever is at F rebuild in 2017. We have before us the yield that keeps overfishing from occurring in 2017 in the projections, and so the reduction is there. If you look at the assessment's estimate of what the total kill was in 2014, the yield at F rebuild is quite a bit lower. Even if we don't know to what extent overfishing was occurring in that year, I think the yield is still standing there.

DR. SERCHUK: I don't disagree with the approach, but I think if you're in for a penny, you're in for a pound, and my feeling is you can talk about we don't believe that F is as high as it is, but you will start your projections using the stock size that's based on having an F of 0.5. You discount it, but you use it, and I'm also worried about how it's going to be monitored, because you might say, well, we'll keep it at this level forever and ever, which basically says I accept the F that came out in the terminal year, or you're going to do another stock assessment in the future.

I'm also worried about the metric that we're going to be using to assess whether we're actually being complaint with the F that we think we're after, whether that's current F, half of current F, and so on and so forth, and so I think we've got to be careful here. The projections are based on the assessment. The assessment is telling us this is what F is. We know that there is uncertainties in the assessment, but if you accept the assessment going forward, implicitly you're saying that this is what the Fs are that generated the population size that we're going forward with.

I'm not a lawyer, and I can tell you a story about lawyers and how they look at things, but I think, in this case, if we accept the assessment, then we're really accepting the Fs that are in the assessment when we use the projections to go forward, because those Fs have determined the population sizes that we're using, whatever metric we want for fishing in the future, whether that's F30 percent or whether it's F20 percent or whether it's F0.11 or whatever, F rebuild. It's based on the stock size that we have going forward. Thank you.

DR. BARBIERI: Thank you for that, Fred.

DR. CROSSON: Fred would have made a good lawyer. I'm having problems escaping that, but I just wanted to get back to that question about your position earlier. You described these sort of two different Luiz's on either shoulder that are arguing, but was one of the concerns related to what John brought up? I'm just making sure I understand this.

The status determination of overfishing does flip an on/off switch, a blunt instrument, in the ABC Control Rule, and you're worried that it's going to, because of that, it's going to trip things up, when it would be better if there were a more nuanced approach, if the overfishing is only slightly above the level that might cause us concerns. Is that what one of the Luiz's is arguing?

No, it's not that, necessarily. It's really the amount, and if the amount is not something that we need to worry about, really -- If the result, the specific numbers, that are coming out of this assessment report are going to be used to guide the reductions in F, that, to me, is a different story than us saying, well, there is a possibility that we are undergoing overfishing, and we have a rebuilding plan in place here that could actually be kicked in and proceed in going forward and reviewing the stock, I'm fine with that.

DR. CROSSON: But you're worried about the ABC recommendation that we have to make, if we do determine that the stock is currently undergoing overfishing?

DR. BARBIERI: No, I mean the actual ABC recommendation that we will have to make is prescriptive, according to the Act and to the Guidelines. It's either zero or yield at F rebuild, with a 50 percent probability of overfishing, and so I mean none of this is equivocal or a matter of choice, even for us.

MR. CARMICHAEL: This stock is under a rebuilding plan. It was put in place awhile ago, before this assessment. The way the control rule is, the ABC is based on the rebuilding plan. That's what everyone has agreed to. The council set the parameters of the rebuilding plan. It's to rebuild to 50 percent probability by 2040-whatever, and so, as you do assessments along the path of that rebuilding plan, you're not ever obligated to change those parameters, and so the ABC control rule and the fact that status affects the P*, in this case, is interesting, but not really relevant, because you have the F rebuilding and you have the endpoint and you have the probability, and so you're under no obligation to change any of those. You can always recommend that the council adopt something different, but we haven't so far in most of our stocks.

DR. SERCHUK: I don't disagree, but let's take the cognizance of reality. Most models fail after they're projecting four years, let alone twenty years, and the idea of whatever recruitment assumptions you put in are generally not going to be realized over that long term. Moreover, the models that we have generally work well when fishing mortality is high and we have significant reductions.

When you're dealing with an F rebuild that is of the same magnitude, or possibly even lower, than natural mortality, let's be realistic about it. The driver is no longer fishing mortality by itself. The impact of natural mortality has a huge impact on the subsequent development in the stock structure, and we assume a constant M across a constant number of years. We know that that assumption -- We talked about uncertainties before. Let's talk about the uncertainties in the projections when that happens, and so I want that to -- We need to think about that, because we have a very long projection period, even for an F rebuild, and none of us, few of us, may be working at the time this stock is rebuilt, according to that. Hopefully many of us that are not retired are happily retired at that time.

The experience that we have had in the Northeast is going out ten years is stretching it. When you're going out to 2045 or whatever it is, your assumptions determine what's happening in terms of the evolution of the stock and not the dynamics of the stock. It's whatever assumptions you've made about recruitment and whatever assumptions you've made about natural mortality. I know we have to do it, but let's be pragmatic about thinking about what assumptions are driving the development of those projections.

MR. CARMICHAEL: Luiz, as I recall last time, the SSC typically has -- There was a lot of discussion of the long-term rebuilding the last time, and the SSC at one point didn't want to provide those long-terms, and they were obligated to, as you know, for the reasons under the law and establishing the rebuilding plans, but I think the SSC ended up giving the council ABC recommendations for a shorter period of time, and I would fully expect that to probably be the case this time as well, and that's been pretty common around the table, of here's the rebuilding plan and we have to do that, but we're only giving you ABCs out three or four or five years, because, beyond that, we really don't have any confidence in those estimates. I think that matches very well with what you're saying there, for sure.

DR. SIEGFRIED: I just have a question, as far as the projections would go. Is the concern, and Fred brought this up, but is the concern that the Fs coming out of the assessment model are wrong, or is the reference point that the Fs are compared to? We just used F30 because that was what was there before. That's certainly not our recommendation, nor do we have the right to recommend that, and so all that would depend on what you chose for the reference point.

DR. BARBIERI: John, I will put you on the spot there.

MR. CARMICHAEL: Katie is exactly right. It's a council decision informed by an SSC recommendation, basically, to what the reference point is, and there was extensive discussion when we were at F30. I think there was a debate ranging between F20 percent and F40 percent. Looking at what you know about the stock now and the stock recruitment relationship, the fact that you fixed steepness as opposed to estimating the last time around, if you were to choose an Fmax or an F of 0.1 or some other alternative approach, that's well within your right.

DR. BARBIERI: Okay. Well, back to I guess our action items. I mean I think we had quite a bit of discussion about this. I don't mean to dominate the discussion, but I just felt that, as a member of this committee, I had to put forth my personal and professional opinion about how I felt about it. Identify and discuss assessment uncertainties.

DR. REICHERT: So where did that leave us in the previous discussion, because some members said that they felt that the assessment provided an adequate basis. You, on the other hand, felt that, in particular, the exploitation rate was not well determined, and so where does this leave us?

DR. BARBIERI: My understanding, from what John and Fred and Steve and my discussion, is that basically we can actually agree that we -- It's possible that we have an overfishing determination, but that will not prevent us from projecting forward yield at F rebuild and providing recommendations on that basis, and so, explicitly, that was my concern, is whether we will have to have a direct metric of reduction of F to satisfy the guidelines and the law. If that's not the case, then I am comfortable with yield at F rebuild, as proposed by the rebuilding plan, and I think we can, in that case, determine stock status and support fishing level recommendations.

MR. CARMICHAEL: It sounds like the group agrees with the assessment conclusion and the review panel recommendation that the stock remains overfished. You believe that there is a basis for supporting fishing level recommendations, using the projections from the assessment, and, with regard to overfishing --

DR. BARBIERI: I think that that status is more uncertain. I personally, and I don't mean to speak for the committee, and that's why I'm trying to hold back and actually wait for folks to --

DR. SERCHUK: As I understand your argument, and I'm trying to see whether we can use your argument, what is the F rebuild? Is it 0.15 or 0.12 or --

MR. CARMICHAEL: 0.14.

DR. SERCHUK: 0.14. So you're suggesting that we can say that overfishing is going on, but we might not believe it's 0.5. It could be 0.3 or it could be 0.2, according to you. In other words, you think that this recent F is too high and it's uncertain. I'm not trying to put words in your mouth, but I'm trying to get a sense of your argument. In any case, what you're saying is we believe overfishing is going on and we're going to reduce F to the F rebuild, and I think that's the way out of it.

MR. CARMICHAEL: I think that's the key, that the SSC believes that overfishing is occurring and that F needs to be reduced to F rebuild.

DR. BARBIERI: To F rebuild. I was trying to determine what the requirements were, and both what John Carmichael brought up and you brought up in this discussion I think has clarified that we are in a situation that we can agree that we can't quantify the degree of exploitation or overexploitation at this point, but we need to reduce F to F rebuild and continue forward.

DR. SERCHUK: Fine. That would be consistent with the panel. That would be consistent with the law, and I think that would be consistent as a way of going forward, and so I support that.

DR. REICHERT: That does answer the question that I just had, because I think it's very important that we very carefully draft that language into our report, because I think that's going to be very important for the council.

DR. BARBIERI: That's why, Fred, when you apologize and apologize and apologize for actually making comments, I'm saying that's what we are looking for to really start fleshing this out. I mean this discussion does bring clarity to the issue, and it allows us to move forward and come out of some of these situations, and so I appreciate you engaging and providing that discussion point.

DR. SIEGFRIED: The council, by way of John, asked me to have explanations available for one of the things that you brought up in your previous presentation about MSY, the pounds and all that, and so, when you all are ready to talk about that side of status or fishing level recommendation, I have that for you, because it seems like, because that was so different, you would need that.

DR. BARBIERI: Excellent.

MR. CARMICHAEL: I was going to say if you go down to the table that shows where you give status, one of the net outcomes will be that where there's the overfishing, and there is usually a number put there, you will simply put "unknown". You're used to putting a number in there, where you have your overfished and overfishing evaluation, and you won't have a numerical overfishing number.

DR. SERCHUK: It would be greater than 0.15.

DR. BARBIERI: You see, Fred, you hit it spot-on. I mean this was one of my concerns, is how we handle the numerical value there which could have some phenomenal implications going forward, and I think, right now, we have built a rationale there that is in accordance with the law, in accordance with the guidelines, and satisfies our requirements here for providing stock status and fishing level recommendations.

Then, regarding the additional bullets, identify and discuss assessment uncertainties, I don't think we need to go one-by-one. Most of those, as Fred pointed out, are very explicitly discussed in the review report, and so trusting that all of you read the report and are familiar with those, anything else that you would feel would be relevant to be added there --

DR. SERCHUK: I'm just wondering, on the last one, is it looks like the last one suggests possibly us giving some impetus to those areas where we think the assessment could be improved in the future, and one thing that I'm -- Again, maybe I'm speaking out of turn here, but I am convinced

that we are not going as good a job as we are able in sampling the older components of the population.

I'm thinking that if it takes a longline survey or if it takes some other way of doing it, this would be a good time, in my mind, to emphasize that, because this whole idea of how we determine whether the population is rebuilding, we should see it in the older age groups, but if we're not sampling the older age groups in the appropriate fashion, then I think we should make a point right here to talk about that. That might give some impetus, politically, to try to move away from the status quo and to improve our sampling systems of the population. Thank you.

DR. BARBIERI: Again, and I know I am repeating myself, but because I see Chip taking notes there, but, please, flesh that out when you get that draft report later this week, probably Sunday afternoon, and start fleshing that out, to add to the report.

DR. BELCHER: To add on to that, as Katie said, the states did help with a big swing in getting aged things, and we did that with our own funds. That was basically freeing up personnel to get out there to intercept and get that information. As budgets are getting tighter and tighter, if these things end up bigger and bigger, we're not going to be able to do it, and so the infrastructure needs to be noted at the state level as well.

DR. BUCKEL: To Fred's point, there's been some of that work done, Fred, you know the Mitchell et al. paper we were talking about before that did a longline survey cooperation between scientists and fishermen, and Katie presented that, the comparison between the age and size structure from the chevron trap to that longline data.

Maybe a more -- Instead of all the effort that goes into a monitoring program, maybe just a smarter -- This is a question to you. If we really want to know if the chevron trap is getting an adequate, if it has a logistic selectivity, then having an experiment to empirically estimate selectivity of the chevron trap, since we have the long time series that the video and the chevron trap was designed for -- Part of this recent design was for red snapper, and so to empirically test that, and so a smarter, less labor-intensive good selectivity study, either through tagging or some other visual observation of what's down there, and then see what gets in the trap.

MR. CARMICHAEL: I would certainly hope at that point there would be some comments about how your inability to get a reliable estimate of terminal fishing mortality certainly adds to the risk and uncertainties and impact status determinations. The whole last half-hour discussion, I think, was a good example of that.

That's a big uncertainty here, and Fred's comments about Fs being below M and the difficulties that creates is certainly something that should come in here to justify the need for perhaps more rigorous monitoring of this population and the uncertainty with the discards and what an important portion of total removals they are, maybe some more dedicated sampling to get information on those. It's an age-based model, and most of your removals are discards, and what sort of ages do you get from them? It's a big question there.

DR. SERCHUK: I think those are all good points. The value added from the SSC is that can we prioritize that? We have a laundry list now, but I think, if I were a council member or if I were ready to put in a proposal someplace as an independent researcher, if there was a priority list that

the SSC came out and said we think this is the biggest bang for the buck, or the two biggest bangs for the buck, I think that would be helpful. I don't know whether we can do it, but I would certainly suggest, if we could do it, that we would have a higher likelihood of having those types of things possibly funded in the future.

DR. BARBIERI: Thank you. I think we are okay, unless I hear otherwise from you, regarding assessment uncertainties and the fact that we're going to -- Jeff.

DR. BUCKEL: I'm not sure that the assessment folks can tell me if this affects the future yield predictions. There has been this -- I hear total removals, the largest impact of total removals, is discards, but then, when you actually look at the F, the high F is the harvest, because it's the four-plus fish are what's been removed in those mini-seasons. It's not that large, relative to discards, but, relative to how many of those are out there, it ends up being a big portion of the F.

Moving forward, for the projections, does the SSC -- This gets into management, but maybe you don't want that fishery, if there is a mini-season, allowing it to be on those older fish. I know it's great to get the data on those, but that's what's been leading to these high Fs, and that would influence the yield as you move forward, what the selectivity of that general recreational fleet is.

DR. SIEGFRIED: Are you saying something like if there was a max size limit imposed? Then what we would do would be to change the -- We have to just put in a different selectivity that would illustrate that potential regulation, and so it would be something where the council would say what if we did this and we would change selectivity, based on that suggestion.

DR. BUCKEL: I know you guys can handle it. I guess my question is more to the SSC as a whole. Is that something that should be discussed, to give other potential management scenarios to the council, because of how F is being influenced when you have that mini-season and everyone is just keeping those older fish?

DR. BARBIERI: That's a good point. Maybe the unintended consequence -- I mean the idea was actually to decrease discards, if I remember correctly, and so people will catch their first fish and go home and not be high-grading, and I'm not sure that that really was realized, and so I think it's an interesting point to include as part of this discussion, in terms of management advice.

Okay, folks. We have another bullet. We have the one on advice on monitoring, and we've collected a lot of good points on that already. Research recommendations, we are collecting already some good points on that, and so I think the only thing left is to provide fishing level recommendations. Is that right, Mike?

DR. ERRIGO: Yes, that's what you have left, but we could apply the ABC Control Rule and get a P* value, or a P rebuild, I suppose.

DR. REICHERT: I don't remember, and it may have been in the research recommendations, but I think it would be extremely useful to get length comps for the video index, and that may address some of the issues that Fred brought up, because then, given the uncertainty in the relationship between length and age, at least we have a little more information relative to selectivity and some of the other parameters. I think that's something else that's important.

DR. BARBIERI: Thank you. Very good.

DR. CADRIN: Based on the projections that have been provided, it appears as though ABC will be based on F rebuild, rather than P*. Is that correct?

DR. BARBIERI: The NS 1 actually establishes that we can actually have yield at F rebuild with a 50 percent probability of rebuilding or we can have some other higher probability of rebuilding than 50 percent, if we think that's desirable. Is that correct?

DR. ERRIGO: Yes, you can recommend a higher probability of rebuilding. I believe, though, that that's a recommendation to the council, and they can either accept it or whatnot, but yes, you can recommend a higher probability of rebuilding.

DR. CADRIN: But that goes in the other direction. If you look at Slide 136 of Katie's presentation, F 30 percent does not provide for rebuilding, and, really, unless we picked a very small P*, we wouldn't get down to an F rebuild, and that's at 50 percent. If we wanted a greater probability of rebuilding, it would be even lower. I think, in the decision tree, ABC should be based on the rebuilding plan.

DR. BARBIERI: Yes, and I agree completely, and not just that rationale that you just added for it, but that, I think, also agrees with the point that Fred made about the value of the current fishing mortality that's unknown, but we know it's above a certain value, and reducing it to F rebuild just brings us into compliance there. Any additional points or disagreement or agreement of support?

DR. SERCHUK: Just another thought. Because we had the discussion about we're only going to probably set ABCs for the near term, I think we need another benchmark, because the F rebuild is based on this long rebuilding period. I am concerned about what milestone we'll be using to come back in three or four years to see whether whatever F we've used will get us to that waypoint. I am not concerned where we're going to be in thirty years, because, again, that's all assumptions by that time, in terms of the dynamics.

I think we need to have something in addition, to be consistent with the fact that we're only going to probably going to go to ABCs for the next several years, and say, okay, by 2020, we think that the stock size should be here, with a high probability. It's not a probability of going for twenty years out, but it's a probability of being there in four years, to a certain rebuilding thing.

One might argue, in terms of statistics, how much of a difference do we need for rebuilding to be detected? That's what you would do if you were doing a statistical thing. How large of a difference do I need to see that we've moved from here to here, given the uncertainty in the assessment, and I think that's a real critical issue here, because we have uncertainties right now with the assessment, but we want to have a high assurance that we're going in the direction we believe, and I don't think using a rebuilding F necessarily, based on twenty years or thirty years, is the same as saying where will we be in four years, because we'll be setting the ABCs based on the four years. Do you get my point, Mr. Chair?

DR. BARBIERI: Absolutely. Yes, Fred, and thank you for that. You can see there is a bullet there, the last bullet in our list of action items, that is explicit about that. Provide guidance on the next assessment, addressing its timing and type.

DR. CADRIN: In that term of reference you just mentioned, I think that we should reference the estimate of a recent strong year class, but that's largely from discard length frequencies, and so an update assessment in the next few years would help us to confirm whether that is a large year class or not.

DR. REICHERT: We have some preliminary data that Joey provided that we have seen that in 2015.

MR. CARMICHAEL: An update in 2019? It would be a request, obviously. It's not binding, but would you think that would be appropriate?

DR. REICHERT: I would say certainly not after 2019, because then the terminal -- I am more interested in the terminal year data, and so this is 2014. That would be three additional years of data. Unless additional management actions are taken, I would say three extra years of data would probably be sufficient for an update, if there are -- If management actions or size differences or something like that, then those three years may not be sufficient to see that reflected in an update, and so that's maybe a little bit of a caveat.

DR. BOREMAN: I would change "would" to "should" in that sentence though.

MR. CARMICHAEL: When the table of fishing levels is filled out from the projections, how far will you go? That's sort of the other question, because we discussed that you have projections out for a long period of time, but how far are you comfortable in providing ABCs to the council? Assuming that the ABC is going to be based on the F rebuild and the table will be populated there with the F build projection, how many years are you comfortable in filling that table out?

DR. BUCKEL: I was just assuming it would be until this update or operational assessment, whatever we're going to call it.

DR. BARBIERI: That's how we've done it in the past, Jeff, and I agree. If we make a recommendation to have an assessment, a next assessment, we go up to that one.

MR. CARMICHAEL: You make a request for a projection. That's kind of out of your hands, as to whether or not -- I mean a request for an update is kind of out of your hands as to whether or not it happens when you ask for it, but what will typically happen is if you end your ABC recommendations in 2019, then if the update has not been completed by that time, then whatever you recommended in 2019 stands for 2020 and so on, until the update gets done.

You, because you give the ABCs, have a little more leeway over that, and it gives you the opportunity that perhaps updated projections could be asked for at some point in the future, to perhaps extend that window, if necessary, because we don't know what squeaky wheels will pop up between now and 2019.

DR. REICHERT: I know this is details, but, initially, we said three extra years of data. That means an assessment in 2018 with a terminal year of 2017. That's three years. If we want to go with four years -- 2014 is the terminal year, and so if we want to go with 2019 with terminal year 2018, we've got four years of data. I just want to make sure we all understand what we just decided, because it's different than what we decided just a little while ago. I know this is all

recommendations, but I just want to make sure we're all clear in terms of how many years we are potentially adding.

DR. SCHUELLER: Say we're consistent here, and we want to add four years of data to look for this potential age class and we go through 2019, Fred threw out the possibility of putting together another sort of population benchmark. I know how this type of thing would work within ASMFC, but I don't know how that type of thing would work here. If we decided that we thought that was a good idea, would we formulate that within this group now and then recommend that to the council and they can choose whether or not to adopt it, or do we say you should consider alternatives and are you going to do that, and then they can task us with it? How does that work here?

MR. CARMICHAEL: That's a good question, because it's kind of a new place for us. I would think, if you saw something before you in the information that you thought this would be an appropriate metric, you could recommend it now. If you like the concept, but you're not sure of what the value should be or the metric should be, then what we've typically done is some sort of subcommittee that might look at that and come back with some recommendations for the SSC in October, and we could tell the council that you're considering that.

I think you express the concerns about this long-term rebuilding and not knowing, but you would like to see some measurable signs of progress toward improvement by the time of this next assessment, and these are some things that you would consider as evidence that we're heading in the right direction. Maybe it is some level of spawning stock, some numbers of fish, some increasing numbers at age, perhaps, if there's a potentially good year class and you have fished it at F rebuild, you know how many ten-plus fish might you have.

I think there's a lot of things, and, unless someone has a good idea now, maybe a subcommittee is the best approach, but that's probably kind of how ASMFC might approach it, too. They have a little more flexibility, and they've dealt more with these kind of trigger concepts than we have.

DR. BARBIERI: Amy, I also think that even if this doesn't get to anything definite, definitive, in terms of the way forward, I think it would be very interesting, as an exploratory exercise, to really inform discussion here and provide some perspective to the council.

DR. SCHUELLER: Yes, and I mean it would be interesting. I guess my thought was, and I don't know if Fred implied it this way, but usually, when there is some sort of trigger like this, it has some teeth with it, and so some thought would have to be given to what teeth would be incurred with meeting or not meeting that potential benchmark, and I don't know if the council would adopt that or not.

DR. REICHERT: I think that would be consistent with the rumble-strip approach that we have discussed off and on.

DR. ERRIGO: Which the council has endorsed. They are onboard with some kind of a rumble-strip.

DR. BARBIERI: Okay, and so I think we are at a point we've completed Agenda Item Number 11, unless there are any additional comments or questions or suggestions from the committee. I think we have most of the issues discussed there and have good notes. We have the fishing level

recommendations, we have the timeline, time horizon, for those fishing level recommendations. I think we can complete this item. Any additional comments?

MR. CARMICHAEL: Back to what Katie had brought up earlier about the MSY estimates, and you noted in your slide the change from the last assessment to this one. We might want to see some of that, because I expect that will be a big point of discussion with the council.

DR. BARBIERI: Yes, excellent point. I forgot about that, Katie, but that's very good. May I suggest then that we take a ten-minute break and that we reconvene at twenty minutes after three to have that presentation and discussion.

DR. BARBIERI: Katie prepared some additional materials, as requested by the council staff.

MR. CARMICHAEL: In response to the point that you made in your presentation about the changes in MSY over time.

DR. SIEGFRIED: Just when you thought I was done. John Carmichael contacted me about some -- He had a premonition that the council would have questions about this, and I agree, and so, actually, it might be helpful for me to bring up Luiz's presentation. When we were going over the concerns about the assessment, this was brought up.

In particular, I looked at SEDAR 24 results versus SEDAR 41 results, and one of the points John brought up, and then Luiz brought up today, is this difference between MSY in the previous assessment and the MSY proxy in the current assessment, 1.8 million pounds versus a three-quarters-of-a-million pounds. That's a big difference, I agree, and so let's look at some of what caused that.

The other metrics, like FMSY, or F30 for us, are very similar. The discard mortality at MSY, or F30, they are relatively similar, and so the biggest difference was in biomass or in MSY or MSY proxy, and so I have a few slides here for you. The first thing is the difference in recreational landings. This is in numbers. Blue is SEDAR 24 and red is SEDAR 41.

The data workshop went around and around and around about the differences here. We have a different value estimated for the historical period, and MRIP versus MRFSS estimates for the current 1981 on period, and so there's a huge difference in the amount of historical take that was used for 24 versus 41, and so that's one thing. You can imagine the model has to assume, as John Boreman said earlier, that there's enough out there to be able to catch that many in the first place, and so that's one basis of difference between the two.

For reproduction, SEDAR 41 and SEDAR 24 -- SEDAR 24 had SSB modeled as gonad weight of mature female. It has a later age at 50 percent maturity and a constant number of batches at age. SEDAR 41 has SSB modeled as population fecundity, which has been regarded in the literature, in past assessments, as being desirable, rather than some sort of proxy like gonad weight, or even just the weight of mature females. It has an earlier, just slightly, age at 50 percent maturity, and we have an age-specific number of batches and that uncertainty around that age-specific number of batches.

We have differences in natural mortality and age and growth. On the left, you have SEDAR 41 is in blue and SEDAR 24 is in red. There is a bigger difference at age one, but it's an overall lower value for the last assessment than for SEDAR 41. The length at age are actually pretty similar, and so the green is the current assessment. The red was SEDAR 24. This is for length at age, and then this is weight, in kilos, at age, and they're pretty similar. SEDAR 41 is purple and SEDAR 24 is blue.

A very large difference is in the selectivity, and when John brought this up, he said is it selectivity, growth, natural mortality, the catches, or maybe all of them, and it seems to be everything except for growth. SEDAR 24, we have this bottom panel is what I want you to focus on. This is the total average selectivity. We see the mode being age threes, with very low selectivity at the older age classes.

For SEDAR 24, the bottom is the analogous panel. The modal selectivity age is five, full selectivity, and there is a lot more selectivity of the older age classes for average weighted selectivity, and so then I did a few analyses, just messing around in R, to see if I could get this to happen, and so I used the RMSY values. It's 584 for SEDAR 24 and 448 for SEDAR 41. I used those as the initial values. I killed them off with natural mortality that's different between the two, I looked at the differences in fishing mortality estimated in the population, and applied the selectivity, and that's where you get the difference in MSY.

I can put together more for the council, John, if they end up wanting all of those analyses step by step, but I think, for this group, this really shows -- This total weight of selectivity is very different, particularly for the oldest age classes, and the modal age of full selection. The natural mortality is significantly larger, which in sensitivities for SEDAR 24, showed a smaller MSY. Then the reproduction is modeled with an earlier age of 50 percent maturity and with these age-specific number of batches, and then this huge difference in historical landings doesn't make the model assume that there's a very large initial population. That's what I have.

DR. BARBIERI: Thank you for that, Katie.

MR. HARTIG: I would echo that, and yes, if you want to put together something more detailed that comes out of the SSC, we would much appreciate that as well. There's got to be some way I can explain this, somehow, some way.

MR. CARMICHAEL: Katie, I think just something showing the recruits at MSY from the two stocks and what the effects of just natural mortality are. What does the natural mortality get you out of 500,000 fish, given the SEDAR 24 versus the SEDAR 41 natural mortality levels? I think that does show a good sense of what's happening to the fish. You're feeding roughly the same fish into the system, a little bit fewer in 41, which obviously that recreational catch probably has a big influence on that, but the higher natural mortality is pulling them out a lot faster, it seems.

DR. BARBIERI: Any additional questions or comments for Katie? Katie, thanks again for putting this together. I think it's really, really interesting, and it's going to be very helpful for discussions with the council. John Carmichael, I think we're going to have the discussion of red snapper projections tomorrow sometime?

MR. CARMICHAEL: Red snapper projections?

DR. BARBIERI: No?

MR. CARMICHAEL: I thought we were done with red snapper now.

DR. BARBIERI: Yes, but I'm talking about Agenda Item 18.

DR. ERRIGO: That's a discussion on management and stuff like that. It's not about the assessment.

DR. BARBIERI: Then so apologies to the committee that we are sort of jumping all over, but, given the length of discussions that we've been having and the relevance of some of these issues, we are having to adjust our agenda to make the most efficient use of time, and so, talking here to Mike E., we thought that the best way to move forward for this afternoon would be to have the golden tilefish update assessment.

We have Dr. Genny Nesslage here, and we don't know how long she will be available tomorrow, and so it would be great, Genny, if we could move into that. Any concerns from the committee? If not, let's proceed with the golden tilefish update assessment, and that's Agenda Item 12. You know already about Attachment 15. That's the golden tilefish update assessment report, and we have a number of action items.

They're very much in line with the ones that we just went through for SEDAR 41, red snapper, but, in this case, perhaps a bit more relevant, since update assessments do not get reviewed by SEDAR review panels, and they are reviewed exclusively by the SSCs, and so we're going to have to perhaps be a little more explicit and detailed in addressing those action items, all the detailed bullets, and then completing our SSC fishing level recommendations table. I am not going to reread all the terms of reference or the action items that I've read before, but they are very much in line with the ones that we just saw, and so, Genny, if you could proceed with the presentation.

DR. NESSLAGE: Thank you, Mr. Chairman. As Luiz already mentioned, this was an update, and so my presentation is going to be significantly shorter than the red snapper presentation. I'm sure you're all relieved, and not to make light of the hard work that Kate did.

What I will begin by talking about will be reviewing the data that were updated for this assessment. Most of the data did not change much. Some of the data -- I will try to highlight all of the changes that had to be made or were made in both the data and the model update configurations, and so, because a lot of the assessment assumption and decisions were not revisited from the benchmark, I will highlight those and point them out, but I will try to focus more on changes that were made and what the ultimate implications of those necessary changes were on the assessment, but do please stop me as I go along if you have any questions.

I will begin with the data, before moving on to model configuration and results. The life history information used in the update was exactly the same as that in the benchmark. For those of you who aren't as familiar with golden tilefish, it's another long-lived species. We model ages one to twenty-five, with twenty-five being the plus class, and the natural mortality and fecundity and maturity are all age-specific vectors. I have plotted here the M at age that's assumed in the assessment. Also, I have provided the maturity at age graph. It's important to note that this fish

as well appears to be, even though it's a long-lived species, to mature at a relatively young age, and so, by age four, most of the fish are mature.

The other life history point that I just wanted to highlight was the length at age. This growth curve is constant and assumed in the assessment. The CV is estimated, but this fish does have sexually dimorphic growth, which we do not have the data to essentially account for in the assessment, but it's something to keep in mind when you're considering uncertainties in future benchmarks if additional data, sex-specific data, were to be collected.

Moving on to landings, this stock was very lightly exploited until the early 1980s. There was a peak in landings in the mid-1980s and then a decline. The terminal year of the last assessment was 2010, and so those were the last landings that the SSC would have reviewed. Since that time, the landings have increased, and, just to give you an idea, there are three fleets, a recreational fleet, commercial longline, commercial hand line fleets.

This graph, I couldn't show you all the years of the commercial data broken down by fleets, but I just wanted to give you an idea of the relative magnitudes of the landings for the different fleets, and so it's pretty obvious, from the graph, that the longline fleet makes up the majority of the landings, with the commercial hand line coming in second, and if you can even see the little white bars at the end here, those are the recreational landings. Again, there are landings in these last few years, but the bars had to be removed.

I should mention as well that there are very minimal discards in this fishery. They were considered during the benchmark, but there was a decision not to include them in the benchmark assessment, because they're so small in magnitude. I took a look at them again this time, and I think it's a good practice for everyone to take a look at what the magnitude of the discards are with each assessment, but they were on par with the previous benchmark, and so I didn't see any need to include them in this update.

The one change that I did make with regards to the recreational landings, or harvest, was that the previous assessment, SEDAR 25, had received the MRFSS estimates -- For 2005, it had this really high point estimate, and that was deemed, during the data workshop, to probably be anomalous, and so they took the average of the surrounding years, and that that brought that value down. When I received the MRIP estimates, the point here in blue with the arrow, that estimate seemed to be much more in line with the surrounding years, and it looked quite reasonable. I ran it by the Beaufort pop dy crowd as well, and everyone felt that we should probably keep the original data and not replace that value with an average, and so that was one change I did make. Again, the recreational fishery is a tiny component of the overall landings, but it's important just to note that that change was made.

Abundance indices, there are two used in the assessment. One is the longline CPUE index, and that's the one that you will see in blue here. Since the last assessment, the CPUE went up for two years and then declined, and the model does pick up on that, which I will point out later in the presentation.

The other index is a fishery-independent index, the MARMAP longline survey. The values for this, because the data are pretty sparse with regards to catches of golden tilefish in general, the data during the benchmark had been aggregated across five-year periods, and so each one of those

points is a four or five-year aggregate, and so that's why it looks a little strange and there is no line connecting them, but I did add the one more year. The terminal year from the last benchmark was, of course, 2010. The survey ended in 2011, and so that one extra year was added on to that point, and it brought it up a tiny bit, but, really, it didn't affect the index that much.

With regards to the sampling intensity, the Table 7.5 summarizes the total number of trips or sets with regards to the MARMAP survey that were sampled for golden tilefish. You can see it's a relatively poorly-sampled fishery, especially in the early years. You can see also that there is a transition from largely using length compositions to age compositions as we move along in the time series, and there tends to be more samples towards the end of the time series, although I should point out that this 2012 number of forty-eight samples for the longline fishery was a bit anomalous. We probably won't see that again. That was partly due to an extra research project was going on, but it's nice to have that extra data.

A couple of changes that were made the data that I should point out -- I already mentioned the 2005 MRIP estimate was not adjusted. The other thing that's important for you to know is that when the data were queried from the headboat survey to provide the recreational samples for this assessment, it was discovered that the benchmark data query had included commercial samples and research samples, which should not have been included in the dataset provided, and so the new dataset that was provided to me was reduced by about a third, with regards to the number of samples. However, I think, and I will show you this in a little bit, that the actual data are far more representative of the recreational fishery than they were before, and that did have some implications for how I chose to model the recreational fleet selectivity.

Then the other point I wanted to make, or just point out, is if you happen to be comparing the benchmark with the update, there was a copy-and-paste error with regards to the MARMAP age comps. The MARMAP data are fine. I see Joey getting panicked in the back. The MARMAP data provided were fine. It was just a copy-and-paste error to the TPL file, and so they were shifted two ages to the right, but that has been identified and corrected. It didn't have much of any effect on the assessment, but the age comps from MARMAP are fit better now. I think the model can make better sense of the data that were provided now.

MR. HARTIG: Can you go back to the sampling? I needed to find out what units we're in, when you talked about the forty-eight samples versus the --

DR. NESSLAGE: That's the number of trips for the commercial longline and the commercial hand line, as well as the recreational, but it -- The recreational is a combination of trips and -- It's angler trips and -- It's a strange combination, and I'm trying to remember.

DR. ERRIGO: For headboat, I think it's angler hours. Then, for MRIP, I think it's angler trips.

DR. NESSLAGE: The MARMAP, of course, is sets.

DR. REICHERT: This is also the five-year aggregates, and so 2010 is actually 2010 through 2014.

DR. NESSLAGE: Exactly. These are the numbers that were provided to the model to setting the effect of sample sizes for the multinomial likelihoods. Are there any other questions? I was going to move on to model configuration, but there are any data questions?

DR. SERCHUK: On the abundance indices, again, they're plotted as point estimates, but do you have CVs around them? The reason I ask is I am always troubled when I see particularly CPUE indices, because many -- They can be a proxy for abundance, but they can be a proxy for lots of other things as well.

Particularly for a long-lived species like this, when I see significant jumps of a couple years, and in some cases they can actually increase by double, that could be a recruitment event, or it could be they moved to a different area than they've been before, and I have no idea how variable these things are. If you don't have them, that's -- I was just wondering how variable they are, that's all.

DR. NESSLAGE: I know that there are CVs for the MARMAP index. I did not plot those, but I can dig those up for you, if you're interested, but you're interested in the CPUE index. I am trying to think back -- There must have been, but I did not plot them, and I can't tell you, off the top of my head, but I can look it up at the break and get back to you.

DR. SERCHUK: You understand why I'm asking.

DR. NESSLAGE: I understand completely, yes.

DR. GRIMES: I wanted to ask you, and I should have asked this before, but a few years ago, there was a report or study rattling around that these things were protogynous hermaphrodites. Did that get sunk or --

DR. NESSLAGE: My understanding is there is still not consensus on that, unless someone has any --

DR. REICHERT: It's my understanding that, from our data, that we have not seen that in any of the samples, but I need to verify that with Dave Wyanski, who is our reproductive --

DR. GRIMES: I never believed it.

DR. REICHERT: We have not seen that.

DR. CADRIN: I agree. In the Mid-Atlantic stock, there were some histological studies that also raised questions with that. That was the McBride et al. paper that came out two years ago. They also questioned that, and I so agree that it's not definitive.

DR. BARBIERI: Any additional questions for Genny? If not, thank you, Genny, and go ahead.

DR. NESSLAGE: Moving on then to the model configuration and changes that I made since the last benchmark, the model, as I mentioned before, tracks ages one to twenty-five, with twenty-five being a plus-class, and the span is 1962 to 2014. We assume that the F in the initial year was very low and that the population was very lightly exploited, according to the available commercial and recreational landings data going back to the 1960s. Not recreational landings, but the commercial landings going back to the 1960s, and we also assume equilibrium abundance at age in that first year.

The growth and maturity curves are specified and constant across the entire time period. We also estimate constant logistic selectivities for all three fleets and the MARMAP index, and the time-varying catchability was explored during the benchmark. The final base run from the benchmark assumed it was constant across the time series, and so I did the same with the update.

The stock-recruitment curve is Beverton-Holt, with an assumed steepness of 0.84, and annual deviation is estimated from 1976 to 2007. The folks doing the benchmark had done a lot of explorations of when the annual deviations should be turned on and off, and the best start year was determined to be 1976, and then it was also considered that, given the animal doesn't really recruit to the fishery until it's about age seven, at least the longline fishery, they thought that dropping off the last seven years of the time series would be appropriate, and so I also did the same thing with the update as was done in the benchmark with regards to estimating those annual deviations.

DR. BARBIERI: Genny, we have a question there from Steve Cadrin.

DR. CADRIN: Thanks, Genny, and I respect that this was the decision on R deviation that was developed at the last benchmark, but this seems to contradict your earlier report showing that the sampling has improved, particularly in the commercial hand line and longline, and I would think when you have composition data and the sampling has improved, that would be a time when you estimate recruitment deviations, and I'm not suggesting that anything be done different for this update, but that might be a decision that's revisited in the context of recent sampling intensities, particularly because the model is not particularly fitting the commercial hand line age comps well in 2011 to 2013, and maybe we could address that if this were loosened up in subsequent updates.

DR. NESSLAGE: Excellent suggestion.

DR. BARBIERI: Yes, excellent suggestion, Steve, and, by the way, this could be added to our report. We have some action items there that ask for that type of information, and so very good.

DR. NESSLAGE: The model estimated 188 parameters. The length at age curve parameters were specified, but the CV was estimated. We estimated R0, the standard deviation in recruitment, and, of course, the annual deviation, as we were just discussing. We also estimate the slope and the age at 50 percent from those logistic selectivities for each of the fleets and the MARMAP survey, and we also estimated the CPUE and MARMAP catchabilities and then, of course, the average F and the annual deviations for each fleet. That adds up, hopefully, if I did my math right, to a total of 188 parameters.

The benchmark had placed priors on the growth CV, the slopes for all the selectivity curves, and the recruitment standard deviation. I did the same in this update, and I also reproduced the iterative reweighting procedures that were done in the benchmark, such that the likelihood components were reweighted, as you've seen with all the other BAM-based assessments, until the SDNRs were all equal to one. However, in the benchmark, there were concerns that that final fit, once they were all equal to one, still did not adequately track the commercial CPUE index, and so it was then upweighted to three. I did the same in this update, and so that is following the same procedures.

The uncertainty was assessed using the same MCB procedures that you've heard Katie talk about all day. I conducted 5,000 MCB trials. The observed landings, age and length compositions, and the indices were bootstrapped. Steepness was drawn from a truncated beta distribution, M was

drawn from a truncated normal distribution, and that upweighting of the CPUE index was also rejiggered as well, but it was drawn from a uniform distribution, plus or minus 25 percent of that value of three, and so those are following the same procedures as the benchmark as well.

There were some changes that needed to be made or that I decided to make with the code and the model configuration, and so I will talk about those next. The code had one mistake in it from the benchmark. The recreational landings were being fit numbers to pounds, which isn't appropriate, and the only reason I really -- It doesn't really have an effect on the assessment, ultimately, as far as stock status. However, if you're comparing or looking back at the old benchmark report and doing comparisons, you really just need to not look at the recreational estimates from the old benchmark. They're not appropriate.

The CPUE was changed to fit whole pounds to match the population in whole pounds, which only affects the catchability, which is a scalar. Therefore, it has no effect at all on the assessment, except for that one value.

The other change that came along with the adoption of the latest version of the BAM code was the use of a robust multinomial likelihood, which I assessed in the Sensitivity Number 1, which I will talk about in a little bit. The other choice that I made was to eliminate the longline length samples from those three years listed, 1996, 1998, and 1999. I chose to do that after re-reading the data workshop report.

There was a big, long section on how they were concerned that the samplers were grabbing only large fish, or largely large fish, out of proportion to the number of fish, out of proportion to what would be expected, and so the -- They didn't actually end up removing those data points from the benchmark, but I got the feeling that that would be the most appropriate thing to do. If you think your data are highly biased, you probably shouldn't throw them in there, and so I did take them out, but I also wanted to make sure that this wasn't having a major effect on the assessment, or at least assessing what the effect would be on the assessment, and so you can see that. We will talk about that in Sensitivity 3.

I also ended up estimating the recreational fleet selectivity, which is a change from the benchmark. During the workshop, folks were concerned that the number of samples were really low for the recreational fishery and that perhaps the estimation of that selectivity curve would not be terribly robust, and so they had set the recreational selectivity equal to the hand line fishery selectivity, given their similar gears, a very reasonable assumption at the time.

However, when the new headboat survey data query was given to me and I looked at the data, you can see here for yourself that I had -- My initial run, these are the pooled compositions, length compositions, for the commercial hand line on the left and the recreational on the right, and you can see that if you force the recreational fishery to have the same selectivity as the hand line fishery that it no longer looks like an appropriate fit, really. It looks like the recreational curve should be shifted to the left quite a bit, and so I tried to estimated recreational selectivity and had no problems estimating those parameters, and so I decided to keep that, given I thought that the old method would provide inappropriate treatment of the data that was provided, and so hopefully you all agree that that was an appropriate change.

MR. HARTIG: The 1996, 1998, and 1999 years you removed, based on what was thought to be biased sampling to the largest sizes, I mean I'm not a modeler, and I am reticent to remove any data, but can those be downweighted in some way, instead of totally removing them from the model? Is that a possibility?

DR. NESSLAGE: I'm not sure how exactly would downweight them.

MR. HARTIG: I don't either. That's why I'm just --

DR. NESSLAGE: If you had an alternate study, I suppose, that knew exactly or had a different, less biased idea of what the length structure should be, perhaps you could, but I don't think we have that information. I can't think of any reasonable way to do that, which is why I just eliminated them. Ultimately, I think you'll see that it didn't have an effect. Luckily, the removal of those data didn't make things change much, but I know I didn't like -- I hate removing data too, but, unfortunately, it felt like keeping them in didn't -- It felt worse, I suppose you could say. Are there any other questions on configuration? I was going to move on to results. Okay.

I will start with fits to the landings data, starting with the commercial hand line. You can see here that the last three years of the hand line landings were quite a bit larger, and more on par with the mid-1990s than they were with the 2000s. The commercial longline landings fits -- Again, all the fits are going to be quite good, given the configuration of the model, but you can see here as well that the longline landings have gone up since about 2004. Let me know if I'm going too fast.

Recreational harvest, these are the plots that are going to look different from the benchmark, and then, of course, there is the additional years of data. It looks like recreational harvest is quite variable, but it may have gone down a little bit in the last few years.

Moving on to length and age compositions, starting here, these are the same sort of plots that you saw from the red snapper assessment, and so I won't go into great detail, but, essentially, starting with the length compositions for the commercial hand line in 1984, the compositions are quite sparse. They jump years. The fits aren't terribly great, but then again, the compositions aren't terribly informative in some years. Note the sample sizes, with regard to the number of trips, is really low in many of these years, especially ones where there is quite a bit of variability.

The length comps for the commercial longline start up in the upper right-hand corner here in 1998. These, in particular, gave me great heartburn, and they were poor fits during the benchmark. It was noted and talked about during the assessment workshop and the review. I knew Erik went to great lengths to try and ameliorate this problem, and I did too. I spent quite a bit of time trying to come up with the answer, and, the more I think about it, the more I think some of Steve's work, the McBride paper, seems to indicate with -- During periods of high exploitation, males, in particular, at least of the northern stock for tilefish, show a change in maturity and growth, and that may be what was going on, because these fits, if you see these, and then I will move on to this last year, 1994, but these commercial length comps from 1988 to 2014, at least the tail-end of the period, where they were being highly exploited, and so it's quite probably that there is time-varying maturity and time-varying growth in this stock, but we can't really go back in time, as was mentioned earlier, but that may be something to explore more carefully during a benchmark.

Even though we can't necessarily tease apart the sex-specific aspect of that, we may be able to pick up on any trends that may have occurred and, therefore, better fit this data, but that was beyond the scope of the update, I'm afraid.

Moving on to the length comps for the recreational fishery, you can see there that they're highly variable and uninformative in many years, but the model tried to fit them as best it could. The age comps are quite a bit better, starting with the hand line fishery in 1997 through 2014. I will say that, with the iterative reweighting procedure and the robust likelihood, there is less emphasis placed on trying to fit these older ages, these points out here, and more emphasis on trying to hit the peak, which tends to be a slightly younger age class.

These are the fits to the commercial longline index, and I will say, even though these aren't perfect, they are quite similar to the fits achieved during the benchmark, and the last set from the longline fishery and the MARMAP survey. Unless there is any questions on the comps, I will move on to the fits to the abundance indices, starting with the commercial longline CPUE.

This is fit relatively well, but keep in mind that this has been upweighted quite substantially. If you assume equal weights in this model, the model can't fit this downturn at the end of the time series and it really tries to predict a much higher value at the beginning of the time series, and so that upweighting really affects these fits.

The MARMAP index has never been -- We've never been able to fit, either in the benchmark or this update, these first two points. However, it is able to fit these last three points relatively well. Are there any questions about those fits or comps? I was going to move on to population estimates.

Recruitment, these are the estimates of recruitment on the left, and then the log recruitment deviations on the right. There is a bit of a trend, as you can see, a little bit of under and then over later in the time period, but I have to say that this is a huge improvement from the benchmark, which, if I may move on to the next slide, if, for those of you who recall SEDAR 25, there was a bit of a discussion about this anomalous high prediction from 2001 for recruitment. I mean it's off the charts, this value, and it was a bit of a red flag. That no longer exists in the update. With a few more years of data, there was no evidence that that was a huge year class, and so that point just came right back down, and so you can see that we're all much more in line with reasonable values with the update.

MR. HARTIG: How are you estimating recruitment? Are you back-tracking recruitment in this particular exercise through cohort analysis and things of that nature? Okay. So how many years behind are we on recruitment, based on this assessment? I mean how far into the future can we predict recruitment, how many years? I thought it was seven or something like that.

DR. NESSLAGE: Sure, that's when they would start to show up in the fishery, at least the longline fishery, which is the most well-sampled.

MR. HARTIG: The reason I ask that is because I fish in this fishery and I have looked for recruitment when we started back in the fishery, I mean actually physically looking in areas where recruits would be, and then I think it was 2013 when we saw a huge recruitment spike in the fishery, and it was so big that even the market grades changed. It's usually small, medium, and large, and there were so many fish two-pounds and under that they had to make another market grade. That

was some of the comments I think we had made, to try and ask you to use market grades, possibly to inform recruitment, but I saw that the market grades are so variable in the different areas that you couldn't use it.

Where the fishery is mostly prosecuted, and I think almost 80 percent of the landings come from Florida, I think those market grades are pretty stable for those areas, and I think you could get some information out of that somewhere down the line, but just to know that we saw a huge recruitment spike come into the fishery in 2013, and Peter has documented it, to some degree, in a paper that he has put together, and so there is some information available.

DR. NESSLAGE: Perhaps it's not my purview to suggest this, but perhaps the SSC could suggest, for the next benchmark, to explore or try to identify that 2013 year class in the next assessment.

DR. BARBIERI: It's a very good recommendation, yes, and I think you can suggest it and the committee will discuss it and it will kind of add to our report there. That's a very good recommendation.

DR. NESSLAGE: Just in response to your comment, the market grade data are highly variable, as you mentioned. When I requested the data, they were thinking of were we going to use it for the whole assessment, the whole stock. I think if the council were to make a very specific data request that you might be able to get that information to support what you were just describing.

DR. BARBIERI: Thank you for that, Genny.

DR. SHAROV: This is just to show you that we're not asleep here. Ben, I have a question for you, just to make sure that I follow you. When you say that you saw the recruitment in 2013, what age or size group are you referring to? These are obviously the young-of-the-year fish.

MR. HARTIG: No, they're not, and I'm not sure what a two-pound tilefish, what age that would be, because they were right around the two-pounds, and there was a lot of them. The longline fishery caught a lot of them. We didn't catch as many, because we could move. I mean I fish the bandit fishery, and so, when I interact with those little animals, I move away from them, but, unfortunately, you set 500,000 hooks in an area that has little teeny fish, you're going to catch a lot of them, and they did show up, like I say, in at least the market grade category, and so it is something that we can look at later.

DR. SHAROV: Genny, is that something that is reflected in the catch information that you've been looking at?

DR. NESSLAGE: There were definitely higher landings in all sectors in 2013. That much is true. Whether that -- I would have to look at what a two-pound -- I would have to do that conversion in my head, which I can't do right now, but I'm going to look at it, because now I'm curious. Maybe I will get back to you all after I've had a chance to do that. Any other questions or comments?

Moving on from recruitment to abundance at age, these are the similar plots you've seen with red snapper. It's color-coded by age across years. The one really good, I think, good piece of news coming out of this assessment is that -- I hope you have this on your screen, because you can't see it so well here, but obviously we don't see any pink fish, the ages twenty and up, in the assessment

at the moment. However, the last ten years or so, we're seeing more of the blue, and so those are our teenagers, and they're showing up, again, in the assessment, and that's a good sign. An expansion of the age structure is always something that we like to see, and so I was pleased that that seemed to be evident in our estimates of abundance at age.

Biomass at age also, you can see, again, obviously the precipitous decline here. However, there has been a bit of a rebuild, and then we're starting to see an expansion out to these older ages in the mid to late teens, and so, again, a very good sign.

Estimates of spawning stock biomass, keep in mind, and I should have mentioned this at the beginning and I apologize, but these are measured in female gonad weight, metric tons, and the spawning stock biomass, of course, has declined as well in proportion to biomass, but then it increased again in the mid to late 2000s, and we are above the MSST threshold, but below SSB MSY. The 75, I threw that on here just to make sure everyone knew that I'm using the new version of MSST, the 75 percent of SSB MSY and not the old one minus M times SSB MSY used in the benchmark, and so if you're comparing the two reports, that's one thing you want to keep in mind.

Moving on to fisheries, the selectivity estimates are plotted on top of each other here, to give you an idea of what ages are being targeted, as estimated by the assessment. This is the new change that you will see from the update, that the recreational data seemed to indicate that the recreational fleet is targeting fish that are a good deal younger than the commercial hand line or longline fisheries or even the MARMAP survey is picking up, and so, again, the recreational fleet is tiny. It doesn't have a huge contribution to the overall landings, but this is a bit of a big change.

The resulting fishing mortality estimates, over time, you can see, again, that the commercial longline fleet makes up the majority of the F, with the hand line falling in second. You can kind of see the recreational Fs blipping in here and there, but they make up a very tiny proportion, but also note that, since the last benchmark in 2010, we've seen an increase in F. The resulting status plots of F over FMSY indicate that the last three years have been overfishing, which is a change from the stock status determination in the last assessment.

The resulting estimated landings are shown in this plot, with landings in weight, gutted pounds, of course, on the left and numbers on the right, with the corresponding MSY values. You can see we're just a tad bit above MSY at the end of the time series, and that's largely where the overfishing status is, of course, coming from. If I am going too fast, yell, but I know it's getting towards the end of the day, and so I will keep moving if I don't hear anything.

Moving on to some of the alternate runs that I did, I presented the base run. I also ran a couple of alternative runs that you should be aware of. The first is, and I know this takes a little bit of explanation, but the SEDAR 25 revised is the old benchmark code with that correction made for the fits to the recreational data, and so it's what the old benchmark should have been, and the other thing that I did was that I made sure that I was estimating the new biomass, or overfished threshold, as opposed to using the old measurement, and so now everything is on the same page. We're comparing apples with apples, which I know Laura likes to do.

That run is the dotted line. The continuity run, what I'm calling the continuity run, is the updated -- It's the same code, but with all of the new data updated through 2014, and so this is what the old model would have produced with those additional four years of data. It's slightly different than if

you didn't have those four years of data, and so they did inform the model, but it's quite similar. Then, of course, the updated base is the solid, dotted line, and so you can see that the continuity and the old SEDAR run have a different stock status prediction than the update, and that, of course, I was curious about that, and so I did a series of sensitivity runs, the first being investigating all of the changes that I had made that I thought were significant enough to have potentially affected the assessment.

I will start from the bottom, again, up. The dotted and dashed line here, you could barely even tell it apart from the updated base. That's the effect of if I were to have included all of the commercial longline samples. Remember I said that I had removed those three years of length composition? If I had included them, it doesn't really have much of an effect at all on the assessment, and so hopefully, even though, again, no one likes to throw away data, but at least that didn't affect the assessment too much.

When I went back and forced the recreational selectivity to equal the hand line selectivity, and that's shown in the dotted line here, there's a slightly lower estimate of F, barely, during this peak period, in the 1990s, but, really, you can't tell much of a difference. The big difference appears to be the adoption of the new BAM code, which included the robust likelihood, and so when I went back and ran the base run, but using the old non-robust likelihood, we get the similar stock status that was predicted by the continuity and the old revised benchmark runs, and so that's really where the difference lies.

This is a -- I don't want to call it a data-poor fishery, but the sampling is pretty low, and so the model, when the non-robust likelihood is used, is very likely chasing after some rare observations of larger fish. When it does that, it's going to increase the estimate of biomass and lower the estimate of F.

We've seen this behavior. I believe the first time the robust likelihood was adopted for BAM was the gag update assessment. It's the same results, a lower SSB with the adoption of the robust likelihood and higher Fs, and so this has been used for the last two SEDARs, I believe, including red snapper and gray triggerfish and the gag update, and so this has also been the recommendation of SEDAR working groups, and I see Steve has a comment.

DR. CADRIN: Just a question, Genny. If the SEDAR 25 data had been fit with the robust likelihood, do you think it would have had that spike in 2001? Is that an example of chasing --

DR. NESSLAGE: The recruitment spike?

DR. CADRIN: Yes.

DR. NESSLAGE: Absolutely, yes. That's a good example of the poor performance of non-robust likelihood.

DR. CADRIN: I think that's a good example of why this isn't just a technical, impressive improvement. It actually improves the performance of the model quite a bit, because that was a huge source of uncertainty that we may have resolved and so thank you.

DR. NESSLAGE: Thank you.

DR. SHAROV: Genny, how is the FMSY estimated here? I was specifically thinking of you have three fleets with different selectivities and different contributions, and so what FMSY represents here is that the full F or --

DR. NESSLAGE: I believe it's the full F.

DR. SHAROV: For the combined selectivities for all three fleets?

DR. NESSLAGE: Yes.

DR. SHAROV: Okay. Thank you.

DR. BARBIERI: Any other questions for Genny? Go ahead, Genny.

DR. NESSLAGE: I also performed retrospective analyses, peeling back to 2006. There wasn't a lot of pattern, obvious pattern. There was a little bit. They're hard to interpret, with regards to golden tilefish, because, when you start peeling back, you start losing whole datasets, and then that really makes the retrospectives hard to interpret, but, in general, the terminal year estimates for F over FMSY and SSB over MSST didn't vary too much with these peels.

You will see the estimates for recruitment were much more variable, but, again, this is a combination of both losing a whole dataset with the loss of several years, once you get back to 2010, but also there is that seven-year assumption about not estimating the annual deviations at the end of each time series, and that also affects where these terminal year, last few year, estimates of recruitment come from, and so they're a bit more variable than you might expect, but not too bad, considering all those changes that occurred with dropping the last few years of data.

Moving on to uncertainty, I will start going through the results from the MCBs. If you recall, the dashed lines represent the medians of the MCBs and the solid lines represent the base run estimate. They were quite similar for most of our big measures, FMSY, MSY, SSB MSY. They were slightly different -- The median was a little bit higher for the MCB runs, but not too much, and BMSY.

Terminal year stock status, uncertainty was -- On the top, I have plotted SSB in 2014 over MSST, and then SSB over SSB MSY, and then the F status of, and I should have mentioned if I didn't, is the geometric average of the last three years, 2012 to 2014, over FMSY. The one thing you will notice is the long tail on the SSB and the F measures, and I think it will be more obvious in a moment, when I show you the next graphs, that the model is indicating that we may be slightly worse off than the base run indicates that we are, and I think that's -- I will show you the trends first.

These are the trends in SSB over MSST. The median and the base run both agree that we're above our overfished threshold, which is good. However, the median of the MCB runs and the base run both agree that there is overfishing in the last few years, and there is a skewed -- It's skewed to being a bit above FMSY.

The phase plot that everyone likes to investigate, here we've got the crosshairs zoomed in on 2014. It's important to note that we have quite a few runs in this quadrant where we are not overfished,

but overfishing. However, there is quite a few runs, in fact much more than there was in the benchmark assessment, indicating that we're both overfished and overfishing, and that's something that I'm sure the SSC would want to take into consideration when considering uncertainty, and so the base run may be optimistic, according to the MCB trials.

Lastly, projections. I ran stochastic projections using the MCBs for ten years past the terminal year. Management is assumed to begin in 2017, and I ran the scenarios that were requested in the TORs of F current, FMSY, 75 percent of FMSY, the current P* of 0.35 for golden tilefish, but then I also ran 0.5, as requested.

I will start with F current, and there is really nothing exciting going on in the projections for F current, and so I won't spend too much time, other than to orient you to these slides. Spawning stock biomass is in the upper left and biomass is in the upper right. Abundance is in the lower left and recruits is in the lower right. Each of these has two panels, because the TORs requested that I also provide F, landings, and the probability that we're above our overfished threshold. Again, F current is not terribly exciting.

If you move to FMSY, you can see that the spawning stock is predicted to go up a little bit, biomass as well, not too much in abundance or recruits, and the F goes down a little bit, which you would expect. The probability of SSB being greater than threshold does go up. The 75 percent of FMSY projections, of course, you are reducing F a little bit more, and so you're going to see more of a dramatic increase in spawning stock biomass. You're starting to see a little bit in abundance, maybe a tad in recruits. The drop in F is more substantial and landings as well, and, of course, that fits back into your probability of the SSB rising above your threshold.

Then the P* projections, the P* 0.35 is very similar to the 75 percent of FMSY runs, and the P* at 0.5 is very similar to the FMSY runs, and so you will see they look -- It's a little bit of déjà vu, but the P* 0.35 is probably the most optimistic, I think, if I remember correctly, with regards to reducing overfishing. Then here is P* 0.5. If I'm rushing, I apologize, but that was what I had to present to you, but I am happy to answer any questions you might have and catch up with some of you on those questions you had that I have to go back and research, but if you have any additional questions you think of after I leave today, please feel free to email me.

DR. BARBIERI: Wonderful. Thank you so much, Genny. Any additional questions for Genny? I think this was very thorough. Thank you, Genny. If we can switch back there to our list of action items, we have the same set of bullets, starting with the review assessment, and I would like to refresh your memory to the fact that this is really the only review of this update, and so we really need to get the content under these bullets to be well fleshed out as we start constructing our report. In terms of the reviewing the assessment, does the assessment address the terms of reference to the SSC's satisfaction, having had a better description of that level of satisfaction?

DR. REICHERT: I would say it does.

DR. CADRIN: I agree, and I would be more explicit that the revision to the robust likelihood is an improvement that we accept.

DR. BARBIERI: Very good, and, of course, we're going to have the opportunity, as we go through our draft report, to go in and, at your own leisure, add some additional thoughts to help build our

report there. Does the assessment represent the best scientific information available? Would anybody be opposed to that? This represents BSIA. Does the assessment provide an adequate basis for determining stock status and supporting fishing level recommendations? I am seeing a lot of people nodding. It is the end of the day, and folks are getting tired, and so this is why I'm saying that we're going to have the opportunity, to Mike E's satisfaction as well as Chip, that we will flesh out these bullets for our report. It just may not happen all right now.

MR. COLLIER: It's always better to have it on the record prior to putting it in the document, just in case there is something that comes up in between.

DR. BARBIERI: Very good point, yes. With that, let me open up the floor for -- Does the assessment provide an adequate basis for determining stock status and supporting fishing level recommendations?

DR. SCHUELLER: I will go on the record. Yes, it does provide an adequate basis for determining stock status and supporting fishing level recommendations, and I would also like to just go ahead and say I agree with Steve's comment that moving to the robust multinomial likelihood is a good move, and I think all of the decisions that were made in the assessment are well documented and justified.

DR. BARBIERI: Thank you for that, Amy. Identify and discuss assessment uncertainties, and, of course, we have been discussing those all along and asking Genny questions and clarifications during the presentation, but if there is anything further that you want to bring up here -- Are key uncertainties identified? If not, indicate additional uncertainties that you feel were not properly accounted for in this assessment update. Alexei. I am just picking random people and kind of putting you on the spot. Sorry. Have the key uncertainties been identified, in your valuable opinion here?

DR. SHAROV: Yes.

DR. BARBIERI: Let the record show that we have concurrence here with Alexei that the key uncertainties have been identified and we cannot identify, at this point, any additional uncertainties that we will recommend be included in this update. Are risks and consequences of uncertainties identified and evaluated? I think that Genny's presentation was very good at pointing out some of those things and bringing all those issues that she evaluated and explored further. Any disagreement that those risks and consequences of uncertainty have been identified and properly evaluated?

DR. SHAROV: I mean we're looking at the assessment update and this is not a benchmark, right, and so, to the extent that we had to review the information, there is no doubt in my mind that the quality was at least maintained, but certainly improved, based on the additional information that was provided. I understand that it is a rhetorical question, where we have to answer it, and so the answer is yes.

DR. BARBIERI: Thank you, Alexei. I think you answered it, but you provided some additional text there. Are methods of addressing uncertainty consistent with SSC expectations? I think this is a real clear yes, and both Katie and Genny described the MCB process, which we are familiar with and very comfortable with.

List and comment on the effects of those uncertainties that most contribute to risk and impact status determinations and future yield predictions, and I think that Genny threw us a softball when she actually talked about the phase plot with the uncertainty in stock status there and the number of different MCB runs that fall in those specific quadrants of the plot. Basically, we have been presented with those uncertainties, and we have them very explicitly outlined there in our report and presentation.

Now, we are ready to provide fishing level recommendations, and that, of course, entails applying our ABC Control Rule and complete the fishing level recommendation table. Genny was kind enough to present us some projections that already incorporate a level of P* that had been set by the SSC for the benchmark assessment that we saw a few years back, and so it's just a matter of us reviewing now application of our ABC control rule and see if we have anything else that we would like to consider regarding the dimensions and tiers of our ABC control rule.

I am going to go then to Tier 1, assessment information. This was a quantitative assessment that provides an estimate of exploitation and biomass, and although it includes MSY-derived benchmarks, I believe that you had fixed value of steepness, and so that would be Tier 2 in that first dimension. The uncertainty characterization, whether complete, high, medium, low, or none.

DR. CADRIN: I believe MCB is designed to achieve a Tier 1.

DR. BARBIERI: No, I don't think so, in terms of the environmental conditions.

DR. CADRIN: It's 2. You're right. Sorry.

DR. BARBIERI: It's Tier 2 for the second dimension. For stock status.

DR. REICHERT: This is something we may -- I know we're going to discuss it when we discuss the ABC Control Rule. I think, increasingly, because we are managing around FMSY, we'll have these stocks that may be on the cusp of being overfished or not overfishing, and so we'll -- I think, at some point, we as an SSC will need to deal -- Because this is important in our P* value, but you're at that point where it's just overfished or not, and so I'm currently not sure how to handle that, unless we have a broader discussion about the ABC Control Rule, which we may have, but I just wanted to mention that this is one of those stocks where you are right around that level. If you look at the uncertainty, that points towards the fact that you are. There is a high level of uncertainty, but, nevertheless, it makes a big difference whether you are choosing one, two, three, four, or five, and so I just wanted to mention that and see if we may want to have a little bit of discussion about the impact this may have.

DR. BARBIERI: To that point, I would bring back the Slide 47 in Genny's presentation. You can see the point estimates there, but, at the same time, have an idea of the uncertainty around those. Right now, if we look at the point estimates, we have not overfished, but undergoing overfishing, which would put us in Tier 3, I believe, right? Yes, the stock is either overfished or overfishing.

DR. SCHUELLER: I mean this gets at the whole why is the long tail occurring, and I just -- Do you have any thoughts about maybe if -- I don't know if you've investigated, but maybe what piece of uncertainty is leading to that long tail?

DR. NESSLAGE: I think the model, if I were to try to think like the model, which is scary, I think what it's looking at is it appears to be looking at that CPUE index, which I should look at the CVs, and seeing that there had been such a dramatic increase and then a sudden decline and a sudden increase in the landings at the end of the time series, quite substantial, and really only a slight expansion of -- I mean a good expansion of the age structure, but not much. Not to the extent that it might compensate for the increase in landings and the decline in the CPUE that it saw.

I think maybe that's not answering your question, but, balancing all those things together, I am guessing that it's not seeing as rosy a picture as the last assessment, especially given that it's using that robust likelihood and not putting as much weight on some of those rear observations, as well as Steve pointed out that huge spike in recruitment in that 2001 estimate. It would have, in theory, pushed more animals through, and those animals aren't there anymore, and so why it's skewed for F more so than SSB, I haven't wrapped my brain around that. I'm not sure I have a good answer for you, but I will keep thinking on it.

DR. REICHERT: Right now, I think our choice would be Number 3, based on the outcome of the stock assessment, but I think it's worth adding some language to the report to reflect the discussion we just had.

DR. BARBIERI: Okay. That sounds good. Then the fourth dimension is --

DR. REICHERT: I looked the PSE score up, and, for tilefish, it's high.

DR. BARBIERI: That will be Tier 3. I have not been adding that up, but I'm sure that Chip has.

DR. ERRIGO: The total adjustment is 20 percent, which would make the P* value 0.3, slightly different from SEDAR 25, I think because of the overfishing status.

DR. BARBIERI: Okay, and so we have our P* value, and this is a stock that is not overfished, and so, Mike E., I think that we need to wait for stochastic projections at that P* of 0.3 to be produced. Genny is saying that she can do it for tomorrow morning, just for the 0.3 value instead of 0.35.

DR. SERCHUK: After you finish this up, I want to return to something else, Mr. Chair.

DR. BARBIERI: I mean I finished. It was just like if we scroll down and look at that table, we're going to have to make recommendations of yield streams for OFL and ABC. Our yield stream for OFL is going to be at a 0.5 P*, and Genny has already produced those, but we're going to need a yield stream at 0.3. She had run them before at 0.35, because she was starting from what we had from the benchmark assessment. If she can produce the 0.3 P* yield streams for tomorrow morning, that will be wonderful. We can just go there tomorrow morning and populate our table.

DR. ERRIGO: You can finish up your discussion without the values by, first of all, recommending a P*, which I think we've done, and saying that that will be the basis for the ABC and then whatever other action items you have left here. Perhaps the amount of years that the projections should cover. We did that for red snapper and the timing of the next update or assessment and things like that.

DR. BARBIERI: Absolutely, and Marcel had already brought that up earlier, about the number of years for the projection, the length of the projection period to provide fishing level recommendations for golden tilefish.

DR. REICHERT: I would say no more than five, especially given the fact that we are hovering around that threshold level.

MR. COLLIER: Before we get to the end, where do you want start, 2016 or 2017?

DR. BARBIERI: I think we would have to start in 2017.

DR. REICHERT: Ben, can you remind us? Is it the calendar year fishing year for golden tilefish?

MR. HARTIG: Yes.

DR. REICHERT: So that would be 2017.

DR. BARBIERI: Okay, and so any other points?

DR. BOREMAN: A couple of points. First, five years may be too long, because it is on the borderline, and where did it come from? It was below the threshold last time, and so it's moving in a bad direction.

MR. COLLIER: We were not overfishing the last time. However, as Genny pointed out, the change to the robust multinomial may have changed what SEDAR 25 came up with.

DR. BOREMAN: Okay, and so we may or may not be seeing a trend here. Three years may be better, I think, until we get the next update. That's my first point. The second point is we do golden tilefish in the Mid-Atlantic, too. We don't have to ask permission or use your assessment or anything. We do it all ourselves. Anyway, I'm just kidding.

We have, in the Mid, we rely exclusively on the landings per unit effort index, or catch per unit effort in the commercial fishery, and we're quite uncomfortable with that, because it's a dome-shaped selectivity, that fishery -- I don't know if they do it down here, but, up there, they don't like landing the big ones, and so it's an obvious dome-shaped selectivity, and the people in the fishery admit to it, and so it's something down here about -- I don't know how this group feels about the comfort level with the indices of abundance that are being used here. It's commercial longline, and so that's a commercial index, and then there is the MARMAP, which I don't think is really -- Is it really geared towards golden tilefish? It's what the comfort level of this group is. If you want to push -- In the Mid, what we're pushing for is a special survey for blueline and golden and all our other deepwater species that are more or less cryptic and aren't vulnerable to our bottom trawl. It would be nice if we could join forces with the South Atlantic folks and maybe have a bigger voice somewhere along the line. Thanks.

DR. BARBIERI: Very good point, and we do have some bullet points there that will request those types of recommendations. Right now, what I'm hearing is a suggestion for a three-year time horizon instead of the five, and I'm seeing a lot of people nodding around the table in agreement, and so, Chip, we are going with three.

DR. REICHERT: Just as an FYI, there is a current effort underway, in collaboration with the commercial sector, to extend the golden tilefish bottom longline survey. It's currently a one-year effort, and so that may provide some additional samples for the next assessment. Hopefully we can extend it, because one of the comments on the MARMAP longline survey has been that it's geographically relatively limited, and so we are currently working on extending that, and so that may be something that may be available. I would still say that that should be a research recommendation, to see if we can strengthen the fishery-independent survey, in collaboration with the commercial sector or others.

DR. SERCHUK: I wanted to make a comment when we were discussing the status of the stocks relative to the status determination criteria. There was a concern raised that we were sort of right at the limit or right over the limit, based on that uncertainty diagram, but I'm a little bit troubled that we're using different metrics on the different axes. One, we're using a point estimate at SSB and one we're using a three-year average on F. That's one issue.

I think, if you really looked at the -- If you made it consistent, you either would use a three-year average on both of them or basically you would say, well, we're going to put a lot of weight into the point estimate on -- A lot more weight on the F, because it's a three-year average. In fact, if you look at the fishing mortality on a three-year average, it actually brings it down a little bit, because we have an increasing trend.

I don't think we're quite nearly near the line as we would otherwise think, and so I wonder whether if your intervention before about being near the limit or above the limit, we're actually -- I think we're probably -- We are overfishing, in my mind, because we're using a three-year average. The fact is that, because we've changed -- Even if you went back, it's simply because if you're using this three-year average and you're below it -- One point above it is still going to be below it. Two points may still be above it, but now we have three points that are above it, and the last one is considerably above the line.

Presumably, you went to a three-year average because you wanted to buffer. Going from one state to another state, you wanted to make sure that you were really in it, and so I don't think you're -- In that logic, I don't see it that we're right near the limit or above the limit. We're quite a ways now from it.

DR. BARBIERI: Right, and the last of the tail --

DR. SERCHUK: When you use an average and you have an increasing trend, you're going to buffer that a little bit, rather than using the last data point. Thank you.

DR. REICHERT: I'm glad you brought that up, because that's absolutely a valid point.

DR. DUVAL: Dr. Boreman must have been reading my mind, because one of the things I was going to ask Doctors Boreman, Cadrin, Serchuk, Sharov, those who are more familiar with the Mid-Atlantic, is how golden tilefish is assessed up that way and whether or not you had fishery-independent indices and if it was considered data-limited and what methods you're using.

Then, also, to Dr. Boreman's point about longer-term surveys, in the South Atlantic, we're sort of working on that. Actually, towards the end of 2014, thanks to the efforts of Todd Kellison, Nate

Bacheler, John Carmichael, Marcel, and myself, Todd Kellison had put in an internal CRP to have a cooperative deepwater survey design workshop with industry, and so that was funded and was held at the Beaufort Lab in April of last year.

It was a great workshop. We had industry representation from throughout the South Atlantic, and one of the topics of conversation there was that these species don't just stop at the North Carolina/Virginia border, and one of the recommendations was that there be coordination with the Mid-Atlantic and areas to the north, given the prevalence of these species outside of the South Atlantic Council's jurisdiction and being able to have some kind of survey that would allow for comparisons between jurisdictions.

That's now a NOAA Technical Memorandum, and it's been on my mental list to do to send that to Chairman Robbins. He and I have had conversations about this, and so definitely the joining forces is heard and something that we certainly would like to do, and I think there is strong evidence that we need it. Of course, there are a lot of needs as well, but the main reason I got up here was really just to ask a few questions about how golden tilefish is assessed in the Mid-Atlantic, the frequency and the indices that are used and that sort of thing. Thank you.

DR. BARBIERI: Thank you, Madam Chair. We got some direction there on items to basically expand as we construct our report. We have some requests of John Boreman and others who are more familiar with the Mid-Atlantic to help us flesh out that part of our report that articulates the need for that additional data collection survey development.

We are now at the point where we provide advice on monitoring the stock and to the next assessment, which I think we just heard some discussion on. Provide research recommendations and guidance on the next assessment, and I think, as I go back through the notes, I'm going to find quite a bit there already in terms of the research recommendations, but, in terms of guidance on the next assessment, addressing its timing and type, my assumption, since this is an update, and there were some issues that had been addressed during this update, is that the next one might be a benchmark, but I will open it up.

DR. REICHERT: I would also let that depend on the conversation with what's happening north of the South Atlantic region, something that John Boreman just brought up. Otherwise, I would personally be comfortable with another update, but this may be -- Maybe we should just wait and see what happens there. That may require either a benchmark or a research track, depending on where we are at that point.

DR. BARBIERI: Okay. Any additional points or comments regarding the next assessment? I don't know if it's already on the schedule.

DR. SCHUELLER: I will just say that I agree with Marcel on his last statement. I mean there's no reason to move it to a benchmark, unless there are new data available. If there aren't, then I don't see the point. Then, I think, putting up there research recommendations, one that Genny threw out too that I want to make sure gets up there is the sexually-dimorphic growth, but, again, that requires the --

DR. NESSLAGE: I don't know that we'll be able to do much about the sexually-dimorphic growth, but the time-varying growth is something I think we can definitely address.

DR. SCHUELLER: We can put both on there, but, again, I think it requires potentially more data to be collected to even address that. If that's not available, then, again, an update.

DR. REICHERT: To address a question that came up earlier, I emailed Dave Wyanski, asking him about hermaphroditism, and he emailed me back and said that there was currently no evidence of hermaphroditism, and there is some discussion in one of the SEDAR documents, but there was some evidence on the east coast of Florida that had a higher percentage of specimens with both male and female tissue, but, so far, there is no support for hermaphroditism, and so just as an aside, and I appreciate David emailing me back at the short notice.

DR. BARBIERI: Thank you, Marcel. Looking at that list there --

DR. ERRIGO: If you guys want to comment just really quick on the timing maybe for that next update or assessment. I know there was talk about exploring the 2013 recruitment event that was seen in the fishery. You would need some time for those to really grow into the population, where you could actually see it, and so I'm not sure when, but 2019 or 2020 or 2018, if you guys have a recommendation for how long you would wait.

DR. BARBIERI: I will call on our council liaison, Mr. Hartig. You had brought up earlier that you had a -- You had seen a lot of small --

MR. HARTIG: Yes, in 2013, we saw a large number of small animals in the catch.

DR. BARBIERI: Considering the length of time it takes for them to really recruit into the fishery, add six or seven years to that.

DR. BUCKEL: I think Ben said in 2013 they were two-pound fish, and so the question is what age were the fish in 2013, and maybe someone at the table knows, maybe Marcel.

DR. BARBIERI: Marcel is nodding that yes, he does know exactly the age.

DR. BUCKEL: Ben, do you know the length? We could look at the von Bert.

MR. HARTIG: Peter is here. Peter, have you got the lengths on those?

DR. PETER --: What was the question?

DR. BARBIERI: The question is at what age those two-pound golden tilefish are. There was a fairly large catch in 2013 of those two-pound, and we are trying to --

DR. ERRIGO: According to the life history characteristics table in the update report, a two-pound fish is somewhere between ages three and four.

DR. SERCHUK: Just a question, Mr. Chair. Yesterday, we talked about a stock assessment prioritization exercise, and I know that we haven't got there yet, but we're transitioning, and so I just want to keep it in the back of our minds, although we're making decisions now about when we might think we would next either update the assessment or have a benchmark, it is not

completely disconnected from the stock prioritization exercise that we're also undertaking, and is that correct?

DR. BARBIERI: That is correct, and, besides that, Fred, we do have a SEDAR Steering Committee composed of council representatives from three councils. It's chaired by the Science Center Director, and so what we do here is provide our input to help those decisions be informed. If there is something that we can provide from the science side that would help weigh factors, that we provide those in our report, but they are seen just as a recommendation, and not necessarily a definite plan for timing for those things.

DR. SERCHUK: My only point is we're going to try to do that exercise as well in the stock prioritization, sooner or later, and I know we have the flexibility to change our minds and we have the flexibility to get other input, but it's not disconnected completely when you take these assessments one-off and make a decision. Somehow, it will have to fit into the matrix, sooner or later, if we're going to take that path. Is that correct?

DR. BARBIERI: That is correct, and I will put Mike E. and/or John Carmichael on the spot, because these are action items that we are being asked to provide. We usually try to provide those recommendations to the council, as a way to inform their discussions and decisions going forward.

DR. ERRIGO: I would say that if you feel that an update or something should be done relatively soon that you may want to make that recommendation. If you feel it can go for a while longer, have more years in there, then we probably will be at the point of the stock prioritization exercise, but we're not quite there yet though.

DR. SERCHUK: My view echoes Amy's view, that it's an update, and so it won't really require all that much effort, and so I'm too concerned that this update that we have now -- I think it's pretty solid, and we can probably use that in the modeling effort in the future again, and so that's the way I look at it.

DR. BARBIERI: Thank you for that, Fred, because you see those are the types of comments and recommendations that go with our report, and the council now knows that this group of scientific advisors is weighing in on whether it should be one or the other and if it should be done sooner or later, and so that's exactly the type of discussion that brings those type of things.

Now, Mike E., I think we are at the end of this agenda item, and let me thank Genny again for the presentation and having addressed all these questions and for volunteering to provide the P* projections at the 0.3 level tomorrow morning. That completes Agenda Item 13. Looking at what we have left, we have about twenty minutes left on our time allotted for this afternoon, and we've had a long day.

I'm basically looking for some light items that are relatively fast to be covered, and what I thought was either Item Number 8, the South Atlantic For-Hire Reporting Amendment, if that is light and fast, or the Spiny Lobster Review. With that, I will say let's move on to Agenda Item Number 8, South Atlantic For-Hire Reporting Amendment. Again, I apologize for the back and forth. I can guarantee you that the next SSC meeting that you're going to have a much more orderly order of items and that the Chairman is going to be much more attentive to those issues. We will make sure of that.

Attachment 10 is the South Atlantic For-Hire Reporting Amendment document. Action items for the committee to consider are comment on what should be included in the core data elements. Emphasis should be given to data that can be used for both assessment and management advice, and so we are asked to review and provide comments on other actions, as necessary.

MR. CARMICHAEL: Thank you, Mr. Chairman. I look forward the new sheriff in town apparently keeping us on track better. I'm going to go through the for-hire reporting amendment, just briefly. A little bit of background. Here in the South Atlantic and in the Southeast, the for-hire vessels are permitted, but it's important to realize that there is a for-hire permit and it covers headboats and charter boats. It's one permit for both, and they're both required to report catch and effort, if selected.

In the past, charter boats have not been selected to provide some sort of additional reports, and so they're sampled by the MRIP program. Headboats, as we know, have been covered by the headboat survey since 1971, and not too long ago, there was an amendment where headboats are now reporting electronically, and so they report trip level, electronically, on a weekly basis. Both types of vessels, everyone under the for-hire permit, is required to participate in video monitoring, if selected.

What the council is working on now is an amendment that will go to electronic, mandatory, trip-level reporting to address the charter vessels, and there is three actions in this. The first is charter vessel reporting frequency and mechanism. It will be weekly. The reports will be due on a Tuesday, and the reporting week will be from Monday through Sunday, and so the intent there basically is to recognize that a lot of times these activities are recreational and they happen on the weekends, and so it gives the guys a reporting week that covers the whole weekend and then they have until the next Tuesday.

The headboats are going to a different timing of reports being due. In the past, they've been the following Sunday, and so they had a week, and this is going to be shortened to Tuesday to reduce recall and get them to get the reports done a little bit sooner, and there is also actions requiring locations in the electronic reports, requiring that they indicate the locations of the trip.

Right now, the headboat program and their electronic system, they report degrees and minutes, and so there's been a lot of changes. If you're familiar with the SEDAR efforts and the headboat survey over the years, it's been a lot of changes in how that survey handles the reporting of trip areas, and now they basically have a map. They can go on their electronic system, like a Google Earth chart or what have you, and they pick a spot and the degrees and minutes fill in. They can enter the degrees and minutes manually, or they can take it from the chart.

We also have the South Carolina DNR that's got a charter boat program that requires electronic reporting of their charter boats, and they report on a ten-nautical-mile grid, and so the headboat is one mile, the South Carolina is ten miles, and the South Carolina is similar to something that the headboat had a couple of years ago. One of the actions the council is looking at is just what's the detail of the catch reporting.

It's always is good to keep in mind we're talking about those two things. One is one report for the entire trip, and so it's not set-level reporting, and we know these vessels, particularly charter vessels, may cover a range of areas within a given day, and there's also, and it's something we had

to stress in the public hearings, is that the information that's reported to the agency can be at a much finer scale than the information, and is in a much finer scale than the information, that is provided back in a general query of the data.

If someone wanted to know something about catches, they would have to have confidential access to get more than state-level data out of the headboat reporting, because there is a lot of concerns of people who don't necessarily want to see all of their catch locations spread far and wide and made generally available to the general public.

Those are the three actions. This amendment was taken out to public hearing at the end of January and early February, and then it was reviewed by the council in March. The intent of the council here, overall, is that there will be reporting for all for-hire vessels. It will be a very similar reporting, consistent across both the charter and the headboats. Ultimately, the goal is one report for all for-hire vessels, and so you know we have folks that, as you get into North Carolina, which may be covered under a Northeast Region permit, Greater Atlantic Region permit, and a Southeast Region permit, and the idea would be that those guys should be able to file one trip report that covers the needs of both areas.

There's a number of HMS reports that these vessels have to file if they're fishing for tuna or billfish, and so there's a goal also to have those encompassed in there and be just one report, and we also have the states, and so South Carolina has their reports, and it would be nice if the South Carolina reports meet the needs of what the South Atlantic Council is doing for the region, and also that the landings would be available weekly for the ACL monitoring.

With MRIP, there is the two-month waves and then the forty-five-day delay, and so there can be some time lag in getting the information available, and so we would like to have that more timely. If the council had that, that may open up some options for them in terms of monitoring fisheries and perhaps responding to year-to-year changes in what fishermen are catching and better monitoring their ACLs.

One of the things that came up at the last council meeting was discussion of core variables, and there is some mention of this in the amendment. There's a lot of discussion of it in the public hearings, because the fishermen are obviously concerned about the level of detail they're expected to provide when they do these reports, and they would like to know what they're being expected to report, and the council is also interested in making sure they get some core information, and it's also going to be helpful for those goals of having one report to have an idea of what will this program require.

The council has asked for some feedback or some guidance on what the core variables will be, and their intent is to specify those in the amendment, most likely as a discussion and clarification item, rather than a specific action. They will review these in June, and then, at that point, they're going to consider final timing and when this may be approved for implementation.

DR. BARBIERI: I'm sorry for interrupting, John, but you said that this has gone out for public hearings?

MR. CARMICHAEL: Yes, it's gone out for public hearing, and, at that time, we had the list of data that are required by the headboat reporting and by the South Carolina DNR reporting, and

there is some differences in there. For example, the South Carolina program collects more information on things such as where the vessel left from, because a charter boat vessel is likely to move from different docks, as opposed to a headboat vessel, which is pretty well obligated to be in the one spot for extended periods of time.

There are some differences in discard reporting, which I will comment on, because the headboat used to collect information on the fate of discards, live versus dead. They moved into just collecting total discards, one of the reasons being the number of people that are on the boat, but also just the subjectivity in the fish being live or dead and then how the data are ultimately used. South Carolina is still collecting fate of the discards, separating dead discards versus live discards, which is similar to what's collected through the MRIP program through the survey.

We took these core variables, and I've had some feedback from the Center folks and as well as the state folks who work on the recreational data, to try to get a sense of what are the core. There is some discussion of maximum data and minimum data and what are you actually asking for, and the idea here is just to say these are the things that the council thinks are important and should be collected.

Of course, we need a number of identifiers, such as when the trip occurs, when did it leave and when did it come back, what's the date, what's the vessel, who is the captain, just to make links, perhaps, to biological sampling which could happen afterwards. Also information on effort, and I think some feedback here on effort would be helpful, and, in the headboat and certainly in the CPUEs, a lot of times the assessments are based on just catch per angler and not breaking down into hours, but, in a charter boat situation, it might be more informative or more useful to consider having actual hours fished. South Carolina collects information on hours fished and some more details, and so I think that's one of the first things, is what do folks think about hours fished? Is that useful?

There is also the question of target species. If you work with MRIP, you know people tend to identify the premier species, the common, popular species, as being the target, despite what they catch, and so there is some question as to how useful something like target species may be for CPUEs, and the same with gear. There is a desire to have information on gears, but a trip, say a charter boat trip, may troll during part of a trip and bottom fish during part of a trip. We're given trip-level information, and so there may be times when that is compromised.

Location is always debated, as I've said, and, right now, the question is the one-nautical-mile, such as having the core being the one-nautical-mile, like the headboat survey, or the larger, such as used by South Carolina, and getting the information on catch and the discard fate. Then something that came up in the hearings was the idea of split trip reporting. This was said, well, if I do a trip and I stop to bottom fish on a reef and then I go out and I troll for dolphins and tunas, could I report that information to you separately?

You would have one trip, but I might say during that trip that I had four hours of trolling at this location for dolphin, and this is what I caught and discarded, and then I had two hours at this location, generally fishing on reefs, and I bottom fished, and I caught these species and discarded those species. To the extent that we could do that, to me, that certainly makes a lot of sense in terms of our refining the appropriate effort for different types of gear types and what people might be discarding and the depths at which they're discarding.

Then the other thing is depth, and so, if you're in a chart-type reporting situation, you have some information, presumably, on the depth of the spot that they picked, but is it useful to have the maximum depth of the trip or maybe a primary? All of those, of course, again, you have multiple stops on a long trip, and what is the individual entering in terms of that data?

One suggestion was perhaps a range. Could you show a range over which most of the effort occurred, and perhaps that range could be tied to depths that are somehow related to barotrauma things, and so that's a general overview of what we're looking at, and any feedback you guys have, in terms of these core variables and what is useful, would be appreciated, and we would take this to the council.

DR. REICHERT: John, so the items with a question mark, are they the ones that are still being considered, or are they the ones that you are looking for some specific information on, for instance the depth, whether or not depth should be collected, or how depth information should be collected or both?

MR. CARMICHAEL: The ones with question marks are ones where, within these general categories, these were some of the questions that have come up, as to how we should deal with this. Depth, obviously, isn't an issue in terms of ACL monitoring, but it is an issue in terms of discard mortality, and so yes, your feedback on that is -- Just knowing what someone caught, just basically your catch and perhaps your discards, suffices for the council's needs for ACL monitoring, but, for CPUE, a little more detail could be helpful.

DR. REICHERT: To that point, I think, in particular, that depth and discard fate -- Because discard mortality is always a big issue in stock assessments, in terms of the uncertainties, and so the more information we have to estimate discards and discard mortality, the better it is, and so I would strongly recommend, if at all possible, to get as much information on both discard fate and the depth, because a lot of the discard mortality is depth related.

DR. SCHUELLER: I'm not going to comment on any specific thing here, but will make more of a general statement, and that is if you want to optimize the usefulness of these types of data, I would make sure that you're really working with the scientists who are actually going to use the data for analyses, and making sure that we're specifying what should be included appropriately, but I guess I also -- My thought about it is I caution against putting this directly into an amendment, because then what if things change over time and it's -- It's not going to be a quick change. It's going to take a lot of work, and so, to me, I think, as data needs would arise in the future, then this is going to potentially lead to a tumbling of things that would need to be done to make a change that could be relatively simple, if it was approached in a different way.

MR. CARMICHAEL: There was discussion of whether this should be an alternative or an action, and the recommendation that will go to the council will be that this should be part of the discussion and not as an alternative and not something to be codified, because then it allows the council to state its intent and to give guidance to the various programs that might be impacted without having to go through say an amendment step if the scientists should decide that, wow, you really need to change this or add this. Yes, we've heard that loud and clear, and that's come through, and we'll make that recommendation to the council. This will be in the discussion as a recommendation, I guess the council's intent type of language, as opposed to Action 2, specify all these.

DR. DUVAL: Just to that point, I mean we have had discussions about this. The existing regulations do allow for the Science Center to specify data elements that are required, and just the push and the feedback that we're getting from the public is they are asking what kinds of things are you going to ask us to report, and so we have to include -- We have included, in previous drafts, just here are the elements that the headboat fleet is currently being asked to report.

I agree that that's not something that we want codified in the text at all, and it's really just a fine balance of what are required data elements. We want to make sure that anything we're asking the public to report is useful, and we want to make sure that the way we're asking them to report is easy enough so that they can -- It's easy for them to do, it's quick for them to do, they can actually go back and access their own information, so that they find it useful, and if there are additional data elements down the road that you all feel would be useful or the Science Center feels would be useful, that can come up later in discussion. It might be that only a subset of that population might need to be asked to report that, or we could survey folks and ask if folks would be willing to report this particular element, if we find that that's a piece of information we need, and so we definitely hear that loud and clear.

MS. LEE: I agree with Amy, in that you want to consult with the analysts and see what they're going to use the data for, and so that will help us guide what data elements we need, and if they're going to use it for say CPUE indices, you definitely are going to need target species, I would think, and detailed information on effort.

DR. BELCHER: Adding on to the idea of the discards, and I know this may not occur with federal for-hire vessels the way it does with some of the state ones, but I know, just thinking about the example that we've been dealing with in red drum, you've got issues of they're not just regulatory discards. There is conservation discards. Some of these folks don't want to keep every fish that they have, and so just saying discard fate doesn't really tell us anything about an animal going back, plus some of our guides actually have more stringent size restrictions on their catch, and so like sea trout is one where we have a couple of guys that they don't want to keep fish over eighteen inches.

You can catch them, you can tag them, you can look at them, but they self-impose that you will not keep fish over eighteen inches. Without those types of things, it's very difficult, I think, to get at that discard, because your assumptions are that it's always regulatory related, over creel or undersized or outside the size slot.

MR. CARMICHAEL: I agree the discard stuff is useful, and I'm well aware of how it's used in assessments and the relation between mortality rates and depth, and we have some of that data, but it always seems what stands in the way is having accurate data of where the fish that were discarded, what depth they were captured in, and so my biggest concern is just knowing that this is trip-level reporting, will the depth variable be of use to the analysts? I definitely agree with -- You know the reason we're bringing this to you guys is because you guys are analysts, and you're involved in this stuff, and you participate in SEDARs and you know how these data are used.

To me, the big question is, on trip-level reporting, is that depth of discard, or just general depth of that trip, going to be useful for your discard information, and think that you have a trip that discards black sea bass and vermilion snapper as well as blue-water species and lands dolphin as well as red snapper and black sea bass or something, and so what would do with the depth information

that you had, and is it really the solution being that this should have some sort of supplement, where some subset of people report set-level information on discards, so that you can gather the extra detail?

That's what I'm really thinking of in terms of dealing with discards, is, from the analytical standpoint, will the data collected at the trip level be considered reliable enough to have it used in terms of refining discard mortality rates by depth and allocating discarded fish across different depths? If no one really thinks it will, because of the compromises that are required in trip-level reporting, then I would think that it's really not useful and a recommendation should be the appropriate way is to collect the necessary detail on discarded fish, including asking for some measurements and some fates and some reasons. What depth did you catch that fish and why did you throw it back?

DR. BELCHER: I am directing this to the analysts, but, knowing that we've talked about these depth closures, and they've been broader bands of bathymetry, are you better suited to go with a bathymetry, in that sense?

DR. SHAROV: With so much focus on discards, and I am not surprised, because this is one of the most uncertain, not well defined data sources, but, with respect to the use of depth information, probably, in relation to the actual catch that is reported, it's more important that this first step, where you could use this information. We, earlier today, were looking at the results of the paper that analyzed the distribution of red snapper by age and size relative to depth, and so there you are.

With respect to discards, it's probably maybe the distant future, maybe not so distant future, but certainly, like John Carmichael said, if somebody will come up with the assessment methodology that would consider different rates of mortality, depending on the depth, that would be useful information. We can easily make probably ten pages of variables that we would want to be collected, but we have to be realistic. For example, in the headboat, I cannot even imagine how -- You have like thirty or forty or fifty customers, and would you follow the discard fate, or even just the discard amount? You have so much action. You have so many anglers. Unless you have a biologist or an observer onboard, there is no way that the captain would be able to provide you with this information. It's nice to define what we would have wanted, but it certainly should be also checked with respect to the actual practicality or possibility of obtaining this information.

DR. BUCKEL: I agree totally with Alexei. If you ask for a lot of these things, they're going to fill it in incorrectly, and so you get the wrong information, because the box has to be filled in and they didn't get the discard fates, but they need it. They need to fill the box, and so they put in something that's a guesstimate, and we don't want that, and so I would -- I think almost having a meeting of some of the analysts with some of the charter boat and headboat captains would be helpful, to say, okay, to get some realistic -- What would be a good expectation of what they could record?

I would try to simplify this as much as possible, to deal with the issues that Alexei brought up. Certainly hours fished, just going through this myself, you've got the half-day and the full-day, and so that's critical. Laura Lee already brought that up. I do think if you have things -- If you have a trip, to speak to John Carmichael's question, if you've got a single trip that targets two different things, then you would subset that.

You would have black sea bass for four hours, what was the depth range, and then the next four hours was trolling for dolphin, the target species was dolphin, and dolphin and tuna were caught, and so that's one way to deal with that. I think, for the catch and discard by species, just keep it depth and maybe have ranges. For black sea bass, for example, 100 to 120 or -- I'm just trying to think of like a drop-down menu, so that the data can all be binned, and this, again, is where the analysts could work with the folks that are on the water to come up with that. That way, it's going to simplify the data analysis down the line.

To me, the -- Alexei already spoke to discard fate. I think depth would be enough. Depth seems to be the, especially now that we have circle hooks, the depth seems to be the major factor influencing the discard fate, for at least the bottom fishes, and so I think, if you had that, you could get -- That's the big thing we're missing now, as Chip knows. He's done enough of these discard mortality that you --You've got great studies that show how discard mortality changes with depth, but then you don't know what depth the discards are coming from, and so this would be incredible. This would be a huge improvement, to get at the depth.

DR. SHAROV: Real quick, several of my colleagues have already said that the best way is to ask analysts. Well, I guess the council presumed that the analysts are here, and so that's why we're being asked. Nonetheless, not to put them on the spot, Kate or Genny, but I think the ideal way, probably, of going with this is select say the ten most important species, recreationally, where the headboats and charter boats are playing the most important role, and either take a look at the assessments for these species or contact directly the lead analysts for each of them and ask them which exactly data you need or what was missing that you wanted to be there to get the best information for your assessment. I think, Kate, for example, easily could have gone through this today and, in ten seconds, she would have told us what exactly was needed and what was missing.

DR. BARBIERI: Thank you, Alexei, and my question is more on the other side of that coin. I mean one is the analysts, and I think that's been articulated very well, but I was wondering, how much success have you had sort of presenting this to the fishing public? Have you been able to make some kind of engagement? Unless they buy into this idea, it's going to be perfunctory type of reporting that may or may not, and so I know this is a very difficult topic, and I'm not trying to throw a curve ball here, but I'm just wondering whether some of this stakeholder engagement has been attempted, because it's something that I feel needs to include the scientific side and the stakeholder side as well.

MR. CARMICHAEL: It has, and, through the public hearings, we talked to a lot of fishermen, and the variables, as I said, was one of the topics that came up a lot. The fishermen would make comments like why do you care how much fuel I used or how much ice I used? They're coming from the perspective of they want to provide the information that's needed, but they don't want to provide things that someone just thought might be interesting to have someday.

That's, to me, where this sort of depth thing comes in. They report depth in a lot of programs, but yet, we don't feel that we have reliable information to allocate discards adequately by depth to apply a depth-based discard mortality rate, and so my thought is, unless the analysts think that getting that for a trip, which might have stops that cover a several-hundred-foot depth range, maybe, just getting one depth value for a trip, is that going to be useful for that exercise or should we really just say you need trip-level for that, and, for purposes of solving these needs, just getting

the counts of species may be enough, because that could get into the kind of thing where fishermen feel like, okay, I report this data, but you never use it.

It's the same with the location. Would we be in a situation where, if a guy stops in ten or fifteen different places or he covers from the marina thirty miles offshore and back and stops coming and going, having him pick a spot that's one nautical mile, is that going to be useful to us, in terms of doing area-based assessments? Maybe or maybe not, or is ten nautical miles going to be adequate, if we ever want to get to say spatial-type assessments? From the analysts that are here, that's the kind of stuff that would be helpful for us to know, because there's no sense in asking for one nautical mile if you're going to be -- At the trip level, what does that really tell you?

DR. BARBIERI: I think I'm going to get a little bit ahead of Zack here on some points, because he made those points last week or the week before or whenever, at the MRIP Review Committee meeting, but it's the articulation to the stakeholder base that so much of this uncertainty, so much of this lack of data, is really generating more difficulty with management, and that having them understand that providing this data may not be their first choice, but it may help the process, and you end up with less uncertain, more accurate assessments that are more representative of what they're seeing out there, and that might be a good selling point for them to understand the value of this and that they need to provide some of that data themselves.

MR. BOWEN: Thank you, Mr. Chairman, and I just wanted to -- When you were talking about the stakeholder-driven, you know we've had public hearings on this. I am a federal permit holder. I'm a charter boat guy, and the majority of the people that I have spoken with, the comments that I have read, are behind this. We want better data, we want accurate data, and, from a stakeholder position, I don't think asking for location in that nautical-mile block, or whatever it ends up being, and depth and catch is -- I don't think that's asking too much, and I think the majority of the stakeholders, like I said, that I've talked to are definitely willing and ready for this to come online.

DR. BARBIERI: Thank you, Zack.

DR. SCHUELLER: I guess I just have an example to illustrate how much thought, how much more thought, needs to go into this. On the menhaden side of things, we have set locations, and, while we may not separate -- We don't talk about discards with menhaden. What we do talk about though is we do have a spatial component on the Atlantic side, where we're separating the fleet by space, because the composition data are different, and, therefore, the selectivity are different, and it has different implications, and so it's not -- I mean there is multiple ways this could come into play, and I'm sure it will be species-dependent, and so this is a very complicated question, and I really do think that it's good to get the fishermen involved, but it's also good to get the analysts involved for the different species of interest, because each species may be different.

DR. YANDLE: I just wanted to comment on the social science-relevant data in this, and it was actually very interesting to me that when I first started looking at what was up there, it was like, oh, all there is is number of anglers. Then, as the discussion went on, more and more came out. Like I think you said at one stage that you deal with fuel and ice purchased per trip in there. Is that correct?

MR. CARMICHAEL: No, we're not proposing that.

DR. YANDLE: Okay. You weren't. Okay. Anyway, I was just wanting to make a comment to look -- It would be a really good idea to look for low-hanging fruit that could also be helping with the socioeconomics on this, and one thing I was thinking about is just even something as simple as we have number of anglers, but what about how far the anglers traveled to make this trip? Where are they coming from? That was one of the basics that Scott and I were discussing could be useful, and looking for other data points would be useful, both from a biological and a social science perspective.

DR. BARBIERI: Thank you, Tracy.

DR. SHAROV: With all due respect, I disagree, because there is a difference between the survey and the weekly trip report. Requiring a captain to report on the weekly trip format where the anglers came from, I think that would be probably -- In my view, that is difficult information to obtain. Again, I understand the need, but that would be probably more appropriate for the survey, rather than on the regular fishing trip, because it's not as simple to collect that sort of information. I might be wrong, but that's just my perspective. I just wanted to respond to this.

DR. BARBIERI: Thank you. Any other points? John, I am actually extremely happy with the responsiveness and engagement of the committee in providing input. Of course, there is many different ways to get this accomplished, but I think that we have captured there some good suggestions and recommendations, right?

MR. CARMICHAEL: Yes, Mr. Chair.

DR. BARBIERI: Any additional comments or questions for John on this agenda item?

DR. SIEGFRIED: I think everything I presented this morning gives support as to the need for this data. The depth fished, even if it's for multispecies fisheries, is important, and if we could get at more information of what they spread their effort out on, that would be great. If you get hook size, hook manufacturer, that would help a lot too, and so I think there's a strong need for all of this, and I don't think it needs to be super complicated. That's just a start.

DR. BARBIERI: Thank you. I think this completes then our Agenda Item Number 8. Now, I think we want to recess for the day. We may have some items that we -- Unless the committee feels differently. I mean if there is a general -- Let me see a show of hands of how many people would like to go through one or two more items this evening.

DR. SCHUELLER: Since John is already in the hot seat, how long is the Citizen Science Update?

MR. CARMICHAEL: Not very.

DR. SCHUELLER: I say we check that off.

DR. BARBIERI: John, you're right there.

MR. CARMICHAEL: Citizen Science. What do we have up here? Citizen Science, I'm trying to think of what we told you guys about this last. You know the council is looking at a Citizen Science Program, and the idea is to have something that's robust and provides useful information,

to not get things where fishermen say, well, I participated in this and I gave you data and then it got to the SEDAR and they said they couldn't use it for this reason or that reason or whatever, and so that's one of the reasons that the council has taken such a slow, careful, measured approach to this.

We're guarding against that failure, and there was a workshop help in January, where seventy people or so -- There was a huge number of people that came, scientists and fishermen from all around the region, data collectors and data providers and data managers, and talked all different manner of citizen science programs, considering everything from handling the volunteers to fishermen as volunteers to reviewing potential proposals that might come forth, reviewing projects to make sure you're getting robust data, how you would manage those data, so that they're accessible, because that's another big concern, that projects are done and data sits on shelves and no one can ever access it. It was really kind of dealing with all the issues that have come up in the past with various research-type programs.

The result of that was a Citizen Science Blueprint that went to the council, and it was reviewed by the council, and received very well. Now we're in the process of trying to find really some funding to support a staff person that would come in and get this program going, get a website going, get the governance type of structure in place, get volunteers lined up, learn how to manage these volunteers and manage the projects, promote it so you get people excited, and get a couple of kick-starter programs going that will show that this works and get us some data that would be useful, but, of course, kind of avoiding, obviously, in the beginning, data that could directly shut down a fishery, because that has a lot of concerns with it

The council is continuing to push ahead on this. There will be a Quest webinar coming up. Julia Byrd, Amber VonHarten, and myself are doing a Quest webinar, and I don't know if you all are familiar with them, through NOAA, and that is May 25. May 25, we're doing that, and a flyer will be going out about that pretty soon, and we're just going to talk some about the need, about things that have happened in the past, and then where this program is seen as going.

We will report to the council in June on this, and there is Amber and Julia and Ben and Mark Brown from the South Atlantic that are going to be going up to the IMCC, International Marine Conservation Congress, meeting in St. John, New Brunswick, the first week of August, and there's going to be a discussion, a round-table forum type of thing, where they're going to have some talks on it and then have a discussion about the citizen science program up there, and there's expected to be a number of citizen science type of experts that are going to be there.

One of the cool things is it's been through some of the efforts of people like Leda Dunmire, who has helped get us in connection with folks at the Cornell Laboratory, the ornithological laboratory, who have just done a ton of cool work on citizen science with their bird counts and stuff. It's Rick Bonney and Jennifer Shirk, but those guys have really been -- They're very excited about it, and it's been a great resource for us, in help directing our program, and just huge support from the council in general. I know some of you guys were there and kind of saw what's going on. We're continuing to push on it, and hopefully we can get some funding at some point to really get to the next level. Any questions on citizen science?

DR. REICHERT: To that point, I want to commend the organizers, because this was a very large, very, very diverse group. I attended that meeting, and it was very, very well organized, and so I

want to commend you guys for hard work you put in. One of the things that came up there that's relative to the SSC is that the SSC is probably going to be asked to be involved, to make sure that the scientific integrity of the data that's collected and the usefulness for management and stock assessments is -- The SSC is involved in making sure that the quality of the data is ensured, and so I assume, John, that at some point we will hear a little bit more about that and how we as an SSC can be part of that.

MR. CARMICHAEL: Yes, you definitely will. We will keep you posted, and we will look for you to be involved in things, for sure.

DR. BARBIERI: Any other questions? Folks, now we have Agenda Items 7, 9, 16. Number 14, we cannot do today, because we don't have Sherry Larkin. It will be better to do that tomorrow. We have 17, but I don't know if we want to have Myra here for that. How about 18 or the decision tool?

DR. ERRIGO: The decision tool discussion will be long.

DR. BARBIERI: How about Number 7?

DR. ERRIGO: That might take a while.

DR. BARBIERI: Socioeconomic Panel Report?

DR. CROSSON: I can briefly go through the section on optimum yield. The other parts that are on the hogfish and on red snapper are kind of relevant to the action items that the SSC has, but I can go through the optimum yield discussion if you want. I won't take too long.

DR. BARBIERI: Marcel, do you prefer to do that tomorrow and so recess for the day?

DR. SERCHUK: The optimum yield, as I understand it, and I attended part of the meeting, but the optimum yield is completely different from the other parts of the report, which we will get to tomorrow, and I would rather spend ten minutes going over the optimum yield, because it will not break up our discussions tomorrow. It's relevant to today, and so if we could perhaps just spend ten or fifteen minutes on it, it wouldn't get in our way tomorrow. The rest of it will be relevant for us tomorrow.

DR. CROSSON: Right, and what I would propose, if I do this, is that I go through the optimum yield discussion, because it's not related to anything else in the SSC briefing book. The sections that are relevant to hogfish and red snapper, I will deal with those points, and that will be that there will be no other SEP report. Does that sound fair? Okay.

The SEP met yesterday morning, from 8:30 until about 11:30, and the primary -- The first couple of hours we spoke, and we had a pretty good attendance. I think there were seven of us in the room, plus we had Kurt Schnier, who called in at five-something in the morning off the west coast, and John Whitehead called from his mountain retreat, and somebody else called in, too. Chris Dumas called from Wilmington, and so we had good attendance through that, and we had a pretty good dialogue going on.

On the optimum yield discussion, you can look at the stuff. We just sent out the SEP briefing book. You can look at our more exact recommendations, but we were asked a number of kind of very broad questions, and one of the contexts for this is the question -- It's very difficult to discuss this without getting into allocation discussions, because some of the council actions that have been related to especially taking, quote, unused allocation from one sector and temporarily or permanently reassigning it to another are designed to perhaps move the fishery more towards an optimum yield.

Those concrete examples, especially for mackerels, came up repeatedly in the morning, but I think we did a pretty good job of balancing out the theoretical and kind of the real hands-on type of stuff, and so, in terms of defining optimum yield, Magnuson obviously has some definitions of it. I know that there is further guidance, I think, coming out from NMFS in the near future, but we were trying to give recommendations that are actually very useful to the council in a hands-on way, and so there are lots of different definitions of optimum yield.

There are lots of different variables or standards that you can talk about. There's MEY, maximum economic yield. You can talk about dependence of waterfront, you can talk about employment, you can talk about the distribution of seafood, which I hear commercial fishermen talk about all the time, to the non-fishing public, and then a big one that we talk about often is fishing opportunities, which is just basically CPUE on both the recreational and the commercial sector, because if we're talking about taking unused allocation and reassigning it to a sector that's more likely to harvest it, it affects the success rate for everybody, and so that's a big concern that the recreational fishing community would especially have if they aren't using all of their quota and that is reassigned to the commercial sector somehow. That might decrease the opportunity to run into a dolphin or something, and so that's in our report.

Chris Dumas, we thought, came up with a great way of conceptualizing this overall. He said that the way you should think about optimum yield is that you have three buckets, which, of course, fishermen love that, right? In one bucket, you have commercial value, which is going to be the harvest value, the consumption value, minus whatever the harvest cost is. In the second bucket, you have the recreational value, which is going to be willingness to pay, consumer surplus, again, minus the harvest costs. Then the third is what he termed, and what we agreed with, is a reserve value, which is both the non-consumptive value of the fish, which is that warm feeling you get knowing that there are groupers out in the ocean, and then it's also the contribution of the stock to your ability to catch fish in the future.

If you have B over BMSY as a relatively high number, that's going to increase the CPUE for these other sectors. Now, the goal for optimum yield is to equalize the value per fish across those three buckets. It's going to take time to get there, but, as you start moving around, you are therefore moving in a direction that increases the total value. You're moving it from an area where it's less valued to an area that is a higher value, and keeping in mind the reserve value is a real value. An unharvested fish does have a reserve value. It has a shadow price, which is basically what would happen if you pulled it out of the water right now. If it's on the end of a hook, that price is still out there. It's just not being harvested right now. It's wealth that's not being used.

Based off of all of that, we came up with a list of things that the council should consider if they're talking about temporarily or permanently reallocating, in an effort to move toward optimum yield, and that's the list of nine items that we have at end of the Section 2 of our report. How certain is

the assessment of the stock? That's obviously a big one. Is it a risky thing to consider? Again, this is in the context of thinking we're not harvesting all of the fish right now and we might be moving some of the fish between sectors.

How certain are you in the assessment of the stock? What's your confidence in it? How quickly can the stock rebound if you move it to a sector that is much more likely to harvest it? Related to that, you should -- Something like dolphin, where in four or five years it's going to bounce back, it's obviously quite different from red snapper.

When you're moving fish, you should be considering doing it incrementally. You always keep some of the fish in the reserve sector, and you don't make any big, large decisions and see how things rebalance over time. Again, leaving fish in the reserve sector is going to help keep encounter rates higher in future years, which is going to affect both the commercial and recreational anglers.

Then the bigger the shift and the more long-term the shift that you're going to do, the better the justification that you need to have and the better the data needs to be to support it. Think about the timing between recreational and commercial demand for the fish. There may be an overlap or they may be in different times of the year, and, of course, keep in mind the geographical dispersion between the South Atlantic and -- I mean we always see this with the states up in the north versus the South Florida fishermen. There's a demand for different regions, especially in our region.

Commercial benefits, you can almost always immediately estimate what the commercial benefit is going to be, because most of the fishery markets are fairly elastic, but just because you can't as immediately estimate the recreational value or the reserve value, it doesn't mean that those values don't have some existence, and so keep that in mind, and that's the shadow value that I was talking about, and, of course, an obvious one is, is the reason that a fishery is not harvesting its ACL due to regulations inside that sector? The example is brought up of moving from a ten-fish bag limit to a fifteen-fish bag limit. In Spanish mackerel, it didn't mean anything at all, and, to some extent, the recreational sector wasn't even necessarily happy with that, because the charter captains had to fillet more fish.

If it's not a regulation that's sort of causing it, then that's more evidence that the marginal value of the additional fish to that sector is low, and you don't even have to have a willingness to pay estimate of what the additional fish is going to be of that eleventh fish. You can even, perhaps, think about, politically, if the council doesn't get yelled at if it starts to -- If there is no demand for it politically to keep increasing the number of fish in the bag limit, then it's probably evidence that there's not a lot of high value in that additional marginal value per fish.

The elasticity of the fishing market for the commercial species, I mentioned, and then the last thing is keeping in mind that a lot of our fisheries are both, especially in the commercial sector, are multispecies, and so even though a fish may be a small portion of a portfolio, it might be the part of the trip that gets the guy over the edge into profit, which is something we generally think about when we look at the potential impacts. If anybody has any questions on that, they're welcome to ask me or any other member of the committee, which at this point would just be Tracy, and so ask Tracy. The other stuff, again, I will get into when we come up with those items, and so that's it.

DR. BARBIERI: Thank you for that, Scott, and I was going to ask you -- You said that this was sent to us already?

DR. CROSSON: I believe an email was sent out by Mike.

DR. BARBIERI: Very good. Okay. Any additional questions or comments?

DR. SHAROV: I am just curious on the recreational bucket out of the three buckets, how you can place a value on the level of happiness and how much the person can spend to be happy. That's one part, and then the second part of it as well is that it's a multispecies, obviously, issue, and so it would be harder to sort of define an optimal yield of the recreational component just on say red snapper, because -- I might be wrong, but, often, they are pursuing several species and, therefore, you would have to look at in some sort of multispecies context.

DR. CROSSON: I will answer your second question first. That's a really good point, in that sense that there may be substitute species, that if you took one fish out of the equation that a recreational angler might be just as happy to catch. I've seen people catching pinfish and being happy off of some of those headboats. I don't think all anglers would be happy with pinfish, but that's just one option.

In terms of how you can measure happiness, our group is not entirely economists, and so there might be differences of opinion on that, but I always think about this that you can measure it, to some extent, and I always think of the summer of 2008, when the price of fuel went through the roof, and I watched the number of boats going out of Bogue Inlet just dwindle to nothing, because it got too expensive.

What did that mean? It meant that the value that they would receive from that trip diminished to nothing, because it was costing five-dollars a gallon at the dock, and so you can look at that. That is not even a willingness to pay measurement. That's an actual just valuation you can do, and so I mean there's usually a -- Not everything can be measured in dollars, but I think, with fishing, that you can usually find a point at which somebody is not willing to go anymore, because the price has gotten too expensive, and they can do something else with their money.

DR. BARBIERI: Any other questions from the committee for Scott? Hearing none, we will recess and reconvene tomorrow morning at 8:30. Thank you, everybody.

The Scientific and Statistical Committee of the South Atlantic Fishery Management Council reconvened at the Town and Country Inn, Charleston, South Carolina, May 5, 2016, and was called to order at 8:30 o'clock a.m. by Chairman Luiz Barbieri.

DR. BARBIERI: Good morning, everybody, and welcome back to day three, the final day, of our May 2016 South Atlantic Council SSC meeting.   Mike E. just sent out a revised agenda order. We tried to get a bit organized, in terms of timing, of these different things, because we were jumping all over the agenda, and he was kind enough to put together a list for the order of how we're going to be going through our items today.

In terms of announcements, we have George Sedberry calling in. We should have still Brian, and then we're going to have Anne joining in through webinar a bit later this morning. She is just dealing with some issues at home, and she's going to be able to join us by 9:00 or 9:30. Then, lastly, our Vice Chair finally, despite having a Y-chromosome, he decided to go to the doctor to look at his ankle. Apparently, it was his wife's idea, and he decided to go, and so he's going to be

here a bit later. He said that the doctor's office is actually fairly close by, and he will be able to join us probably by 9:30 or so.

We're going to start by going through a very quick presentation. Genny was kind enough to put together a few slides to show us the P* projections at 0.3 for golden tilefish, and that will really complete everything that we need for that item.

DR. NESSLAGE: Thank you, Mr. Chairman. I did also, before I show you the P* results, pull together the CPUE, as Fred had requested, with the upper and lower 95 percent confidence intervals. As, I think Fred was indicating, we may be making more out of that little bit of decline at the end of the time series than is warranted, given the amount of variability in that index, and so that's something to keep in mind, especially for the benchmark, and so that's something to keep in mind, especially for the benchmark, and especially if there are new indices available at some point, developed, perhaps, in concert with the Mid-Atlantic. That would be wonderful, because it's a highly-variable index, at least as the CPUE has gone up, especially. Are there any follow-up questions on that? Okay.

Then the P* projections for 0.3 look quite similar, barely perceptibly different from the 0.35, but, if you do compare them, remember that I only went out the three years, as requested, from the management starting in 2017 to 2020. We've got spawning stock in the upper left, biomass in the upper right, abundance, recruits at the bottom, and then our Fs dropping, of course, with the new management measures. Landings are dropping by about half, and then the probability of SSB being above the MSST threshold going up. Then the last slide is our tables that I thought you would need to complete your tables. Any questions?

DR. SHAROV: Genny, you have two constant Fs, one for the first three years and then you dropped in another?

DR. NESSLAGE: Right, and so this is set for management to begin in 2017, and so that's why you see the three years, essentially. You keep the terminal F, and then 2016 and 2017, because terminal year is 2014 in this assessment, and then you would implement your management measures to achieve that P* that would be effective -- You would see the effect of that in 2018. Does that make sense?

DR. SHAROV: So this is the F corresponding to F* of 30 percent?

DR. NESSLAGE: Exactly.

DR. SHAROV: Was there any consideration of a constant catch scenario rather than a constant F scenario? I don't know if it was ever requested, but I'm just curious.

DR. NESSLAGE: No, it was never requested, and I didn't explore that.

DR. CADRIN: Was there any discussion on what the best assumption for 2015 to 2017 catch is? Should be it be the ACL or should it be status quo F, or is that part of the benchmark method as well, that we really try not to revise?

DR. BARBIERI: Well, no, I don't think it's like a part of the benchmark. In this case, I don't know if we have in our terms of reference any specificity about how to configure --

DR. NESSLAGE: If I remember, and I can go back at my notes, but there were -- I think, and maybe Erik can correct me if I'm wrong, if in the last three years you're over your ACL, then you implement the average of I think it's the last three years in the projections, and so that's what that F represents, because all three of the sectors were over in at least one of the last three years, some of them all three years.

DR. CADRIN: That sound reasonable to me. Thanks, Genny.

DR. ERRIGO: I was just conferring with John, and I think we may also want to see projections where the effect of management happens in 2017, because, if it's a change in the ACL, we now have a change in the management procedures, so that it's a framework action, and it can be done relatively quickly. We may be able to actually have it in place for the 2017 fishing year.

DR. NESSLAGE: I can run those really quickly and just give them to you, so you can fill out your table. Does that sound reasonable?

DR. ERRIGO: Yes, that's fine. You can just send it to us.

DR. NESSLAGE: I will also do a quick little write-up that can be appended to the assessment report for staff.

DR. BARBIERI: Any other questions for Genny? I guess not, Genny, and thank you so much for doing all of this overnight. Our next agenda item is for me to go over a few points, similar to what I did yesterday with red snapper, about the gray triggerfish, SEDAR 41, the other species that was in that SEDAR set of assessments for SEDAR 41.

Basically, what I did here is you have all of this in the review report, the review panel report, for SEDAR 41. We produced actually two reports, one for red snapper and one separate one for gray triggerfish, and so what I did is basically just pulled from the report a few points to sort of facilitate this morning's discussion as we go through the assessment review.

We had two models as well that were used for this. The BAM was the main model and we had ASPIC as a secondary model. The assessment ended up -- I mean the BAM was chosen as the model to provide actual stock status and catch level recommendations, but the panel actually decided not to accept this assessment as representing the best available science. It's been recognized that there was a great effort on the part of the assessment team, the lead analyst and the rest of the team, in putting the assessment together, but there were a lot of uncertainties, problems and issues, with data and parameter estimates that couldn't be resolved within the timelines of the review workshop, and so there was an attempt to make some corrections and produce a revised model as the base model, but that did not stick, basically because there wasn't enough time to explore all the things that could be explored, and so the panel decided not to complete that assessment and rejected the assessment, in terms of providing stock status and management advice.

One of the things that was, I think, key in this process was an error in the database, as identified on the chevron trap survey age composition data, and this was discovered sort of halfway through

the review workshop. Katie, yesterday, brought this up in regards to red snapper, regarding red snapper. It wasn't really as critical. The changes in the ages did not really cause any major change in the model results, and it did not provide any unreasonable diagnostics, and so it was something that really didn't have any substantial impact on the model, and the model was accepted. In this case, it was different. The magnitude of changes to the data results and the model diagnostics really caused a lot of concern on the panel, and they decided not to accept it.

One of the issues that came up was the assessment panel had recommended upweighting the CVID survey. That's the combination chevron trap and the video survey, as Katie described yesterday for red snapper, and so the landings and the indices were relatively flat, with little contrast, and the model was having trouble fitting, and the review panel decided then to make a recommendation to upweight the CVID, which ended up getting over fit to that data, and that brought concerns with the panel as well, and so that model was rejected.

Here, you can see some of the issues that came up. You have this in more detail in the review panel report, but you can see, on the left, the exploitation history of the stock here in the South Atlantic, and then, on the right, is the abundance at age estimated from the revised, corrected model. As you can see, it's producing some results that are really not considered plausible, because, as exploitation and landings go through that time series and increases, you actually have a decrease in -- At the beginning of the time series, you have lower abundances estimated by the model when the exploitation was much lower. Steve and Church, anything you guys want to add to this?

DR. CADRIN: I agree with that entirely. In fact, while this is up there, even the model that came out of the assessment workshop had this feature with the first year, and it really became related to weighting of the different components, particularly the upweighting of the trap survey, and the model -- From my perspective, the model was estimating low abundance in the first year to match the first two years of the trap survey, and so what looks odd is, on the left, you have what looks like a fairly typical fishery development, where we typically see a drawdown from near unfished stock sizes, and we start with a very low abundance, which raised a red flag, and it does seem to be that the model is over fitting those first two years of the survey.

There was a hurricane that came through that affected several stocks. We just had a lot of concerns about that, and then when the age composition correction happened, as you correctly said, unlike red snapper, there was a major change in the data. In fact, for 1991 to 2007, the age compositions were off by about a year, or they were corrected by about one year, but 2008 to 2014 were not corrected very much, and it wasn't even like we were making a consistent change through the series.

We felt we had an entirely different model that still had this concern with data weighting, and so that's what led to this unfortunate decision to have to reject the model. However, I think we also felt that all the investment in the data workshop and the assessment workshop leaves us in a good place to resolve these issues, to perhaps evaluate different weighting schemes. Is there justification to either downweight or remove that first two years of the survey? I don't think we need to start from scratch here. I think there's a lot of valuable information in the assessment. It's just unfortunate that we came in with concerns and had to make a major correction mid-week that led to the decision.

DR. BARBIERI: Thanks for that, Steve. Church, anything to add?

DR. GRIMES: I think you guys have described it more than adequately, but one thing is the reason that this was redone from before was that the aging was not considered adequate or accurate, and that's been redone, and that was a big improvement, and so that was a good thing about it, I think.

DR. SHAROV: I'm wondering if the important issue was the model performance or the estimates in the initial years, and then just the simple start of the model, two years or three years later, was this tried, a run of the shorter period than -- If it results in stable estimates, particularly in relation to most recent years, then all right, but was this tried at the time of the workshop or not?

DR. BARBIERI: I don't remember, off the top of my head. This is described in more detail, Alexei, in the actual report, but, because, as Steve pointed out, this issue with the error with the aging -- It came up fairly late. I think it was Wednesday, halfway through the week, and, really, with red snapper and still a lot of things to be done for red snapper and having to try different runs and examining diagnostics and having to handle all of this, it was just too much to have this completed there at the workshop, and the panel decided it would be better for this to be handled separately, with proper attention and time to discuss all the issues. Then I think I have a couple more slides, just to show briefly.

DR. BUCKEL: I just want to add that we did talk about some of these things at the assessment workshop, and one thing that seems odd, for gray triggerfish, and one reason we were okay moving forward out of the assessment workshop, is with the increase in abundance, it was, even with that fishery development that Steve was just talking about, you didn't see any truncation of the age structure. Those older fish were still there, suggesting that there wasn't that mining that you would see in a developing fishery, if it was having an impact at the population level, and so that did give us the confidence that the population wasn't getting that impact like you see, because you didn't see the loss of those older fish in the population, and so I just wanted to bring that up, as we discuss how to -- I'm not sure if we're going to give recommendations on how to move forward, but that could come into play.

DR. BARBIERI: Right. Absolutely, and I think one of the main points here for this discussion is to try to bring your input on the way forward and some recommendations on the way forward, for how we're going to handle gray triggerfish. There are other issues that came up as well that we can discuss as the presentation and discussion goes forward, but I felt, personally, that the estimate of natural mortality that came out of that new Then et al. paper produced an estimate that seemed not to be reasonable, and, since then, actually, we have been investigating further.

I have actually talked to John Hoenig a couple of times about this. We have been looking into that database and trying to get to the bottom of that and have found a number of issues with that database, the meta-analysis that was developed. John actually pointed out that one of the things that they did was to post -- They have posted the entire database, using that meta-analysis, to request input from folks in corrections and adjustments, because the database is so large and such a large number of species that they couldn't really check on the accuracy of all the estimates that were there and using different methodologies, and so this is something that I think we need to explore further.

I actually discussed offline a few months back, after the review workshop, with Mike E., Marcel, and John Carmichael in thinking about inviting John, and perhaps Amy Then, to come over and give us a presentation and have some discussion about that, because if this latest version of the Hoenig equation is going to be considered the best available science, we need to understand what's going on there regarding the values that are coming out.

They are very, very different from the values that would come out using the older equation, and so this is something that we need to explore, because it had some impact on red snapper as well, but not as significant as it had for gray trigger, but it had some impact as well, and I think that, before we start applying that new method as the routine method for natural mortality estimates in the South Atlantic, I think we need to explore it further, but yes, Jeff, that was a very long-winded way to say yes, we need to start discussing recommendations going forward.

DR. CADRIN: Yes, and I think Jeff points out something that the review panel also recognizes, that at the current levels of catch, the surveys are relatively stable and the age composition is relatively stable as well, which may give us information to base an ABC recommendation.

DR. BARBIERI: This is just for you to see some of the impact that concerned the review panel. Here, you have the average selectivity. This is the age composition, observed and predicted age compositions of the chevron video trap survey. The base model is on the left, and the corrected data and revised model are on the right, and so you can see a very different fit there that couldn't really be all resolved. The issues couldn't be resolved right there and then, and it caused the panel to recommend that this be addressed at a later assessment.

Then, finally, just to show you some of the alternative index weights that were discussed and attempted at the assessment workshop and then the recommendation to upweight the CVID, to get better fit to the CVID, and ended up generating concerns with the review panel in regards to over fitting that. Basically, the panel considered that the upweighting of that index was causing the model to basically be fitting to noise, to the sort of interannual variability there, instead of identifying trends in abundance, and you can see, in the upper left, that when you have no upweighting of the index, it's pretty flat, with some ups and downs, but around an average abundance level with no real contrast.

I think this is the last one. This is just to say that, despite the issue with the BAM, there was also an issue in getting a reasonable solution from ASPIC. The panel felt it was most likely caused by that little contrast and the removals and the index time series, you know landings and the indices. Despite some fluctuations up and down and noisy variability there, it didn't really show any trends going up and down, and it couldn't really be producing any reasonable ASPIC result. The B1 over K values that were coming out were thought to be unlikely and not producing anything that could be used for stock status and management advice. Steve and Church, anything there on the ASPIC?

DR. CADRIN: No, I think our report is pretty explicit that, because of the low contrast in the landings and indices, the ASPIC results are not informative.

DR. BARBIERI: All of this, again, is in the review report. The figure is included, and a more detailed description of the discussion held by the review panel. This opens up then for us to start discussion on the way forward, as Jeff brought up.

DR. SERCHUK: Presumably there is a method that has been used to set ABCs prior to the current year, and so what is that? In other words, what I understand is nothing came forward from the workshop in terms of a new way forward. Is the default then that you revert back to the existing procedure? I'm not really quite sure what the default is here, and it would be helpful to know that, in relationship to research recommendations, relative to when we might expect possibly another look at this stock, with respect to a better model or a better way of handling the assessment.

DR. BARBIERI: Yes, Fred, and very good point. My understanding, and, Mike E., jump in and supplement what I have to say, but my understanding is that because we don't have an accepted new assessment that we can either go through our control rule, our ABC Control Rule, and say, okay, no quantitative assessment, and we go to the next year, the next year, the next year, until we feel that we get to the point where we have the information content to provide an ABC recommendation in accordance with that tier within our ABC Control Rule. What we have right now is --

DR. ERRIGO: We have ORCS. It was done through the ORCS method, and without this quantitative assessment, and the production model also didn't work, it would fall back down to ORCS, and so we will basically keep the same ABC that it has now.

DR. SCHUELLER: Does the SSC have the option to say we acknowledge the current problems with the assessment, but a lot of work has been done and we can pick that up and fix those things and then move forward with it?

DR. BARBIERI: It depends on what you consider fixing those things. I mean the SSC can provide alternative recommendations on catch levels. We can depart from our ABC Control Rule if we can provide proper justification, and so if you have some ideas on how to take some information from this assessment to provide catch level recommendations, we can. We just have to justify -- According to NS 1, ABC recommendations have to be made according to our ABC Control Rule, and our ABC Control Rule Tier 1 is if we have a quantitative assessment, we go through our P*, if applicable, procedure and develop an ABC recommendation from there.

If that quantitative assessment is not an option, it's a matter of this committee to weigh in on that, if it is an option or not. If it isn't, we just go to the next tier down. In this case, it would be down to Level 4 or 5 that is the ORCS tier.

DR. CADRIN: Building on what Amy suggested, I actually think we should recommend that. I think that there is a great deal of investment that's been done. There is a lot of information to go forward with, and that we should recommend that an assessment can be developed, that there is the makings of an assessment here, and recommend to the council that it be prioritized. The second thing I wanted to say was going back to the --

DR. BARBIERI: Steve, I'm sorry for interrupting, but I think that what Amy was suggesting is that we actually use the existing assessment. No?

DR. SCHUELLER: No, I guess I was thinking -- Steve has mentioned a couple of things that could be fixed, maybe the initialization and fixing some of the fits. Can the SSC not do that in conjunction with the Science Center and work as a group to push that forward and fix some of

those things that have been deemed to be problematic and then move forward and use that as an assessment?

DR. BARBIERI: Not the way I understand it. Weigh in, Mike E. and --

DR. ERRIGO: The SSC can certainly suggest or recommend that the assessment panel get back together and try to fix the problems and that be done as soon as possible, and that was actually one of the suggestions at the review panel. I don't know about the SSC itself getting together with the Science Center, but you could certainly say let's fix it, let's get a workshop together, as soon as possible. There's very little that needs to be done, perhaps, to get it going, and, therefore, we might be able to get a working model relatively quickly, and so that is one possibility you could take.

DR. BARBIERI: Steve, I interrupted you, in trying to understand. I guess I had misunderstood what Amy had --

DR. CADRIN: All right, and so I want to get back to the default basis for the ABC, which was Fred's initial question, and it was based on ORCS, and I think the information from the review panel -- As we just said, the survey has been stable, the age structure has been stable. It's that the current catches are not depleting the stock. Looking at the average from the series, it's slightly greater than the current ABC. I don't think there's any information in the assessment that suggests a depletion, and so if you did a depletion corrected average catch, you would essentially come up with the average catch. The current ABC is 626,000 and the average is 642,000, and so, again, I think the information in this assessment supports the continued use of this data-limited ORCS method.

DR. BARBIERI: You see, Amy, in that situation, because depletion corrected average catch -- I think it still is part of a tier in our ABC control rule, and so it's really not providing stock status determination and management advice using this quantitative assessment as a result of estimates that came out of this model. I mean we can use some of the data that was used in the assessment, but we would be applying another methodology in line with our ABC control rule.

DR. SERCHUK: Steve mostly made the points that I was concerned about. If there was any other information that was gleaned from going through the workshop exercise that indicated that either the age compositions have been stable or growing or a trend indicator showed that the stock was stable or increasing or decreasing, that's the sort of information I think that one might want to bring and modify it, and so I think the SSC has some latitude on that, if there's something that was compelling that came out from the indicators that might have been used in considering the model.

The model was not accepted. It was a benchmark, as I understand it. It was a new development. Typically, when a benchmark is rejected, at least I don't know what the situation is, but you revert to a status quo approach, and we've talked about ORCS here. My feeling is once a benchmark is rejected that you need another benchmark. You need another thing. I am not convinced that you can go and fix it and have the SSC come back. I would not want to do that, quite frankly. I think you would want to have some other deficiencies that have been pointed out here, particularly when you have a change in natural mortality -- The whole landscape could change after that.

I can tell you that, in the Northeast, if you're going to change natural mortality, that is not an operational update. That is not a standard assessment, because that changes the perspective of the

stock very much. Now, you're not bound by those rules, but what I'm suggesting is when you reject a benchmark, I think it has to go through the same set of exercises again, with addressing the recommendations that came out of the workshop, before we can accept the model. That would be my view on it. Others may disagree, but I feel very strongly about it. You put a lot of effort into this, and the model was not accepted. I don't think we should say, okay, then we can just tinker with what needs to be done as an SSC. I don't think that's fair to the process.

DR. BARBIERI: Just to clarify, I think that this conversation is getting a little confusing, because we are confusing, a little bit, different things. One thing is we have a benchmark assessment that was not considered the best scientific information available by the review panel. It was rejected, and, actually, it was producing outputs that, in my opinion, and I wasn't on the assessment panel, I felt were not credible.

To be showing that at this point, or I guess the terminal year is 2014, that this stock was at 90 percent of virgin biomass, I don't believe that's a plausible outcome, and so I think that there are some fairly critical issues, in terms of the parameter inputs for this model. There are issues with the data that need to be fully explored. I am very sorry that Marcel is not here, because he had brought up also some issues with the aging that I think need to be further explored, and so this is one thing, us commenting on the review and our recommendations as a result of this review.

Another thing is make a recommendation on how to handle the next assessment, type of assessment and timing. Do we go back to square one and do we hold another data, assessment, and review workshop, or do we recommend to the SEDAR review panel that perhaps the data workshop doesn't need to be repeated and that we just go to an assessment and then review? Those are the things that I think the council and the SEDAR Steering Committee would appreciate to hear from us.

Then catch level recommendations for gray triggerfish is a separate matter that we're going to have to discuss as one of the action items in our agenda, and so there is our evaluation of the review is a topic, recommendations on the way forward for the next assessment is a topic, and then catch level recommendations and stock status determination for gray trigger is a separate one. I just wanted to break those down as three separate things that we're going to have to handle.

MR. CARMICHAEL: With regard to SEDAR and the rejection, it's very similar to, as Fred described, in the Northeast, and, given that the review panel rejected this, our request to them, and it's in the instructions to them, is how should this proceed? What should be done next time?

In this case, it says that additional modeling work is required, and so, from a SEDAR perspective, that at least says go back to additional assessment workshop effort, which, of course, raises the question of timing, and if you went back another year or so, you would have more data, and you would probably want to do a whole other data workshop. We have had cases where they've identified modeling issues and data issues, and so start this thing over again at step one. This is a little more gray there, because the data issues aren't discussed so much with regard to that. They say additional modeling work, and so you could read into that that perhaps an assessment workshop convened to deal with that.

There are some -- I think there's been a case or two where they gave some very specific things to either be corrected or fixed or something, work to be done that couldn't be handled at that

workshop, and then said have this reviewed by the SSC. I think if they felt it was okay for the SSC to review it that that would have been explicitly stated. Since that wasn't explicitly stated, I wouldn't be comfortable having just the SSC weigh in and attempt to do that work.

DR. BARBIERI: Thank you for that, John.

DR. CADRIN: Again, I'm just trying to repeat what happened at the review workshop, is that the panel also felt that a lot of this investment can just be built upon, and that's when we suggested something similar, is that you don't need to start from square one. You don't have to have a data workshop, really, and you can start with an assessment workshop, but I thought it was interesting that the assessment team -- Actually, Kevin Craig, the lead analyst, felt that the data weighting, which Luiz showed, actually involves some expert judgment on the content of the data sources or things like the effect of Hurricane Hugo on that first year of the survey, which are really data issues.

Kevin felt that some data discussion and decisions are needed and that we can't just start at the assessment workshop, that there are some data considerations that are needed. I think that doesn't have to be a full-blown data workshop, but those two things, what are the relative information contents and the composition data, the catch and the survey, and how to deal with that first year of the survey.

MR. CARMICHAEL: I think SEDAR could accommodate something like that, sort of a hybrid, where you deal with a few data issues and bring in the right data folks for that, which is overall more of an assessment workshop.

DR. SERCHUK: My feeling, from what I heard -- I mean I agree with everything that's been said, but my feeling, from what I heard, is there was no indication that this stock was in trouble or that somehow it needed immediate action, based upon what the catches were and what the age compositions were.

My feeling, personally, is it's a priority. It's not the highest priority, because the indicators that I'm getting out from the discussion, from the people that were there, said we think where we are is an acceptable level for the indicators that we have. To me, that suggests that, sure, if you can get things done quickly and it takes things to build upon a workshop, fine, but it's not as if we need to go out and have this next year because we have no idea what the status of the stock is in relationship to the removals. If that's a fair summary, then I would say try to slot it into the schedule where it's appropriate and it doesn't bump other higher-priority initiatives.

DR. BARBIERI: By the way, again, this is the type of discussion that I think that the council and the SEDAR Steering Committee are expecting from us, is for us to weigh in and provide some recommendations, give them some direction, some guidance, on how to handle this. The sense of urgency, the type of assessment, the timing, all of those things I think would be very helpful to the council and to the SEDAR Steering Committee.

DR. BUCKEL: The quotas are being met in this fishery, and so it's maybe not a high priority to folks at the table, but the folks in the fishery, when the quota is met and they have to get the hooks out of the water -- If there is underfishing going on and there's more fish that could be taken, it would be good to get that information sooner than later.

DR. BARBIERI: Very good point. Thank you, Jeff. I am going here through our list of action items. We have already, informally I guess, discussed several points that have to do with those bullets, and so, similar to the discussion that we had yesterday, regarding red snapper and -- Well, red snapper. In that case, the golden tilefish was not CIE reviewed, but, in this case here, we have an opportunity to weigh in regarding the assessment review. We have those same bullets there that we had yesterday for red snapper.

Several of those are kind of self-explanatory, because they're already explicit in the review report, and I can summarize the discussion that we had here to identify uncertainties -- I think we have come down to provide fishing level recommendations, apply the ABC Control Rule, and complete the fishing level recommendations table, and then some bullets and discussion on advice on monitoring the stock and to the next assessment and research recommendations and guidance on the next assessment, all topics that we have been discussing, but if we can get all of those better described under those bullets, that would be a big help in putting together our report. Going to provide fishing level recommendations, one option is we keep status quo or something other.

DR. CADRIN: I would support that. I would suggest status quo as an interim basis for ABC, until an analytical stock assessment can be developed.

DR. BARBIERI: I am seeing a lot of nodding heads around the table.

DR. BOREMAN: I agree. I would just modify that recommendation to status quo until we get another stock assessment or evidence becomes available that suggests the stock is not performing as anticipated, or some language like that.

DR. BARBIERI: Right, and I think, John, this is very much in line with the point that Jeff had brought up earlier. As we have our annual review of the ACLs and the performance of the recommendations that we've made in the past and how they are being realized in the fishery landings, that allows us to keep an eye on where things are. Chip, you captured some of those points there. Provide advice on monitoring the stock until the next assessment, I think we have the indicators and metrics in place, but it would be good if you have any points or suggestions to make there, even if it is just to reinforce and articulate what we have in place. I think it would be helpful, because the council will have questions on this, I believe.

Right now, we have gray triggerfish being monitored by the SERFS or SEFIS, and so we have a fishery-independent -- We have landings monitoring taking place, and we have, within our processes, already requested to see those landings at every one of our -- I mean we ask annually to see those, perhaps twice annually, in April and in October, just to keep our finger on the pulse of how the landings are doing relative to the catch level recommendations that we have in place. I guess the next is research recommendations and guidance on the next assessment. Chip, I don't know how much you had captured from before, because the discussion was kind of --

MR. COLLIER: The one question that does come up is is there a trigger within these metrics or, they're more indicators right now, but is there a trigger that would cause you to say this become a high priority?

DR. CADRIN: I think, similar to what John said, things to keep an eye on are the survey trends. Do we see a depletion of the survey? In the other direction, do we see increased discards because

the ACL has been reached, an increase in biomass that the ABC is no longer appropriate for? So keep an eye on the surveys and the discards would be reasonable.

DR. BUCKEL: I think something that Fred brought up earlier, when we were comparing the ACLs versus the landings, are the landings meeting the ACLs, that percentage.

DR. SERCHUK: If you looked at the tables that we had for gray triggerfish, you realize that the ABCs, the ACLs in some cases, were being caught or being approached or being exceeded, and so it's not as simple as they're at 85 percent or 90 percent. In some cases, the closures had to be imposed to prevent overages, and, in some cases, the overages were more than just a little bit. I can understand people saying, well, wait a second, the reason that we're doing so well is that you're underestimating the productivity or there are more fish out there than we think there are, and I think, if we're going to use the table, we have to really make an interpretation of this, because I can see arguments coming back and saying not only were we able to catch it, but you imposed some things to reduce from being overages, and I see some cases here where the overage was 133 percent, 33 percent above.

On one hand, you would be concerned if you saw some impacts from the stock, and I can see people coming back and saying, well, you just did a review and it looks like the age compositions are okay, and there is no real sign that things are being deleterious. Is it possible, quite frankly, that we ought to increase the status quo approach, in terms of -- Steve mentioned that it could be a little bit higher than it was. I don't know. I don't want to get into that, but my point is that we have a table that we've looked at. We ought to take advantage of it with respect to whatever approach we have, because we have some information on what happened in 2015, and we have some information on what happened in 2016. Is there anything in that table, in our interpretation, that would suggest that we depart from the way we've done it? That's all. I'm not saying I would or wouldn't, but I'm just saying we have a responsibility here to do that.

DR. BARBIERI: No, that's spot on, and, in looking actually at our action item bullets here, and Chip mentioned the trigger for actions or indicators for us on where we are. This is why we get that report twice a year, in our spring and fall meeting, is to keep an eye on how those things are going and make recommendations accordingly, and so I completely agree that that's the kind of thing that can and should be taken into account.

MR. HARTIG: I mean I've heard discussion around the table about the DCAC approach, and Fred has certainly brought forward that the ages are not in decline with the landings level we have now, and you have a lot more information before you now than you had when we did the ORCS, and I don't know where DCAC falls into your tier. Is it higher than ORCS or lower or is it part of that same tier?

DR. BARBIERI: I think it's higher.

MR. HARTIG: To me, any time you can move a species with the information you currently have from even an assessment that wasn't accepted, but when you have a chance to move a species into that, you should do that, from our perspective, from a management perspective, to get a more accurate picture of what can be actually landed. I would encourage you all to do that, if you can.

DR. BARBIERI: I don't know that, in terms of getting it done, application of the DCAC methodology, if this would be done -- We can make a request to the Center for the development of that that we can review at our October meeting, if that is -- Right?

MR. HARTIG: That would be fine. That would be a way to move forward.

MR. CARMICHAEL: You could, but will you? In other words, if I have to write a memo to the Science Center requesting that they do it, is that your request?

DR. BARBIERI: That's a question being posed to the committee. I mean, personally --

DR. GRIMES: Why don't we consult with the Center people that are here about how they would react to such a request?

DR. BARBIERI: Right.

DR. WILLIAMS: So exactly what is the proposal or the request?

DR. BARBIERI: The proposal is, and we've crossed this bridge before several times. We have here a situation where the qualitative assessment was not accepted to provide stock status and catch level recommendations. We have the ORCS as the status quo, but, because we had all the data summaries and we went through that whole assessment process, we actually have a whole lot more information now in place to perhaps move from ORCS, which I think is 4 or 5 in our ABC Control Rule, to something higher, which, in this case, would be DCAC. The committee is requesting, would like to request, the Center to conduct that analysis and provide us with the results for review at the nearest opportunity.

DR. WILLIAMS: Yes, it's a reasonable request. The only thing I always worry about these is that time is always against us with assessments, and so we're already into 2016, and the data we have in hand is only through 2014. Certainly there would be an obvious advantage to trying to add another year of data, but then adding another year of data potentially complicates things, and so that's the only thing that worries me about it. It seems like an easy enough request, but then, when we start to factor in whether we're going to add another year of data or not, it suddenly complicates it a little.

MR. HARTIG: I mean I would keep it specific to the assessment. I would keep it within the range of what was done in the assessment, so we don't have to take those into consideration.

DR. WILLIAMS: If that's the case, then we could do a rapid turnaround of that.

DR. BARBIERI: Thank you, Erik, because I think that this would be the best option, is to have this handled ASAP and then provide it that way. Thank you, and so we will be submitting a memo to the Center requesting that depletion corrected average catch estimates for gray triggerfish in the South Atlantic be produced and be brought to the committee for review and catch level recommendations, development of catch level recommendations.

One other thing that may still be hanging, as far as our action items, and I'm trying to make eye contact here with Chip to see how much he's been able to compile there or capture from the

discussion earlier regarding the next assessment timing. I mean I heard, from at least one member, that the recommendation would be that the next assessment for gray triggerfish be a benchmark assessment, and so that talks about type, assessment type. Then timing, again, none of this is written in stone. It's really us articulating to the council and to the SEDAR Steering Committee our input regarding the urgency or the need to have this addressed sooner or later. I have Alexei and then Jeff.

DR. SHAROV: I am a little bit puzzled, because, earlier, half an hour ago, we talked about the fact that even though the assessment has been rejected, but the panel itself admitted, and I will quote from the report: "At this point, the review panel concluded that there wasn't enough time left in the meeting to establish a base case for the gray triggerfish and the assessment panel needed to review the findings to date and work with the assessment team to develop a new base case."

That tells me, based on the issues that are reported, which prevented the review panel to accept the assessment, that had the panel had extra time that -- It appears, to me, that they just spent much more time on reviewing red snapper and, having some issues with this assessment, they were not able to thoroughly review and explore alternatives right on the spot and ended up rejecting it, which means, under more favorable conditions maybe, and I'm not saying that it would have been, but maybe they would have found a different outcome.

Steve earlier mentioned that it is important to build upon what we already have, and, to me, it seems the most efficient at this point to not drop the assessment as a whole, but rather have this workshop, which would be primarily assessment, but probably with some data consideration, be organized and let the assessment panel do the best job that they can. If they conclude in the end that the structure and behavior of the model is not robust and they don't feel like it would pass, then that would be the final conclusion.

Alternatively, if they would feel that they are able to defend their case, then maybe there is a path of an additional review. Like with the red drum, for example, we have the so-called desktop review, whatever the formal mechanisms are, but they would be the fastest, the easiest, because, otherwise, you drop it and you are looking at several years of additional data collection, and you will have to start from the beginning with the build-up of all the steps from the data workshop on. I think it's probably worth the effort to try to give this effort a second chance to be completed.

DR. BARBIERI: Jeff, before I go to you, just to get some input here on what Alexei just brought up. Basically, what we had discussed was that we would have not necessarily all the way back to square one and have a data workshop, but have a new assessment workshop that would involve discussion of some data issues and nothing would be lost. I mean all of these things that have been accomplished so far, all that data and the results of those runs and all those outputs, are available to this assessment panel to continue work.

If I understand you correctly, Alexei, if that doesn't produce something reasonable, then we will go to the -- That panel would make a recommendation that a new benchmark assessment be developed, perhaps involving all three steps. Fred, how do you feel about that, since you made the --

DR. SERCHUK: I don't feel good about it, if you want to know my candid opinion, and I've been involved with the SAW/SARC process, and we've had benchmark assessments rejected. What we

do is we take the recommendations that come out from the panel, from the peer review panel, and we go back and we try to address them, and then we come back and do another benchmark.

I'm thinking that, to do anything less, when we basically said the assessment was rejected and it was not the best scientific information available -- If you take a different route, I am concerned that the next time an assessment is rejected that we won't do what formally the process requires to be done, and so I get the feeling that it wasn't that we're 90 percent of the way there. There are some things here, in the beginning of the report, about natural mortality and things about aging. There are things about the MRIP program. There's a whole list of things here that the panel has informed us need attention.

Many of those sort of things actually require significant peer review after you're done, and so I'm thinking, if that's the sort of suggestions we've been provided, that maybe they can be addressed in a short amount of time and it won't take three years to develop this sort of stuff, but I also think you've got to go back to the peer review process, the external peer review process, because you're going to be developing a new model.

The model that we have doesn't exist, and so that's why I certainly want to build upon what's available to be built upon. If it's going to take an analyst a short amount of time to do it, that's fine, but there are a number of things here that's going to require some work. To me, it suggests that, rather than coming back through an abbreviated process and coming back to the SSC for a review, I would prefer that it go through the sort of normal process for a benchmark. Thank you.

MR. CARMICHAEL: I had similar thoughts along the lines of Fred's, to say that if -- Obviously there is time savings if you felt a group could get together and work through this rapidly. The more time that passes, the more data change, and you're going to have to go back, eventually, and have to bring in new data. If this were something that could be done in the next six to nine months, then there may be some value, but the question I was going to bring up was similar to that.

Then the review panel, by saying go back to the assessment stage and not addressing review, leaves it open, and it sort of implies to SEDAR that an additional peer review panel to look at this. We've had cases where it's do A, B, and C and send it to us as a review panel in an email and we'll review it, thumbs up or thumbs down, or do A, B, and C and let the SSC review it, and we'll make it clear this is what we hoped that A, B, and C would do.

Here, it is kind of open for review, and so I think that raises the question of you guys might say, yes, you're okay asking the Center if can you do the work in the next six months and can we get this existing panel together, assessment panel, via some webinars and do this work, and then do you, as an SSC, feel comfortable reviewing that body, knowing that that may require, obviously, an extra day or something of a meeting, at minimum?

If you were comfortable, then we could probably find a way forward to follow that path, but if you're not comfortable taking that review responsibility and that effort, then yes, I think, as Fred has said, you're probably going to end up in a situation where you're really going pretty far back, and, with the many issues that are in there, that's probably not a bad idea.

DR. BARBIERI: Thank you, John. I have Jeff and Scott.

DR. BUCKEL: My points were raised already by you and Alexei.

DR. CROSSON: At the Science Center, as an economist, if I do some work and I submit it to a journal for external peer review and it gets rejected and it doesn't get published, therefore, a lot of times I'm not necessarily going to ditch the work. I might try and get it out in the gray literature as a tech memo. Is that going through the same type of peer review that it would have it had gone though an external review? No. Is there still some information that may have value to people that's better than not having anything out there at all, for all the work? Yes. People that are looking at that -- Of course, in the gray literature, you look at it with a jaundiced eye, because it's not gone through the same rigorous review, but it's still valuable information.

When I look at this process, it seems to me that there's enough that could be salvaged right here, that we can get something better than just, again, sticking with the ORCS approach or something like that, and so I would hate to see all the work that's gone into this just ignored and starting from scratch, because I think there is a lot of value in there. In fact, again, I understand what happened with the external peer review panel. I don't know if it's possible for them to look at it again or it's something that's going to have to be a subcommittee of the SSC or it's going to be a DCAC approach, but it seems like there is something here that can be done. I don't see the need to start from scratch again.

MR. CARMICHAEL: That existing panel no longer exists, and so it would be a new panel. The question does come up of how would you handle the peer review of this, whatever gets done and not knowing what the changes may look like, or if the group can actually resolve the issues. Maybe, with more time, we could, but that's always an uncertainty. You don't know what else pops up, that they decide they can't, and then that becomes a wasted effort, and so there's risk there as well.

DR. SERCHUK: I understand where Scott is coming from, but there's a huge stamp on this review. It does not represent the best scientific information available. That means you can't shop it off to a gray literature or another journal someplace and pass it off as the best scientific information available, and that's the issue, and we have to take that very seriously.

DR. CROSSON: I would never take something that had been rejected by a journal and send it to another journal. I just recognize the flaws in my work and act accordingly. I am not arguing that this is any way, shape, or form, in its current existence, is best scientific information available. It's been rejected by the panel, correctly, but I'm saying additional work can be done to salvage something out of it. I think that's a different statement.

DR. BARBIERI: Also, in terms of the process here, also understand the points that Erik made earlier. I mean they have also their calendar of activities, their assignments to staff, and to insert -- I mean it's different to do a DCAC analysis than to say, okay, now we're going to try to kind of put this into their schedule. It has to be greatly disruptive to the processes that they have in place already, and so I am kind of leaning towards a benchmark myself, the more I hear about all the issues that are being brought up, and let the record show that Vice Chairman Reichert has returned and looks like he is feeling better.

DR. ERRIGO: It sounds like people are thinking that if we go back to a regular benchmark stage and kind of just toss this in the garbage and start all over again, but this has already been through

two data workshops already. The next time, it would probably go much faster, especially through the data workshop phase. We will still use all the information that was gathered and done at each of the preexisting data workshops and even the assessment work, and so we can still build on what was done. We wouldn't exactly be tossing everything out the window.

DR. SHAROV: I will just say that I would accept whatever the group will agree to, but I just have to note that I've seen much worse assessments being accepted, people going to the great extent of doubling and tripling natural mortality to account for terrible retrospective bias, and the work would be accepted, and then another benchmark would later say this was a bad idea.

In this case, based on the document, yes, there are some issues, but the panel admitted that they just didn't have time to thoroughly review this, and so they're saying, sorry, but we just didn't have time to actually give it the full review. Yes, certainly this was the best attempt to be objective, but we know that a panel of few people is -- The objectivity is the sum of the subjective opinions, and so that's only reasons for the considerations that force me to suggest to give it another chance, but, as I said, whatever the committee decides, I will agree to.

DR. BARBIERI: Thank you, Alexei, and, Alexei, I think we are actually saying the same thing here. It's just a matter of how we're interpreting it. Basically, what we are calling benchmark is not that, as Mike E. brought up, that this would be tossed or have to go back, all the way back, to having a brand-new data workshop now. I mean what we are saying is that there will be a new assessment panel that's going to have to be convened that will be working starting -- Right?

MR. CARMICHAEL: Procedurally, I sort of see you have two approaches. You can kind of take the suggestion of the review panel, this kind of gray suggestion, and I would think that would mean you could take the assessment panel that existed before and reconvene them and let them put the extra time into it to get this to the next stage, in which case then we raise the question of peer review. I think it would need a peer review, because you have a peer review that says it's best science, and it's a benchmark. To me, that means a peer review panel, CIE. We could do that as a desk review, perhaps, for efficiency, but I think it's either a panel or a desk review.

Your other choice is that you say, okay, this needs to -- There's a myriad of issues, and there's a lot to be done and we're not comfortable with that approach, and we don't think we can get that done timely enough that it's not going to raise a bunch of data problems, where it's going to be way out of date, in which case it goes in the queue for the next available time which it can be brought in as a benchmark.

There is the quick option, which maybe you could go back to the AW and try to salvage from there, or it goes into the queue, and that's sort of what it comes down to, and that's kind of where I felt like we were earlier, and so now I think we're at the point of which of these recommendations does this group put forth as their preferred that we take to the Steering Committee on Monday and run through the council in June and probably work into the schedule of Steering Committee stuff come October.

DR. BARBIERI: Thanks for that, John.

DR. BUCKEL: I think John laid out the two options very well.

DR. BELCHER: Laura and I were just talking too that -- As the idea of the approach of the DCAC, you may not be given the discussions and uncertainty of natural mortality, knowing that that's part of your inputs. Obviously that's going to lead to a little bit more discussion point than just saying, well, we run with what's in the model and go forward, and so this might actually put you back towards where John was suggesting.

DR. BARBIERI: Excellent points, because, yes, in this case, let's go back and erase that recommendation. I mean I personally feel that that estimate of natural mortality needs to be completely revisited. It's something that, to me, did not produce a credible estimate, and now that Vice Chairman Reichert is here, I am going to ask him to weigh in a little bit on the aging issue that we have discussed.

DR. REICHERT: Unfortunately, I just joined, and so I'm not sure what the discussion has been, and so I would kind of like to catch up a little here. I mean the aging of gray triggerfish, using spines, is very uncertain. We have had, I believe, two workshops to discuss this. There are some validation studies underway or starting. They're in the third year, and so all I can say is there's a lot of uncertainty, and, in the earlier stage, it was discussed whether or not these age estimates should be used, but that's been extensively discussed in both data workshops, and I think, until we have that clear validation, those uncertainties will remain.

DR. BUCKEL: Back to John Carmichael had laid out two options, and I think the decision on those two options -- I would like to hear from -- I think the analysts that were involved with the gray triggerfish assessment and the key data providers need to really make the call and weigh in on that, either here or later, because they're the ones that are going to know if they feel comfortable with the review panel's recommendation to kind of pick up at an assessment workshop or hybrid data/assessment workshop and then go to some desk or CIE review, but they're the ones that would be best able to make the call on that.

DR. SERCHUK: Here is my recommendation for the SSC to consider. I think that it's not appropriate to reconvene the existing panel. I do think it's appropriate to see whether one of the panel members would be willing to serve on a new panel, to get that institutional memory. I think, because there is some unresolved issues, that it needs an in-depth CIE peer-reviewed look at it, but I think it would be helpful if we could get at least one of the three members to participate again, if possible. That would bring institutional memory in, and it would also have some new eyes looking at some new data, because there are going to be new data in terms of natural mortality and something else, and so, if that's possible, that would be my suggestion.

DR. BARBIERI: Thank you, Fred, and so let's just see. Does anybody disagree with this recommendation? No? Are we will in full agreement that this is going to be our recommendation going into the report?

DR. SHAROV: Can you state what is the recommendation?

DR. BARBIERI: The recommendation is that this goes forward as a benchmark assessment, but it's starting from a new assessment panel being put together with one of the previous members, one member of the previous assessment panel being part of it, to help bring that institutional memory there and help highlight some of the issues that have come up and help facilitate that

connectivity between the old and the new panel, and then an assessment model be developed from that, and that goes then to the CIE for review and then comes back here.

MR. CARMICHAEL: A new assessment panel with overlap of the old one, and then a new review panel, with overlap of the old one. If we want a CIE overlap, we will request that, and that's certainly within our rights to request, and so we can handle that. I guess a question would become timing. If this doesn't happen by some date, does the passing of time and the new data points negate this approach, or an outcome of this aging validation?

MS. LANGE: Luiz, I don't think what you just stated addresses whether we're talking about -- Which of the two options that John presented we're talking about. Are we talking about getting back into the queue with the new benchmark down the road, or are we looking at a really short time? I thought there were issues relative to the aging and everything else that would preclude being able to do just a short time, and I think it would be more efficient, if that's the case, to wait until some of those issues are resolved and put it back in the queue for a full benchmark down the road.

DR. SCHUELLER: That's pretty much what I was going to say, was why -- I mean you hate to throw it away, but if there's an age validation study that's going on and they're in their third year and we wait a couple of years to get that data, and the M is going to be reconsidered, those are two major data workshop issues, and I don't see how we can ignore that.

DR. BARBIERI: Yes, and I see a lot of heads nodding, but, since this is our consensus report, now what I'm hearing as a revised recommendation, and this is fine, because we are discussing and reiterating, is that we go just to a full benchmark. We recommend that we start over, basically. This goes back into the queue and we start over. That would allow just new data, new estimates. It's that serious.

DR. GRIMES: I am going to Marcel. This thing was put off one time because the aging was inadequate, and so you're saying that's never been resolved, because I thought, when it went forward to be assessed this time and go through the SEDAR process, that that had been improved, but you're saying that there's still issues. If that's the case and there are the other issues, as brought up earlier, then I agree that it needs to go back for an assessment and a review.

DR. REICHERT: I think, because we don't have that validation, that the age issues are still unresolved. The aging was done by the Science Center, and remind me if I'm wrong, Erik, because the analysis showed, the second time around, that the internal variability within the Science Center was lower than the internal variability within the South Carolina DNR lab. However, at that point, we did recognize that that does not mean that we are closer to the relationship between the results of the aging and the true age of the fish. That remained an issue until the validation study was resolved, but, at that point, collectively, and that was after the second age workshop, that was a decision that was made at that point.

Prior to that, the collective age labs looked at the results and recommended that ages perhaps should not be used, because of the high variability and uncertainty of those ages. I believe that that was what happened, but I would need to go back and look at my workshop notes, because the first workshop was actually several years back. That was prior to the first gray triggerfish data workshop.

MR. CARMICHAEL: When will the age validation study be done and the information be available that a data workshop could consider it and decide what it means?

DR. WILLIAMS: As Amy correctly identified, we're in the third year of a three-year study. We have some fish then that will have been marked and in captivity for almost three full years by the end of this year, and so then time to work that up, and I would say we could have results within two years, I mean certainly -- Actually, probably six months after the end of this year. When I say the end of this year, I really mean probably next summer would be when we would start to cull those fish, and then the end of 2017 might be the earliest.

MR. CARMICHAEL: So end of 2017 when they would be getting close to having it done. To me, that sounds sort of like a 2018 timing for this assessment, where you go in and you would be adding three more years of data and doing a whole benchmark.

DR. REICHERT: That is true. However, if the validation study requires us to re-age every single spine, that takes a considerable amount of time, especially if the Science Center lab is going to do all the aging again. I mean if we can spread that wealth over the various labs, then we may do it earlier, but the entire time series, that is a significant amount of spines. I would keep that in the back of our minds if we are looking at scheduling.

MR. CARMICHAEL: That's in the front of my mind, and so is that six months, nine months? Saying that you had aging study results in early 2017 and it said you had to -- I want to play worst-case here. It said you had to re-age everything. Then at what point would you have ages to go into a data workshop?

DR. REICHERT: I do not have the number of spines in the back of my mind. I would argue that, given there is other assessments going on at the same time, and obviously, both for the Science Center as well for us, resources are a limiting factor, I don't know. I would need to look at that and see how long that may take, but if the entire series needs to be re-aged, that's a huge amount of work, and I would say six months is the minimum amount of time that it may take to do this, but it may take considerably more.

MR. CARMICHAEL: So I think we would have to tell the Steering Committee this would be roughly a 2019 data workshop, at the earliest, barring any other unforeseen circumstances. Does that make you guys reconsider M and DCAC?

DR. REICHERT: This may be something that was discussed earlier, but a length-based assessment is out of the question? Again, you guys may have gone through this before I arrived.

DR. BARBIERI: No, we haven't, but those are the things, in my opinion, that are discussed at a data and an assessment workshop, and so that would be handled that way.

DR. CROSSON: We've been knocking this around for an hour now, and the further we knock it around, the more uncertainty we get, and I think it's pretty clear at this point to put it back in the queue for SEDAR and deal with it then, because we have a lot of other long items on the agenda that we need to get through.

MR. CARMICHAEL: Amen. I was going to say, Luiz, 2019 for a request, we will ask. We will maintain the other things that there is overlap from the previous AW and overlap from the previous RW, when the time comes around, and I think that pulls all the different things together that have been discussed.

DR. BARBIERI: I do not, personally, want to reconsider having a DCAC with this M. I just don't feel it would stick.

MR. HARTIG: To the end discussion, I would encourage you to have this discussion sooner than later. I mean this is going to come up in the next assessments that we're doing. This is going to be a way that M is derived from now on from the Center, and so I would have this discussion sooner than later, so we can --

DR. BARBIERI: Right, and the discussions that we had offline, in planning, we kind of thought about having this for the October agenda, as an item then. Okay, folks. This completes this very long-winded, but, in my opinion, very productive, because a lot of things that had to come up ended up coming up, and I think we have a much better idea now of where we are. Looking at the agenda, we just covered two items in our agenda for the last couple of hours. The next item will be the Hogfish Decision Tool Review, but, considering the time that we've been here, let's have a ten-minute break and then reconvene at 10:20.

DR. BARBIERI: The next item on our agenda is the Hogfish Decision Tool Review. This is Item Number 17 in our overview document. We received a number of attachments, Attachment 22 through 26, but a lot of information regarding this decision tool, and we have Dr. Nick Farmer here from SERO, and I believe we have David Records on the webinar for this presentation and discussion.

There are a number of action items that are being addressed as part of this item. Discuss the uncertainties associated with this decision tool. There are a number of sub-bullets there. We're going to go into all of them in more detail after this presentation, and then determine whether this tool used the best scientific information available and are appropriate for use in managing South Atlantic fisheries. Nick and David, if you guys are ready.

DR. FARMER: Thanks, Mr. Chair. Just to give you a brief background on Snapper Grouper Amendment 37, FWC completed SEDAR 37, a hogfish benchmark stock assessment, and one of the major findings of that assessment was that there are three genetically distinct stocks for hogfish. There is one on the West Florida Shelf, there is one that basically starts in the Florida Keys and extends up the east coast of Florida, and then there's an additional stock, ranging from Georgia through North Carolina.

The assessment's advice, in terms of management benchmarks and stock status, was accepted in the Gulf of Mexico for the West Florida stock and in South Atlantic for the Florida Keys and East Florida stock, but not for the Georgia through North Carolina stock. Then you guys have had several subsequent discussions, which you probably recall, that set the ABC for the Georgia through North Carolina stock in numbers of fish, based on the ORCS approach, and set the ABC in numbers of fish for the Florida Keys and East Florida stock, based on some innovative approaches developed by the South Atlantic staff.

This is basically carrying forward on those. Snapper Grouper Amendment 37 has, I believe, twelve actions in it, aligned with rearranging stock boundary definitions and then implementing different sorts of management regulations. The goal, from the Regional Office, was to provide tools to the council, and that's me contributing to the overfishing of Florida Keys hogfish. I'm sorry, but I did it.

Provide some tools to the council that will look at the combined effects of different seasonal closure options, size limits, and bag limits for the Florida Keys/East Florida stock and the Georgia/North Carolina hogfish stocks, and then, based on input from the Southeast Fisheries Science Center, from whom we received a full and comprehensive scientific review and addressed the comments, we also wanted to look at the impacts of effort shifting and uncertainty.

The data source for this was a modified recreational ACL dataset. These are datasets prepared by the Science Center from the MRIP and Southeast Region Headboat Survey data that undergoes some additional QA/QC. I am not going to belabor the way that we assigned average weights for this dataset, because, subsequently, the decision was made to manage hogfish in numbers, and so it's not all that critical, and so I'm just going to skip right past this one.

Basically, the long story short of that is that dataset allowed us to partition the hogfish stock into those three genetic subregions. Then the next step was to look at the impacts of seasonal closures. Basically, the model works off of daily catch rates. Those daily catch rates are put together from MRIP landings by mode and headboat landings, and basically they're based on wave-specific data from the most recent three years, and we assume that landings are uniformly distributed within waves for the MRIP data and uniformly distributed within months for the headboat data, and so the recreational decision tool, or RDT models, allow a user-specified number of days closed for each month, and we have effort shift scalars that we built in there that basically redistribute days as a proxy for increased effort, before and after closures.

The way that works is if your catch rates were even across all months and your number of days were even across all months, then if you closed say one of your thirty-day months with a hundred-fish-per-day catch rate and you selected a 100 percent effort shift scalar, the catch rate would be redistributed, in terms of the number of days fished outside of those months, so that those 100 fish per day times thirty days would be replaced or caught outside of the closed period, and so it allows for a percent compensation for lost fishing days due to seasonal closures, while preserving differences in daily catch rates between months, and so, for example, if you close, in the actual tool, a month with a high catch rate, then, even if you select a 100 percent effort shift, if the catch rate is lower in the months outside of that, you won't be able to perfectly offset the number of landings, although you will offset the number of days of fishing effort, based on the scalars that we developed.

Another thing that we wanted to look at was size limits. We've got a graph here, which is Figure 3 in the associated report, and what you're looking at is on the top is the Florida Keys/East Florida stock. On the bottom is the Georgia through North Carolina stock. This is the number of samples for fork lengths of landed hogfish reported by the headboat survey from the three most recent years we had for that, which was 2011 through 2013, and those are in red, and then from MRIP, which was 2012 through 2014, and those are in blue.

It should be immediately apparent that there is a big difference in terms of the size distribution of landed fish between those two regions, and it should also be pretty apparent that any size limit in the Florida Keys/East Florida area is going to be probably a lot more effective and get you a lot bigger reduction than in the Georgia through North Carolina area, and also that the Georgia through North Carolina area is not nearly as well sampled.

What we did to simulate size limit impacts is we used the catch effort files from the MRIP survey and from the headboat survey, and we removed oversized fish at different size limits and then recomputed landings, and then we compared the recomputed landings to the baseline landings and came up with basically scalars by wave, or by month, depending on the survey, as to what percentage of landings would be removed by that size limit. Then we pooled data across waves, to ensure that a sample size was achieved of greater than thirty, because you will have some waves -- For example for MRIP charter mode, you might have a wave where maybe only two fish were sampled over that three-year period, and so you pool across, to make sure that you're not getting an artificially high or low scalar. For example, if you only had one sample and it was a twelve-inch fish, then it would say that a thirteen-inch size limit would eliminate all harvest in the wave, and that might not be a realistic assumption, and so we pool across waves, to ensure that we're not creating a bias.

DR. SERCHUK: Your figures here talk about lengths of the landed hogfish, but I'm just wondering whether it also includes those hogfish that were captured, but returned, because they were released alive. It has something to do with the size limits, obviously.

DR. FARMER: Yes, and so the size limits that we're looking at are all increasing size limits, and so the assumption would be, and, granted, there are some issues, because there is also a bag limit in place, at least in some of the regions, of five fish. The assumption is that, as you increase the size limit, you're not going to have any discarded fish that were bigger than the size limit that exists now, which is twelve inches. Now, there could be some bag limit discards, but we wouldn't have any measurements for those fish.

DR. SERCHUK: My point was really to get at the bottom figure here. You said they were very much different in terms of the size limits, but it could be that there's a lot of discarding of small fish going on in the Georgia/North Carolina region that doesn't show up in the landed portion of the catch.

DR. FARMER: With regards to high-grading, I don't think that there is currently a bag limit in that region, and, also, just based on a lot of the sampling that we've seen, it just seems like the fish, in general, that are selected up there are bigger, and you will see that in the discussion. I get into what the size at transition from female to male is, and I guess I probably should have also familiarized you guys with the biology of the hogfish stock, because I know some of you are from other regions and might not be all that familiar with hogfish.

Basically, these are a wrasse, and they're highly vulnerable to spearfishing, because they're, quite frankly, just not very smart. They don't really run away from spear guns. They tend to turn to look at you when you're chasing them with a spear gun, and so they're pretty easy to catch that way. There are some ways to catch them on hook and line. That's a little bit harder. Also, they are hermaphroditic and they form harems, and so you have a dominant male, basically, with a

harem of females, and so they have kind of a unique breeding strategy that make them, I think, a little bit more vulnerable to exploitation.

MR. HARTIG: That is predominantly a spearfish fishery. Most of the catch is caught by divers.

DR. FARMER: Bag limits were another thing that the council was considering. We used the mean of the last three years to simulate bag limit impacts. It's a very similar analysis to the size limit, and we had presented this to you guys at a previous SSC meeting, our bag limit methodology, but, basically, you remove fish over the bag limit. You basically compute the catch per angler on the trip, and then you remove fish from those catch files if it exceeds the simulated catch per angler trip that would be allowed, and you re-compute landings.

We also pooled this data across waves, to ensure an adequate sample size, and developed scalars. Then we wanted to look at the combined effects, and so we basically just roll all these percent scalars together, including the effort-shifting scalar, and then another thing that we provided, although the management targets are ACLs in numbers, we wanted to provide the council a sense of what sort of removals their management actions would provide, and so we did also compute dead discards.

Basically, dead discards would be our baseline discards per mode and month times the new discards resulting from the new management measures, as impacted by release mortality, which is 10 percent for the hook and line fishery, and also the percentage of the landings that are hook and line. Basically, the assumption that we make is that there are no dead discards originating from the new management action, due to spearfishing. The assumption is that spearfishing perfectly selects the size limit and bag limit, recognizing there is a little weakness there, but, looking at SEDAR 37, only about 4 percent of the total discards are attributable to spear gear, despite that being the predominant gear harvesting hogfish.

In the Florida Keys/East Florida area, about 73 percent of the landings came from spear from 2010 through 2012, and Georgia through North Carolina, it's kind of a different story. Although the anecdotal information suggests that most of the landings probably come from spear trips, no spear trips were sampled by MRIP between 2010 and 2012. Basically, the way that this works in the model, is 27 percent of the new management discards are subjected to that 10 percent release mortality rate and converted into new projected dead discards and added to the original baseline dead discards per month and wave.

Then, for Georgia through North Carolina, 100 percent of the new management discards are hit with that 10 percent release mortality rate, and so, basically, because the release mortality rate is pretty low, the removals due to these new management changes aren't all that high in terms of adding to the initial baseline dead discards, and especially for the Florida Keys/East Florida model. You don't get a big change in terms of your total removals, because only 10 percent of 27 percent of the new discards, and so 2.7 percent, are actually becoming new dead discards.

The 10 percent release mortality rate is consistent with SEDAR 37, and you add the projected dead discards to the projected landings and get total removals, and then another thing that David Records added to this decision tool, because we've been using these decision tools a lot. We used them in the South Atlantic for red snapper back in the Amendment 17A days. We've used the in the Gulf several times for greater amberjack, gray triggerfish, gag grouper, and a few other species.

David added an economic effects add-on to this tool, and, basically, what it allows you to do is get immediate feedback as to the economic effects of the selected permutation of management measures. One of the things that we discussed is what would be the economic baseline, and so, prior to Snapper Grouper 37 being approved and implemented, hogfish in the South Atlantic would be managed as a single stock, from East Florida all the way up through North Carolina, and we wouldn't include landings from Monroe County into that baseline, and we would have an ACL of 85,355 pounds whole weight from MRIP.

The projected overage date, based on the same methods we used to project the landings in the decision tool, would have been April 26 for next year, had we continued with that management strategy, and then the Monroe County hogfish, managed in the Gulf, would have no anticipated overage, and so, for the Florida Keys/East Florida baseline, for economic effects, we're looking at the East Florida landings that we would have projected from January 1 through April 26 and their economic benefits, plus the Monroe County landings and their associated economic benefits for the entire calendar year. Then, for Georgia through North Carolina, you're looking at Georgia through North Carolina landings, and there are economic benefits from January 1 through April 26.

Then what the recreational decision tool does is it displays, to the user, the total change in consumer surplus relative to the status quo under any combination of the ACL, minimum size limit, bag limit, and seasonal closure alternatives, and that consumer surplus is the landings times the willingness to pay, and the willingness to pay is -- We use snapper as the nearest proxy. In a lot of the informational posters, say from the State of Florida, hogfish is -- One of their common names is hog snapper, and so we thought snapper was probably a better proxy than grouper, from which we also had a willingness to pay value, and that was used as the best proxy for willingness to pay for hogfish.

Then another comment we received from the Southeast Fisheries Science Center was to do a better job of accounting for uncertainty in the runs, and so what we did is we projected the uncertainty in the seasonal closure dates and landings across 1,000 bootstrap runs for each user-selected model configuration, and, basically, those bootstrap runs accounted for uncertainty in the projections data by taking each data point that went into the projection -- The projection was really simple, because hogfish didn't seem to have an interannual trend. The seasonal trend wasn't all that compelling either.

It's really noisy data. Basically, you could have a wave that resulted in a quota closure in one year and there would be no landings, hardly at all, in that wave in the next year, and then the following wave would have huge landings, and so it was really noisy, and so what we did to try to account for that was we just averaged straight across 2012 through 2015, and so this input data takes into account the noise in that landings by mode and wave, with the associated PSEs, drawing from a normal distribution, fit to the mean and standard deviations from those surveys. Then it averages those to create the projected landings, then it also hits those with the size limit and bag limit reductions and seasonal limit reductions.

The size limit and bag limit reductions have their own associated uncertainty, because they're an average across the three most recent years, and so we use the mean and standard deviation of those scalars as well, and so we tried to account for all the internal to model noise in those uncertainty runs, and so let's look at some results.

The first thing I wanted to show you is how noisy the projected landings are. These are mean landings by month for the Florida Keys and East Florida stock, up on the top, and then for the Georgia through North Carolina stock on the bottom. The first thing that should probably jump out at you, looking at that Y-axis, is that Georgia through North Carolina don't land a lot of fish compared to the Florida Keys/East Florida stock.

Then the second thing that should probably jump out at you is those 95 percent confidence interval error bars there, which I realized, as I was looking through this this morning, I probably should have just done that as a line, rather than an error bar, but you can see how noisy it is, in terms of just this sort three or four-year average. There's a lot of variability in those landings through time, in terms of the seasonal trend, but we use this as our input for projections.

Then, for size limits, these are the associated reductions, and so you've got reductions on the Y-axis. You've got month along the X-axis, and then you've got the size limits along the Z-axis, ranging from twelve at the front, and you see a zero percent reduction for twelve, because that's the status quo, ranging up to twenty in the back, and so you will notice, for headboat, that you've got not a lot of monthly variability. That's due to low sample size, and so we had to do a lot of pooling for headboat.

For charter, you will notice most if it is pretty stable, and that's because, again, we had to pool most of the months, in order to get that sample size of thirty. Private was better sampled for the Florida Keys/East Florida region, and so you get some of that monthly variability jumping in there, and the size limit reductions are -- Kind of as you would expect, the reduction gets larger as you move into bigger and bigger size limits.

Then, for Georgia through North Carolina, it's kind of a different story. For headboat, you really have to push it above a certain size to get a reduction, and, once you get there, basically the landings disappear entirely. For charter, it's kind of a similar story, and then, for private, the landings were all bigger than the twenty-inch size limit, for the most part, and so there is no associated reductions with the size limit.

Then, for bag limits, for headboat, because there's so many angers onboard, you really don't get any impacts in the Florida Keys/East Florida stock until you move to a one-fish per vessel bag limit, and then you get a pretty big impact from that. For charter, there is a little bit more of a buildup, and then, for private, the bag limits are more effective. In this graphic, again, it's percent reductions on the Y and month on the X, and then fish per angler trip on the Z-axis, ranging from five at the front down to one fish per vessel, actually, at the very back. Bag limit, for Georgia through North Carolina, not a lot of folks coming anywhere close to any of these bag limits, and so really the vessel limit is the only one that jumps out as any kind of real reduction. Then I wanted to quickly demonstrate the decision tools to you, so you can see how those work.

DR. BARBIERI: Just one second. We have one quick question.

DR. REICHERT: Nick, can you remind us, what's the current bag limit?

DR. FARMER: The current bag limit is five fish per angler, and I'm not sure that that applies everywhere. I've got it -- Myra might have that, but I think it's five fish per angler federally off of Florida, and that's the only place where it's officially codified, I think.

MR. COLLIER: The State of North Carolina also has a five-fish bag limit as well.

DR. FARMER: I think we've got those status quo regulations in the report, on the first or second page, and so, basically, the way the tools work is you've got this front page, and I will make it a little bit bigger, so you can see it, but your first input is you can select a seasonal closure. You can go in here and there's dropdown menus to pick the number of days you want closed in each month. You can go down and select a partial closure of July, for example, and it should auto-calculate.

This used to run a whole lot faster, and then I added the bootstrapping uncertainty stuff, and now it's a little bit slower, and so I'm going to keep my permutations of this to a minimum, so that I don't drag on too much time, but, basically, you have dropdown menus for the seasonal closure, you have a dropdown for effort shifting, and you can choose a range from zero to 100 percent. You can select a size limit, ranging from twelve inches all the way to up twenty inches. You can select a big limit, ranging from five fish per angler on the vessel, which is the status quo, to one fish per vessel.

Then what you're going to get as your output is a series of recreational landings by month, in numbers, dead discards in numbers by month, and then total removals by month, and these are for the different ACL alternatives, which are scalars off the ABC, and so it's 100 percent, 95 percent, and 90 percent of the ABC for Alternatives a, b, and c here.

You will get, obviously, a slightly shorter season under 2c than you would under 2a, because the ACL is slightly lower. Then, at the bottom, what you're going to get is the total ACL in pounds, the total ACL in numbers, the landings expected under the permutation of management alternatives that you've selected in numbers, and then the percent of the ACL that was landed.

I should also mention, in this time series, it will tell you when the fishery was closed due to a quota closure, and so you can see, with Alternative a, which is the 100 percent of the ABC, in this permutation you would close on the 31st of July. In this permutation, you would close on the 30th, and, in this one, you would close on the 28th of July. It gives you the total removals under each of those alternatives, the projected closure date, and then the number of open days in the season.

It gives you a couple of warnings about the weaknesses of the tool, and it also gives you this kind of dynamic cumulative landings time series here, and so, in this instance, this one had everything closed up until an opening date on the 24th of July. You can see how quickly the landings hit the ACL, and so it's a very short season under some of these permutations.

Other things that are in here is a repeat of the projected landings graph that I showed you earlier, and I should show you -- In the tool, it also gives you the economic effects, down on the bottom, and so it gives you the change from status quo consumer surplus, in 2014 dollars, for the combined full recreational fishery for ACL Alternatives a, b, and c, and you can see, for the Florida Keys/East Florida tool, all of these result in a major economic loss, and the reason for that is because the Gulf of Mexico portion of the stock, which is Monroe County, would have remained open if it had remained part of the Gulf stock.

Then, on the economics tab, you can see a breakdown of the consumer surplus by mode, if you really wanted that level of detail. There is a table that the document pulls from from the ACL alternatives developed by the South Atlantic staff and approved by the SSC, and then there is a

breakdown of the percent landings by gear from SEDAR 37, and so you can see how the removals are calculated.

That's the Florida Keys. The Georgia/North Carolina tool is coming up. This one is still calculating, and so I don't know. On my computer, it takes about maybe thirty-seconds for it re-compute the landings. I thought I would probably be able to go through some more alternatives and permutations, but I don't think that's really a good use of your time, but the Georgia through North Carolina tool looks very similar. It's basically the same type of inputs. It's just the ACL is lower and the projected landings are lower.

DR. BARBIERI: Nick, we have another question from Alexei.

DR. SHAROV: In the projected results, you are showing, I assume, point estimates of the projected harvest, but where are your uncertainty estimates coming in?

DR. FARMER: The uncertainty estimates are produced on a table that's hidden in this one, but it goes into the document, and so the analysts have provided that in the SEDAR 37 document. I'm about to present those results to you in a table also, but I don't have them on the front page interface of the tool, because I'm assuming that the council is probably going to manage to the point estimate. However, in the document, the uncertainty will be expressed to them for their preferred alternative and a variety of other alternatives as well.

DR. SHAROV: I was not interested much in the numbers, but I could guess that the variability is high. Certainly the projections will have a large confidence interval, but it's more like the question is how are we going to deal with the uncertainty? That tells you that the actual catch could be well beyond your midpoint estimate, and so how are we going to use that information in management? You said that the council is likely to use just the point estimates? That's a well-known approach. Unfortunately, it often leads to a serious crisis.

DR. FARMER: In the SEDAR 37 document, there are a series of tables that are either in there or will be added in the next permutation of the document that are going to show the combined effects, total outputs, for a variety of scenarios for both of the regions. You can see here these are the standard deviations from those bootstrap uncertainty estimates for open days and landings, and so what you're looking at here is the preferred alternative for the Florida Keys/East Florida region right now. It's a sixteen-inch size limit, one fish per angler bag limit. The projected closure date is the 31st of December, which means basically that we're not anticipating a quota closure with that short open season and these regulations, and so you would have a total of ninety-two open days. None of the bootstrap runs indicated a quota closure within that time period.

The landings are pretty well below the ACL in numbers under that run, with some variability associated, and then here is the removals anticipated, and then there is the change from the status quo consumer surplus, and so I've put in a variety of runs here, including a full open season at the status quo, and so a twelve-inch size limit, a five-fish per angler bag limit. You can see, under that run, we anticipate a closure date of the 7th of February.

There is not a lot of variability in that, and I think what happened, and one of the findings of the uncertainty runs, was, under the more draconian management approaches, or under the no new management approaches, where the season is extremely short or the management measures are

extremely tight, there is not a lot of uncertainty, because there's not a lot of time for that uncertainty to accrue, and so you end up getting very similar outputs for the bootstrap runs as you do from the mean run, whereas, under management measures which kind of split the difference and run the middle ground, you can get a higher level of uncertainty.

Under all the uncertainty runs, we're assuming a quota closure will happen at the appropriate time, and so it doesn't accrue additional landings beyond the data that the landings exceed that quota, which also controls a little bit of the uncertainty. That's for the Florida Keys/East Florida region.

Then for Georgia through North Carolina, you can see one of the big take-home messages here is the closure date. Under all these permutations, there is no closure. There weren't any scenarios that we looked at that looked like the Georgia through North Carolina region would exceed their ACL.

The preferred alternative right now is the second row here, January 1 through December 31 fishing season, a seventeen-inch size limit, a two-fish per angler bag limit. There is open days, full year, and then the landings are anticipated to be 412 fish, plus or minus thirty fish, and so it's pretty small, and the change from status quo consumer surplus is relatively small as well.

Then we looked at the impacts of effort shifting, and so we evaluated effort shift scalars ranging from zero percent to 100 percent. In this graphic on the left, you're looking at the number of fishing days, and this is for the preferred alternative for the Florida Keys/East Florida stock, and so the sixteen-inch minimum size limit, a one-fish per angler bag limit, and an open season from July through September. You're looking at the impacts here. Basically, you have no quota closure, and then you get to a certain level of effort shifting and you start getting quota closures, and the season gets shorter and shorter.

The same deal for removals, and probably landings should be what I focus on first, and so the orange line here. You keep going up, and then, finally, you get to an effort-shifting level that allows you to hit your ACL under that preferred alternative, and then the landings flat line, because you're closing at the ACL, and the removals almost flatten, because you're closing the season. Then your consumer surplus, you'll see that you're gaining a little bit and then you start losing, because you start closing earlier and earlier.

There's a lot of uncertainty in these decision tools, and a lot of that is driven by the extremely low ACL and the highly-variable landings data underneath it. As with any projection model, they're going to be reliant on the underlying data and the input assumptions. The projected baseline landings and discards are highly uncertain. Economic conditions, weather events, changes in CPUE, different fisher responses to regulations, and uncertainty in survey estimates could all impact the projected future catch rates.

The landings, historically, have been variable. There have been, in Snapper Grouper 37, some very substantial proposed changes in management boundaries and regulations. All of these could mean the past might not be a very good predictor of the future. Our bootstrapping runs suggest that the closure date could vary by up to a month or more, and so that suggests some pretty high uncertainty, and, like I said previously, the uncertainty was highest for the longest seasons and under the less draconian management measures.

Some limitations of the model were that we didn't really have a good sense of what effort shifting scalar to use, and so we provided it as a user-defined option, from zero to 100 percent. I tried to ground-truth it by looking Gulf greater amberjack. We built a decision tool for Gulf greater amberjack back in 2010. In 2011, based on that decision tool, the council put in a June through July spawning season closure for recreational greater amberjack fishing.

Comparing the 2009 to 2010 data, by wave, to the 2011 through 2015 data by wave, if you look at Wave 3, which contains the months of May and June, and so one of the months of the closure, if you look at the pre-closure time period versus the post-closure time period, the landings were cut almost perfectly in half, which is what you would expect if there were zero effort shifting. It was like, all right, sweet, zero percent effort shifting, and it looks good.

Then you look at Wave 4, which contains July and August, and the pre-2011 time period versus the post-2011 time period, and the landings look pretty much the same. Dang. 100 percent effort shifting, and so that's why we provided the range, because we're really not sure what the appropriate scalar for effort shifting should be, and so we really wanted to just evaluate what are the consequences of effort shifting and provide the council with that important caveat, that effort shifting could result in an earlier quota closure, much more landings than you would expect.

We also recognized a limitation here in that our size and bag limit impacts are computed separately. I think there's an opportunity in the future to try to develop a single data file that addresses those impacts in a combined manner, so that when you implement both a size and a bag limit that you don't have any risk of double counting.

Another issue would be that the gears were evaluated in aggregate and then parsed to hook and line versus spear, based on SEDAR 37 observations. I would love to get a data file that would allow me to evaluate all these management measures by gear, to provide a more accurate estimate of what the change in dead discards would be. Again, that's kind of a feature that maybe is less relevant for management to the ACL, but certainly is an important thing for the council to be aware of with regards to the cumulative impacts of their decisions.

This model also assumes no spearfishing off of Georgia through North Carolina, because there were no observations of spearfishing trips observed in 2010 through 2012 by SEDAR 37. We know spearfishing happens recreationally between Georgia and North Carolina, and so this model likely overestimates dead discards for that area.

Some conclusions would be that it's going to take a combination of size, bag, and seasonal closure to constrain that Florida Keys/East Florida harvest to below the ACL. One point to note would be that the size at 50 percent male maturation in the Keys, back in 2008, was estimated at 16.4 inches fork length, and so I think the current minimum size limit preferred alternative of sixteen inches is getting them close to that transition size.

Also, we found that no additional measures were going to be needed for Georgia through North Carolina to constrain harvest below the ACL, and their size at 50 percent male maturation appears to be twenty-four inches fork length or greater, and so it's a very different size at transition, and we think that that size at transition is probably mediated by observations of the fish in their environment. As the stock becomes more and more depressed, as the size structured becomes

more truncated, you would anticipate that that size at transition would probably compensate accordingly. With that, I will leave it open for some questions.

DR. BARBIERI: Thank you much for that presentation, Nick, and we have a number of action items here to go through. You discussed a lot of points in a lot of detail, but let me open up the floor for any questions that you might have specific to this presentation.

DR. CADRIN: A question for confirmation and a comment. When adjusting the minimum legal size limits, I assume that the reference points and the projections are not being revised for projections that are consistent with those selectivity patterns. Is that true?

DR. FARMER: Yes, that's correct, and that was one of the big take-home messages from your webinar discussion in March, based on the yield per recruit analysis, was that it appeared that changing the size limit would allow you to retain the same ACL in numbers, because the loss that you would anticipate from catching bigger fish, up to a higher level -- Basically, you're going to allow them to catch more total pounds by increasing that size limit. It would be offset by the increased yield per recruit, by allowing the fish to get to that larger size.

DR. CADRIN: Okay. I think we just need to recognize that that feedback is not -- It's not an integrated reference point projection management model, and that perhaps some iteration might be needed once the council makes a decision on a size limit -- You could revise the reference points and the projections to recalculate, and so it would take some iteration, where they're not integrated, but I think we should at least recognize that they may not be consistent.

DR. BARBIERI: Very good point. Thank you, Steve. Any other specific questions for Nick?

DR. CROSSON: I'm sorry, but I had to step out of the room. The presentation that Dave Records gave to the SEP, are you giving that one to the SSC, or is that just not necessary?

DR. FARMER: I'm not sure if Dave was going to give that for the commercial tool. I provided the recreational version of it in this.

DR. CROSSON: All right. The SEP gave -- I'm not going to read all of it, because a lot of it was rather technical, regarding some of the estimates of consumer surplus, but we had a pretty good interaction with Dave Records the other day at the SEP meeting, and it seemed like a very productive discussion on both sides, and so there are some things in the SEP report that if anybody is curious about the consumer surplus numbers they can look at, but I think it was useful advice, and it's very interesting stuff that you all are doing.

DR. SERCHUK: I want to get back to Steve's point, because I think you have to -- I agree with his point about the reference points, but if the reference points don't take account of the transition between -- This is a species that transitions from one sex to another sex. If the reference point is irresponsive to that, then you have to consider things like what's the size of transition, for going from females to males, particularly if you have a harem-type of species.

While what he said is certainly true, if you have reference points that doesn't take that into consideration, that's not the entire story, because presumably a sex ratio is something important in

this species, and if you don't allow sufficient animals to become males, it can have a very negative impact, I would expect, on the species reproduction. Thank you.

DR. BARBIERI: Very good point. However, if my recollection is correct, I think that the reference points do take into account the transition.

DR. ERRIGO: I believe they do. I think they had a function in there for transition in the assessment.

DR. BARBIERI: Any other questions for Nick? If not, then let's go over our action items. The first one, which has a number of sub-bullets to guide our discussion, is to discuss the uncertainties associated with these decision tools. Are the datasets appropriate for the types of analysis being conducted? Should datasets and methods from SEDAR be considered?

DR. ERRIGO: This is always an issue that we have when we have a SEDAR assessment, because we have data from SEDAR, but it doesn't always match with the landings data let's say that's used to monitor ACLs, and so that's why that question came up.

DR. BARBIERI: That has to be a fairly common thing that happens and difficult to avoid.

DR. FARMER: To set you at ease a little bit with regards to the question, I think it's becoming more and more common that the data provider for SEDAR is the same individual providing the landings data to the Regional Office for this catch monitoring type of work, and one of the reasons that we typically don't use the data directly from SEDAR when doing our management impact stimulations is because the data coming into us from the Science Center for ACL monitoring purposes is the data that we'll use to manage the stock, and so we're trying to keep our management decision-making model data consistent with the data that we're going to use for management decision making.

DR. BARBIERI: Thank you, Nick, for clarifying that. Are we okay with that bullet there? Okay. Are the time periods for each of the data series appropriate? Any concerns from the committee regarding the time periods presented for the different data series? No concerns. What are the potential tradeoffs between timely data, i.e., most recent information, and complete time series, consistent time series across years and fisheries? Can you be a little more specific?

DR. ERRIGO: This is only coming up -- There was some data that was used from 2015 before 2015 was finalized. That was Waves 1, 2, and 3 that were used, and so that's why -- Do you guys feel that that's fine, or should we stick to full years? What's appropriate for these types of analyses?

DR. BARBIERI: Thank you. That helps me understand what this bullet is really related to.

DR. REICHERT: I was thinking that it may increase some uncertainty on the other end if you have a signal, possibly, from previous years to indicate there is no anomaly going on. Then I would feel comfortable using that, and so that may be something that can be looked at. If the data indicate that something is going on in that year and that you don't have complete data yet, then that may be caution, but, otherwise, I would be comfortable with that.

DR. FARMER: The reason we wanted to make sure that 2015 data point was in there is because we had a very early quota closure in 2015, and so the time series that we had for 2015 contained the entire time period that hogfish was open and some, because we closed it so early.

Basically, in 2014, and then, in 2015, we had extremely high landings in the early waves, I think something around 80,000 in 2014 and then even higher in 2015, and so we didn't want to ignore that really strong signal that winter catches of hogfish appear to be really high, but then we also looked at the interannual variability there with regards to what wave was showing the peak signal and what the interannual time series was, and it was super, super noisy.

That's why we averaged across the years, kind of hedging our bets that maybe this is somewhere in the range of what we're going to see, that this is kind of our average expectation, but I think, in the report, we really tried to stress that, although these models are probably pretty useful for a relative comparison of the impacts of the different management alternatives, I wouldn't hang my hat on the precise landings that you would anticipate or the closure dates that you would anticipate coming out of these models, and that's pretty true for the Florida Keys/East Florida model and very, very true for the Georgia through North Carolina model, although, with the Georgia through North Carolina model, we're not seeing any signal that says we're going to hit the quota, and so I'm a little less concerned about the uncertainty in the landings there.

DR. BARBIERI: Yes, and I think that, as long as you have that uncertainty presented and it's transparent, then you can take that into account. Is the procedure for estimating daily catch rates from wave-level data appropriate and consistent with how the data are collected? For example, given month and day, are both collected for each trip? I think this is relative to breaking up to a level of resolution of the data that is below the wave data that's coming in for recreational. Nick, I think that the issue that you pointed out, relative to that example, with the greater amberjack, I guess exemplifies some of the complications, I guess, that would come.

DR. FARMER: One of the things that we've thought about is, gosh, it would be really nice to be able to distinguish between say weekdays and weekends or holidays and non-holidays, but it's very difficult to take that MRIP data and partition it out at that level that would require a special request to the Office of Science and Technology. It would really break out the data at a level probably much finer than it was ever intended to be broken out at, and so, given that we kind of manage using this daily kind of catch rate approach, and given that, in some instances, we haven't seen big differences between weekend and weekday catch rates for other species -- We do see them for this one, but it's just very hard to partition the data out at that resolution and have much confidence in it, and it's kind of that law of diminishing returns, I guess, in terms of how much effort you expend to break the data out at that resolution versus the reliability of the data once you get it there.

DR. BARBIERI: Any additional points on this bullet from the committee?

DR. SERCHUK: Just one question. I mean, presumably, one could apply the tool in past years, for example, and then see how closely you approximated -- Let's say there was a closure. How close are your predicted values for closure, approximated to the closure value? Would that be something that you could do to basically see whether the model functioned at the level that you're thinking it's useful at?

DR. FARMER: Yes, I think we might be able to do that sort of retrospective analysis, maybe for just the baseline catches portion of the model, but I mean we recognize that that's a weakness of it right there. We know we're averaging across the last three or four years, depending on where the data is coming from, because we don't have all of 2015 in there.

We would fully anticipate that the model would overestimate the landings in 2012 and 2013, probably get really close to 2014, and then probably underestimate 2015, just because of how we computed the projected landings, and we were kind of hedging our bets along that line, and we wouldn't be able to, I don't think, do a retrospective simulation of the size limit, bag limit, or seasonal closure portion of the analysis, because we didn't have those in the past.

Now, for some of our other fisheries, we have done that sort of retrospective look, to see where we're getting on these things, and, really, it comes down to a function of how much data is feeding the model as to probably how well we're doing the projections. Like, for Gulf of Mexico red snapper, we're doing pretty darned well on our projections. South Atlantic black sea bass, we've done pretty well on our projections, and so these species that are higher-profile that we have a lot of data going in, multi-million-pound quotas, you get a lot of information, and you can make a better prediction.

For hogfish, because it's partitioned so finely -- You know we're already exceeding kind of the stratification of the MRIP data, because we're pulling Monroe County out of the Gulf and throwing it into the South Atlantic, and then we're adding just East Florida to it, and so we're already kind of partitioning it pretty finely, and so adding additional stuff on top of it -- Again, it's just the uncertainty is very, very high, and so kind of the take-home message is, on a relative scale, I think it's useful to say this alternative is going to result in an earlier closure date than this combination of alternatives, but, in terms of we're definitely closing that date, I wouldn't hang my hat on it.

DR. SERCHUK: I wasn't looking for that. I was looking just for some validation, because, normally, we put models together that we -- We try and make common sense, and we try to make reasonable assumptions, but actually, many times, we don't take it and say, can we externally validate this model, either by subdividing our datasets and using data from one part to see whether it actually brings forth the reality that we know happened in another dataset. That's all I'm asking.

DR. FARMER: Yes, and I think that would be kind of like a cross-validation approach, internal cross-validation, where we could pull a year out of the model and then see how well it predicted that year, and that's certainly something we could look at.

DR. BARBIERI: Yes, that's a good recommendation.

DR. IRWIN: To that point, on validating the model, it's also sometimes informative, if you're going to engage in that exercise, to pick one of the parameters where you're most uncertain and see across what range of plausible values can this parameter be adjusted, in order to have the model actually be able to reproduce what was actually observed, and sometimes that's kind of an informative way to see if people are comfortable with the range of values that can be adjusted on the most uncertain parameters. Thanks.

DR. BARBIERI: Thank you, Brian. That's another great recommendation there to explore some of these sort of performance and validation metrics for this model. Going on to the next bullet, do

these decision tools appropriate account for the overlap in reductions estimated for implementation of multiple management measures? In this case, I think, Mike, you're talking about the stacking up of size, season, and bag limits.

DR. ERRIGO: I think Nick already said that it doesn't. They weren't able to do that, but didn't the SEP have some comments in relation to accounting for the overlap of management measures, something about calculating the percent reduction from let's say the size limit, reducing your data by that percentage, and then calculating the bag limit percent reduction on that reduced dataset or something like that? I can't remember who it was that made the suggestion, but I remember hearing it while I was in the SEP meeting.

DR. CROSSON: I think Jim Waters made some comments regarding the order in which you did the reductions, and he suggested that you do the minimum size limits first and then the limits for trips.

DR. FARMER: David and I have been talking about how we would handle that, and I guess I'm trying to figure out if it would work. I guess the idea would be that -- Basically, the way that these reductions, scalars, are computed is you're comparing the landings that happened without that simulated management measure to the landings under the simulated management measure, and so what you would do, in that instance, is you would scale down the baseline, but I am wondering, would you also scale down the remainder, and so therefore you would come up with the same answer?

If you're multiplying both of them by the same percentage, then the output is going to be the same. It will be the same ratio, and so the only way that would work is if it made sense to only scale down the baseline, so the landings that would have happened under -- Let's say you have a fifteen-inch minimum size limit and then you apply a bag limit reduction on top of that.

You would say that the landings under a five-fish bag limit are now reduced by say 60 percent, and so that's your new baseline, but then would you rerun the simulation of the bag limit reduction and also hit it with that reduction from the size limit? If you did that, then, like I said, the ratio is the same, in which case you don't get any new information and the model stands as it is, and so it's only if you scale down the baseline by that percentage, but then left the new landings coming out of the simulated bag limit reduction the same, that you would get a different ratio.

I don't know. We've got to think more about that one and figure out if that makes sense. The cleanest way to do it would be to use the same information and then run the analyses through the same dataset simultaneously, so that you kick out the size limit guise and then you kick out the bag limit guise in the same data, but, unfortunately, the dataset right now is two different datasets, and we don't have a good way of tying them together.

I think probably the cleaner approach is to just recommend and discuss with the Southeast Fisheries Science Center, which is who we receive that data from, is there a way that we can integrate these two datasets, because there's got to be a way, because they're both coming out of MRIP and they're both coming out of headboat, and so there must be some sort of way to link the records and do it, and I think it's just a matter of doing some creative coding, and it might not happen in time for this particular amendment, with the statutory deadline that's in place to get it done, but it's certainly

something that we need to look into as an office, and we're aware of it and now we've got both our data management group and our econ group actively trying to figure out a solution.

DR. BARBIERI: Very good. Thank you, Nick. Moving on to the next bullet, are all assumptions made appropriate and consistent with standard practices?

DR. BUCKEL: I have one on are the assumptions made appropriate. Nick, correct me if I'm wrong, but you have this one laid out in the spreadsheet as a warning. This is the one that I guess it would -- In my mind, you've got the scalar that goes to 100 percent effort, but there's the chance that, with rebuilding, with more hogfish out there, that there could be more effort that would go into that fishery, and so that potentially could go over the 100 percent. Is that right? I know the note said that folks are targeting other things and then they get hogfish as part of the --

DR. FARMER: The effort shift scalar only applies when you select a seasonal closure, and the top end of that range is 100 percent. If you did believe that under rebuilding, and this projection model really only applies to the -- I guess it's supposed to apply to the 2017 season, and so if you believe that rebuilding would happen within the 2017 season, which I don't think -- I mean we wouldn't have enacted any management measures yet that would result in rebuilding, but if you thought there was a reason that people would increase their effort towards hogfish beyond those daily catch rates that are observed as an average of that last few years, then yes, you would see higher daily catch rates and you would result in an earlier quota closure than predicted by the model.

We try to kind of more encompass that in the discussion, rather than build it into the tool, but that's certainly something to be aware of as hogfish moves forward. This decision tool is really only intended to apply to the first season, and it's trying to give the council a sense of what are the combined effects of the different management measures they're trying to put into place, so that they can pick something that keeps the stock below the ACL and promotes rebuilding and have a sense of whether they're really overshooting on that and pick that optimum set of management measures, based on public input.

The idea of the tool is really to make the management decision-making process more transparent. Anybody can go onto the briefing book and use this tool and then go to their council member and say, hey, I worked out a combination with this tool that I really like and I can live with this, because we recognize, especially for Florida Keys/East Florida, this is going to be a big cut for some folks, and so they're going to be looking for kind of the optimum solution that they can live with, and all of them are suboptimal, I think, for a recreational fisherman. I certainly, as somebody who likes to spear hogfish in the Keys, am not looking forward to these reductions.

DR. BARBIERI: Just to add a little bit to that, I mean I do think that you have been very transparent, you guys have been, in pointing out all of these assumptions, and I think that as long as we keep that transparency, then we can interpret the results, given all of those caveats and points that are made and the uncertainty and the limitations, and I think that helps the interpretative capacity for the tool to be used.

DR. CROSSON: Regarding spearfishing in the Keys, that was one of the things that came up during the SEP meeting, is that the consumer surplus values that they were using as a proxy were based on the hook and line fishery, which spearfishing, probably you have a higher consumer

surplus, considering there's a lot more gear and it's a lot more involved. If anything, some of these losses may be understated.

DR. BARBIERI: So noted. Are the models used appropriate for the available data and the analyses being conducted? Again, my interpretation of this is are we stretching the use of the model beyond the capabilities of the data?

DR. ERRIGO: If you have suggestions for alternative models that may work better with those data at hand, this would be the time to bring it up. You have had quite a bit of discussion on the modeling.

DR. REICHERT: Yes, and that was my point. Given the discussion and the caveats, the discussions we've had and the caveats mentioned, I would say that the answer to this bullet point is yes.

DR. BARBIERI: Okay. Then, finally, determine whether these tools use the best scientific information available and are appropriate for use in managing South Atlantic fisheries. Any concerns from the committee in considering this the best scientific information available and appropriate for use in management? Seeing none, I guess the answer to that is yes, no concerns.

DR. ERRIGO: We have David Records on the webinar, and he is prepared to talk about the commercial tool. It's very similar to the recreational tool. There are a few differences in how daily catch rates are -- They're actually modeled with a SARIMA model and things like that, and so if you would like to have him go through that really quickly. Most of your discussion probably will apply to this, but if would like, for completeness, to go through that and then you can determine whether you think all your comments for the recreational tool still apply for the commercial tool.

DR. BARBIERI: Wasn't this one already presented and discussed by the SEP?

DR. ERRIGO: Yes, and so the SEP did see this, and they had quite a lot to say, but we would like -- The SEP is a subpanel of the SSC, and we do need the SSC to stamp this as best scientific information available, because the SEP can make recommendations, but --

DR. BARBIERI: Okay, and so, yes, by all means, let's go ahead and have the presentation. Perhaps we can, as we go through our bullets for the action items, we can sort of speed through and see if there are any additional concerns or points or questions, but if it's very much in line with what we just saw, we can apply the same types of general conclusions to that. David, are you ready?

DR. RECORDS: This is Dave Records over at the Southeast Regional Office. Today, I am going to present to you the commercial sector economics effect estimation that we did for Amendment 37. Just to point out one difference, this is not actually a decision tool for the council staff to use. It was conducted as a static analysis, with all combinations of management alternatives having estimates that were provided, and I gave this exact same presentation to the Socioeconomic Panel, and so, for those of you who have already heard this, it might not be that exciting. You might need to get a cup of coffee.

The goals are very straightforward here. We want to estimate what the economics effects to the commercial sector of the various Amendment 37 alternatives would be. Here, we are measuring them as a change in commercial landings, ex-vessel revenue, and so that's going to be our proxy for profit, because we don't actually have sufficient cost data to estimate profit, and then also we're measuring changes in season lengths.

The first step in this analysis was to get an estimate of the baseline landings for future years for each of the subregions that are considered. In the graphs below, you see hogfish landings in pounds whole weight for 1986 through 2014 for each of those subregions. Our analysis of this graph shows that, for the East Florida/Florida Keys subregion, there is a clear increasing trend in annual landings for the last four years of the observed dataset. The Georgia through North Carolina side, we don't see that same trend. It's more of a random pattern.

We took two different approaches, one for the Florida Keys/East Florida subregion and one for Georgia through North Carolina, based on our interpretation of that past figure that you just saw. For the East Florida/Florida Keys subregion, we decided to build a time series model to try and capture that increasing trend, and also to account for seasonality. For Georgia to North Carolina, we just stuck with a simple average landings from the most recent three years of available data.

The type of time series model that we elected to use was a seasonal autoregressive integrated moving average model. We fit this to average daily landings by month, and the reason we did daily landings by month versus annual landings is, ultimately, we're interested in measuring changes in season length, and that requires daily-level estimates.

The graph below shows the average daily landings by month in blue, and that's overlaid by the SARIMA forecast in red. This SARIMA model had a first and twelfth difference to account for non-seasonal and seasonal trends in the data. It had a first-order autoregressive term and a first-order moving average term.

You can see we've predicted two years into the future past the end of the observed dataset in 2014. The gray and blue-shaded areas represent the 75 percent and 95 percent confidence intervals. They're rapidly expanding, due to compounding uncertainty, as we go further into the future. The SEP had a couple of excellent recommendations on how to potentially improve this model fit. One was to include a four-year lag variable.

My initial experimentation with this yesterday did not prove fruitful. I was not able to get a statistically-significant value for that lag term. However, I did play around with adding in a seasonal moving average term, and that actually improved the fit slightly and also shrunk those forecast intervals a little bit, and so these estimates may be subject to change as we play around with the model specification just a little bit more.

From the existing SARIMA model, we estimated 2016 annual landings would be 28,406 pounds whole weight. This is the proxy we used for 2017, and that's because we didn't feel comfortable estimating further into the future, given those rapidly expanding forecast intervals. For Georgia through North Carolina, again, we just took a simple average of the past three years. That comes to 28,534 pounds, and when you sum those two subregion-based landings together, the value is less than the status quo ACL, and so our assumption is that, in the absence of South Atlantic Amendment 37, we would not actually have a quota closure.

For East Florida/Florida Keys in 2017, we estimate our baseline season length would be 365 days. Our baseline landings would be 28,406 pounds whole weight. Again, I'm playing around with the model specification there, and, by adding in that seasonal moving average term, it looks like these might be reduced slightly, by maybe 8,000 pounds. The average annual price that we used was $3.74. This was used to generate baseline ex-vessel revenue estimates of $106,000, approximately.

For Georgia through North Carolina, our baseline landings are 20,534 pounds. These are lower than the lowest ACL alternative that's being considered in the amendment. Therefore, our assumption is that the season is going to be 365 days, regardless of which management alternatives we select for that subregion. Again, we estimated baseline ex-vessel revenue by just multiplying the baseline landings by that average annual price.

Now that we have our baseline landings, our goal is to project what our new landings are under these different management measures. Here, I am going to discuss briefly how we did this for trip limits and size limits, but it's very similar to the presentation that Nick just gave you for the recreational side.

For the trip limits, the way we calculated percent remaining was we truncated trip-level landings at each proposed trip limit within a given time period, and then we summed overall trips in that time period and then divided by the total sum of non-truncated pounds in that time period. Now, using this percent remaining scalar to project new landings makes a couple of important assumptions. One is that the distribution of trip-level landings will be the same in the future, and, two, that as we reduce trip limits that we're not going to have any shift in effort. Commercial vessels will just harvest less if the trip limit is reduced.

For the minimum size limit percent remaining scalar, here we calculated this by adding the weight of fish that are greater than or equal to each proposed minimum size limit to the weight of fish smaller than the current minimum size limit to account for some non-compliance or measurement error, and then this was divided by the total harvest for that time period.

Now, using this in the future assumes that the landings distribution by fish size would be the same, and the SEP had a good recommendation here, which was to apply the minimum size limit percent remaining scalar to the underlying data that was used for the trip limits prior to generating the trip limit percent remaining scalar, and this would help us avoid over reducing baseline landings by double-counting fish that may have already been thrown back by a minimum size limit, and so they shouldn't really count towards the trip limit.

Now we have our baseline landings and we have our percent remaining scalars. What we want to do is estimate what our new landings are, what our new season length will be, what our new ex-vessel revenue would be. For the East Florida/Florida Keys subregion, because we expect the ACL to be constraining, we're interested in changes in season length, and so we have to do this at a daily level. What we do is we sum the average daily baseline landings by month, which were generated by our SARIMA forecast. We apply those percent remaining scalars to those daily landings, and then we sum over the course of the year.

When that running daily sum of landings equals the proposed ACL alternative, at that point we simulate a quota closure. We stop fishing. The cumulative landings at that point equal our new

landings estimates. The season length equals the number of days from the start of the fishing year to that closure date, and then we generate ex-vessel revenue estimates by multiplying, once again, by that average annual price.

Here, using the average annual price makes an implicit assumption that we're not going to have price effects as we change the temporal distribution of landings, and that's based on economic theory and assumptions about price elasticity for hogfish, and so we discussed that with the SEP, and they were okay with that assumption.

For Georgia through North Carolina, because the ACLs are not expected to be constraining, we're not trying to measure season length. We did this at an annual level. We applied the annual trip limit and size limit percent remaining scalars to our annual baseline landings and then generated the ex-vessel revenue. That's, once again, using that average annual price.

A few brief results, highlights, based on the current model specification. Again, these might be reduced slightly when we respecify the model. The status quo landings and ex-vessel revenue for East Florida/Florida Keys would be reduced by 87 percent to 88 percent, and this is a huge drop, percentagewise. As I mentioned during the SEP meeting, it's not a huge drop in terms of actual magnitude, but we were advised to break this down by gear types, because some gear types may be more effective than others, and so we're going to look into that. The season length is fifty-five to 191 days. Again, the status quo season was expected to be open all year.

For Georgia through North Carolina, we don't expect ACLs to be constraining. The season length will be 365 days, under any of the combinations, and here are status quo landings and ex-vessel revenue, and that would be reduced by anywhere from zero percent to 47 percent. There are a number of ACL alternatives in the amendment, all higher than the baseline landings, but they could represent a higher potential economic benefit if our baseline landings are in fact underestimates.

I would now like to turn it over for questions, and a lot of the details on the model fit statistics and the procedures for running those daily sums are included in the attached documents in the briefing book. Thank you.

DR. BARBIERI: Great, David. Thank you so much for this overview. Before I open up for questions, let me ask Scott to add any additional -- Dave, I thought, did a very good job at highlighting all the points that were brought up by the SEP, but anything else that you would like to add?

DR. CROSSON: First of all, don't sell yourself short there, Dave, because I know at least one member of the SEP is tuning in right now to see the presentation a second time, and so this is good stuff. We didn't have any disagreement with the use of the price per pound for the marginal value. I wanted to ask you something that I thought earlier in your slideshow. We had noticed, the SEP had noticed, that there might be a pattern in the landings. That was peaking every three to four years, and so we had asked whether you wanted to try to use a four-year lag to account for that. Did you say that you had tried that since the meeting and not found anything?

DR. RECORDS: That's correct. I did not get significant results for the four-year lag. I think one other thing that the SEP just -- It just came to mind that you guys mentioned was potentially adding in exogenous variables into this time series model, and that's something I actually intended to ask

the SSC, if there are any biological variables that might inform this model and kind of explain some of that cyclical pattern that you're referring to.

DR. CROSSON: That's exactly it, because, as a group of economists and social scientists, we had noticed it, and we thought it might be related to the biology of the stock, but it's something that's outside the realm of our knowledge, and so I don't know if the specialists here have any opinion on that, if it's real. I mean we see it, but --

DR. SERCHUK: I think this is -- I attended part of the meeting, and this was something that I was a little bit concerned about. If we could go back to the very first slide, it shows the landings. You can see what the concern is about. You can see that we have an initial period, we have a decline, and we have an increasing trend and we have a decline. Then we come down to 2004, and we have an increasing trend. Then we have a decline. we have an increasing trend and then we have a decline and then we have an increasing trend.

What the model is doing is it's taking that increasing trend, at least as I understand it, for the baseline years and projecting forward to continue that increase, and so the question was raised at the meeting of are we seeing a pattern here that the assumption of increasing landings again -- Will that be thwarted by looking back at the history and saying, well, we haven't really had any years, beyond three or four years, where the increases have been sustained.

As Scott pointed out, is there some biological reason of why we tend to have these cycles about every four years? That's the question I think he's asking the people here, but I am concerned that the decreases that are being predicted, this 80 percent decrease, may be overestimated, to a certain degree, because the baseline is higher than you would have expected in 2016, for example, if we're actually going to have a decline in the catches. We don't know, but that's the issue.

DR. RECORDS: In terms of relative percentage decreases though, we're using these baseline landings estimates to generate the new landings estimates, and so, in percentage terms, they should be the same, but I understand what you're saying. The model does put more weight on the local trends in this time series, because it is a non-stationary time series, and it doesn't include a constant in the model. I've tried to specify it with a constant and not do the first differencing. Then you do see somewhat of a negative trend. However, the constant comes out very insignificant in the model, and, based on several tests I ran, it looks like there is a random walk pattern going on, which suggests we should be doing this differencing, and so this is really just a caveat of the SARIMA model that was specified to have the best fit.

DR. BARBIERI: Thank you, David. Any input from the panel regarding potential biological reasons for this cycling? Nothing that I can think of.

DR. CROSSON: I'm always remind of how good we are at finding patterns and things that aren't necessarily there, and so it just --

DR. BARBIERI: Yes, and I mean it will be interesting to see if this continues into the future. It's quite apparent there. Any additional points or questions from the committee? I understand that we have a whole number of more detailed comments and recommendations in the SEP report, right, Scott?

DR. CROSSON: Not so much for the commercial stuff. Most of our comments were on the recreational side. I look at these peaks right here, and they also seem to be centered around presidential election years, but I don't know that that's the cause of it.

DR. BARBIERI: In terms of discussing uncertainties, and we have already discussed several, but without us having to go through all these detailed bullet points --

DR. REICHERT: Maybe the way to say it is are there any additional concerns that we should address? I personally don't have any, but that may speed up the discussion.

DR. BARBIERI: Then, for the second bullet, the last one there, Mike E., Dave basically explained that this is not really a decision tool. At this point, it's just basically an analysis that's being conducted to inform this.

DR. ERRIGO: Right. You can't go in and change the different alternatives, but it still would be good to know if this analysis is best science available and can be used for management in Amendment 37, that's all.

DR. BARBIERI: Any concerns from the committee in considering this the best scientific information available and appropriate for use in management? No concerns? Thank you so much, David, for not just the presentation, but addressing the questions and clarifying points.

DR. RECORDS: Thank you as well for all the excellent suggestions.

DR. BARBIERI: Then this completes this agenda item, Mike E.?

DR. ERRIGO: Yes, and, also, we were talking about that we may want to, and we discussed it with staff, that it would be all right to skip over the Amendment 37 discussion, because we were actually going to have that presentation right before the decision tools, to orient you to Amendment 37, but, seeing the time and what's left, we feel that we can forego that, to try to get some of the other agenda items tackled.

DR. BARBIERI: Right, and that's exactly to the point that Marcel had brought up earlier. He has put together a list here in trying to see how many of these items we actually have to -- Looking at that list there, we've gone 1 through 4 now. We are going to go to Item Number 5 in our new list. It's Spiny Lobster Review, and we have Sherry Larkin. This is Attachment 15 in the overview. Sherry, you are there on the webinar, right?

Spiny Lobster Review, we have Review Panel Meeting Overview that Kari is going to present, and then, in terms of action items, discuss and make recommendations, as necessary. Specifically, the councils would be interested in the SSCs' input on potential changes to calculation of the ACL and of specific management measures to address landings exceeding the ACT and ACL in recent years. With that in mind, go ahead, Kari.

DR. MACLAUCHLIN: Okay. We talked about this after we had the review panel meeting, and then also we had joint advisory panel meetings last week, and so the tentative schedule for your involvement with spiny lobster is that this is more of a briefing on what's going on and then, in October, bring back lots of information, the landings information and other information about the

fishery, and have you guys evaluate the OFL and the ABC that are in place right now, and so this is just a little background about what has been going on with spiny lobster.

There was a stock assessment, SEDAR 8, that was completed in 2005 and then an update completed in 2010, but the assessments were not accepted by the Gulf and South Atlantic SSCs, because there needs to be a region-wide assessment of spiny lobster, and so the Gulf SSC designated spiny lobster as Tier 3a under their Gulf ABC Control Rule, and they recommended that OFL be set as the mean of the most recent landings in the last ten years, and at that time, it was 2001 through the 2010 fishing years, plus two standard deviations.

Then the ABC was set at the mean landings of those ten years, with one-and-a-half standard deviations, and so that would have set the OFL at 7.9 million pounds and the ABC at 7.32 million pounds, and so the Gulf ABC Control Rule was what was accepted in Spiny Lobster Amendment 10 by both councils. They set the ACL equal to the ABC and then set an ACT, annual catch target, at 90 percent of that ACL, and so 6.59 million pounds.

The accountability measure was that if landings exceeded the ACT, you would convene a review panel to assess whether corrective action was necessary. In the past few years, the landings have gone over the annual catch target, and so, in 2013/2014, they were at 7.9 million pounds, and this was over not only the ACT, but also the ACL and the OFL. The review panel was convened in February of 2015, and the review panel is made up of FWC folks. We had SSC representatives from both SSCs, including Sherry Larkin. There were AP representatives and then also NMFS and council representatives.

Then, in the 2014/2015 fishing year, landings were at 7.032 million pounds, which was over the annual catch target, and the review panel was convened again in March of 2016 via a webinar. This year, the preliminary landings are at about 7.3 million pounds and are already over the ACT and potentially, as they finalize those landings, they could be over the ACL.

I have a chart here to show you what the landings look like, and so this is from the 1991/1992 fishing season through the most recent, with the 2015/2016 landings as preliminary, and so the green line on the top is total landings. The light blue is commercial, and then here are the recreational. There is a break here, because these were the years that were very active hurricane seasons for Florida and so they were not able to collect that recreational data, but, in general, the total landings are driven by commercial, and then we have this red dashed line is the OFL, the purple is the ABC and ACL, and then the ACT. In these years that were used to set the OFL and the ABC, landings were always under those. Then, in recent years, they have increased.

This is a slide from the FWC presentation that was provided to the advisory panels, who met last week, and I like that because they had designated a couple of these different time periods, and so this was before 2000, a historic landings period, and then we had kind of this period of low landings, and that was what used to set the ACL. Since 2010, there's been kind of this increase, and so the conditions of the fishery have changed and the landings have changed.

The Spiny Lobster Review Panels, there have been two of those. The first one, in 2015, talked a lot about what was going on in the fishery and why landings had increased in recent years. In general, it seemed that there is a new live market, where live lobsters are sent to China, and this has contributed to higher landings in January and February. Usually, most of the landings are in

late summer and early fall and then they switch to other things, but, because of this live market, the landings in January and February have been higher.

The review panel did not recommend a new stock assessment. They did not feel that that was going to provide any additional information, but that the ACL is the wrong methodology to manage spiny lobster, and also that OFL should be redefined as the MFMT, and then no recommended changes to accountability measures.

The 2016 review panel met via webinar in March and they made some recommendations using motions. Not all of these were unanimous, but one was to recommend calculating the ACL based on landings from the 1991/1992 through most recent, to capture all the dynamics of the stock and the fishery. Another motion that was approved, although not unanimous, was examine setting the ACL based on a rolling average and then examine setting the ACL trigger, which would trigger the accountability measure based on the landings and the landings to effort index.

The Gulf and South Atlantic Spiny Lobster Advisory Panels met last week, and they passed a few recommendations as well. One would be that the council should accept the recommendations of the review panel to use that longest period, 1991 through the most recent landings, with the rationale that the landings are increasing in the past few years, but they're not as high as they were in the 1990s, and that the APs felt that this indicated that the stock was improving.

Remember that in the 1990s, that was before the trap reduction program was put into place, and there was an estimated over 900,000 traps in the water. Currently, there are about 450,000 traps, through that program.

The APs felt that the mortality from ghost traps had decreased, because of incentives and regulations to retrieve derelict traps. They also felt that there was probably a decreased mortality of the undersized lobsters that they use as attractants, that are called shorts. They can have fifty of them onboard at a time, but, since the live market, the live export market, has kind of come into play, almost all of the vessels have installed live wells or improved their live wells, and they feel like this has probably decreased mortality of the shorts, and then also that commercial effort is capped through a limited entry program for the divers, and then there's the trap reduction program and a limit on the number of traps, and that recreational effort is stable and not expected to have a substantial increase, and so they felt like looking at this longer period, to capture these dynamics of how the stock goes up and down and the fishery changes, would work better and that there would be not a lot of risk, because of all the effort restrictions in place here.

Then they also requested that in federal and state intercept programs that lobster be made a priority species for size monitoring. This motion was passed because the AP members felt that size was the most important way to indicate if there was a problem or not with the fishery, and they wanted more data collected on size.

Then they also made a recommendation for an accountability measure that is triggered when landings go below a certain point. Usually, our accountability measures are triggered when landings exceed your ACL, and what they felt like is that there would be a problem in the fishery and that they would see that if landings went below a certain point, and so they're recommending kind of flipping those accountability measures, using a lower landing trigger, and that would initiate the review panel if it was below a certain level for two consecutive years.

DR. REICHERT: Kari, that last point was in addition to that higher level or in place of the higher level?

DR. MACLAUCHLIN: They didn't specify, but I think that they wanted to replace the accountability measure, like change it altogether and not use any higher level that would trigger, because it's the same thing. They could have both. They could say convene a review panel when it's above this or below this.

DR. REICHERT: But that's not what they recommended?

DR. MACLAUCHLIN: Yes, they only recommended using a lower landings trigger.

DR. GRIMES: What's the accountability measure?

DR. MACLAUCHLIN: For lobster, it is if landings exceed the ACT, which is 6.59 million pounds, to convene a review panel.

MR. HARTIG: I attended the AP meeting and listened to all the discussions, and there was a lot of discussion about the commercial fishery and the fishing longer in the season and things of that nature, but in the end, and I will make it brief, both the commercial fishing increases are up 15 percent and the recreational fishery is up 15 percent, and so I think the productivity is probably real, given that the same forces that are impacting the commercial fishery are not impacting the recreational fishery, and we are going into a different productivity regime. How high that's going to be, we don't know yet, but I was convinced that at least the productivity increase was real.

DR. REICHERT: To that point, and I was actually waiting for that later, but the landings -- I was wondering if the landings were driven by market-driven processes like the live shipping, and I'm not sure if the economic panel discussed that, and that goes to your question about whether the changes in landings are driven by the productivity of the stock or they are market driven or both.

DR. SHAROV: Being still new to the committee, I wonder, what was the recommendation that the assessment is not needed and that it will not add anything to the existing knowledge? The last one was in 2010. We're in 2016, and just considering that landings are a reflection of the fluctuations in the stock size, sometimes it might be true, but most often it isn't, and so that's a pretty dangerous assumption. That's question number one.

Number two, if there is no assessment, are there any other metrics, any monitoring programs, that give us some sense of what's going on with the population in terms of the CPUEs or relative indices of abundance? Is there anything available to use in assisting with the decisions on possibly modifying the ABCs, et cetera?

DR. BARBIERI: Alexei, if you don't mind, because those are very good points, very good questions that I think we need to address, but she hasn't finished her presentation yet.

DR. SHAROV: I'm sorry. I thought we were already in the question-and-answer part.

DR. MACLAUCHLIN: Also, I want to -- You are not making any decisions at this meeting. The plan is at your October meeting to bring back all of the information that you need. There is lots of

information to consider when evaluating the OFL, not just the landings, but the history of it, and a lot of the fishery conditions and studies from Florida Fish and Wildlife and how they are estimating mortality from the shorts and from the ghost traps, and we'll be bringing all of that to you in October, and so you're not making any decisions right now. You can make any recommendations or let us know what else that you feel like you may want in October, but this is more of a heads-up and just briefing you guys on what's been going on with spiny lobster.

That's really it for this presentation, because it was just briefing you guys. The South Atlantic and the Gulf Councils are meeting at different times in June, and they're going to review the recommendations from the review panels and the APs. The Gulf SSC is going to get this information, the recommendations, from the APs and review panel and talk about it at their meeting later this month, but our plan is that you guys review this in October and evaluate if this OFL and ABC are still appropriate.

I did have a couple of extra slides in here just for reference. The Gulf SSC Tier 3a, that is applied right now to spiny lobster, and then also I had some information about all of the landings by gear type and by commercial and recreational, if anyone needed it.

DR. BARBIERI: Thank you, and just a clarification here. In terms of our action items that we have there in our overview document, specifically the councils would be interested in the SSC's input on potential changes to calculation of the ACL and on specific management measures to address landings exceeding the ACT and the ACL in recent years, are you telling -- That last slide, when you said we're going to actually be addressing this action item in October, or --

DR. MACLAUCHLIN: I think when we were first -- When the review panel was convened and then we wanted to bring the APs together, to get their input, and then really realizing that if the councils wanted to consider raising the ACL that the OFL and ABC have to be evaluated first, because the ACLs can't be over what they are right now. What the review panels and the APs are recommending -- That ACL is higher than the OFL and ABC, and so you guys will be the first ones to evaluate if that's even possible by raising the OFL and ABC, and you had a -- Your agenda was already kind of full.

I think when we were first planning that that we were thinking that you would just comment on what the review panel said, but then we started to think that no, actually the process is going to be bigger and require you guys getting all the information and really evaluating the OFL and ABC and that we would slate it as an October agenda item.

DR. GRIMES: Is there a plan to conduct a region-wide assessment and include the Caribbean? I know the Caribbean Council talks about the same thing, but never does anything.

DR. MACLAUCHLIN: Not to my knowledge. I think that would be an amazing coordinating effort, to get all the Caribbean nations involved in a Caribbean-wide assessment, but there is a lot of new information, I feel like, that has come out with some of the recruitment, looking at internal and external recruitment and genetic studies, and then some other information about environmental factors that affect the Caribbean in different ways.

I think this slide with these different periods -- What was going on in these different periods and how are they different? Are we in a different period for the spiny lobster fishery here? Have

things changed enough? We will never know exactly why landings dropped so substantially, but there are probably lots of factors. There is a virus that affects the juveniles, but the trap reduction program, there could have been lots of market factors in play there. There were a couple of very active hurricane seasons and lots of things.

Maybe whatever was going on there, the fishery has started to rebuild itself and it's okay, and that's what I think the councils will come back to you and want you to talk about, if the OFL is appropriate for this period that the fishery is in.

DR. BARBIERI: Okay, and so, before opening up for additional questions, since Sherry is on the webinar, Sherry, do you have anything to add? You were a member of that review panel. Is there anything that you would like to add?

DR. MACLAUCHLIN: Sherry says that she will definitely be here in October and be a part of that discussion.

DR. BARBIERI: Yes, that will be a lot easier, in person, and thank you, Sherry. Now there is a question of if we're going to revisit this in more detail in October, and considering the time constraints of whether we will go -- I think that Alexei's questions are very important, and I can give you a summary. I was chair of the review panel for that assessment, whenever it happened, and so I have some recollection of what happened there and could talk about it, but I'm wondering, considering the time constraints, if we should just wait and do this in October, since we're going to have a more detailed discussion then, and actually address the action items then. Hold that thought, Alexei, and we're going to revisit it in October. Thank you, Kari.

Let's review the number of items. It's a little bit past lunchtime, and I need some guidance here, Mike E., on what you think is really necessary for us to cover today, so we can see whether we continue through this and have a late lunch or whether we break for lunch.

DR. ERRIGO: This is going to take a long time.

DR. BARBIERI: We have a couple of fairly long, relatively long, items, and so let's break for lunch. Let's try and get back here and reconvene at one o'clock.

DR. BARBIERI: All right, everybody. Let's move forward with our agenda. We only have a couple of hours left, and the next agenda item is Review of New Bag and Size Limit Analysis Methodology that Mike Errigo is going to present and discuss with us. I guess this was in our original briefing book. It was Agenda Item 7. We had the black sea bass bag limit analysis report, and we have the usual set of action items that come with this type of presentation. Discuss the uncertainties and determine whether this is the best scientific information available and appropriate for providing management advice.

DR. ERRIGO: I am going to go through this presentation, and I want to make sure that I don't lose anyone, and so if you have a question at any point, just let me know, and there are certain points that I will break and ask for questions as well. Real quick, how this came to be, the council decided that they wanted to consider a bag limit increase for black sea bass at their September 2015 meeting.

When we do a bag limit decrease analysis, it's fairly straightforward. Any trips that are landing above the new proposed bag limit, we reduce their landings down to the new bag limit and then proceed with the analysis that way. However, we didn't really have a great way of handling a bag limit increase. In the past, what we did was we assumed that any trip that reached the current bag limit would reach the new bag limit, all the new proposed bag limits.

We did refine that approach to say that we would only increase the landings of those trips that hit the bag limit up to their reported discards of whatever the species was of interest that we were looking at. For black sea bass, if they reported discarding ten black sea bass, then their landings can only be increased by ten fish before they were cut off, and so that was the refinement, but, up until now, that's how we've had to deal with that kind of analysis.

The assumption of that method is that all the discards on trips that reach the bag limit are discarded due to reaching the bag limit and not because they were below the size limit. However, when we looked at the black sea bass data, the recreational data, about 89 percent of the trips that encountered black sea bass had no landings at all, and less than 1 percent of trips were actually hitting the bag limit. The discards made up over 93 percent of the total catch.

From these data, it was suggested that really it was the size limit that was driving the discards and not the bag limit, and so the assumptions that were made in our previous analysis were really not correct, and so that's why this analysis was developed. There is also some talk of changing the size limit, but there was talk about perhaps even decreasing the size limit, due to these facts, and we also don't have any established analysis for a reduction of the size limit.

This is just to show, for MRIP, headboat, and the total number of trips, this is the percentage of -- The blue is the percentage of trips that did not hit the bag limit, and the red is the percentage of trips that did hit the bag limit. As you can see, it's very, very few trips reaching the bag limit. These are the percentage of trips that, for the first column, there were no landings of black sea bass at all, but they had reported discards. Then the next bin is the percentage of trips that had landings per angler of less than one, and so less than one fish was caught per angler on the trip, and then one through five is the -- They caught one fish per angler, two fish per angler, and so that's the numbers of trips. Five is the current bag limit. You can see less than 1 percent.

This is landings to discards, and so discards are -- Almost 93 percent of the total catch is discards. These are the dead discards, and so this applies the 7 percent discard mortality from SEDAR 25 and the update, and so 46 percent of the total killed are dead discards. Does anybody have any questions on any of that before I continue on with the actual analysis?

DR. SHAROV: Did you say 46 percent are dead discards? I mean the graph that we have here essentially says 92.5 percent are dead.

DR. ERRIGO: This is total discards.

DR. SHAROV: These are not dead discards?

DR. ERRIGO: Right. These are total discards, and then these are -- You take the total discards and apply a 7 percent discard mortality rate that was used in the assessment to get dead discards. The goal of the new analysis is to find a way to estimate what proportion of the discards are above

the size limit and below the size limit, so that, when we increase the bag limit, we know how many fish we can add to the catch.

There are some assumptions that go along with this. All the discards on trips that didn't reach the bag limit were assumed to be due to size limit restrictions, and so if you discarded a fish on a trip and you didn't hit the bag limit for that trip, it was probably undersized.

Discards on trips that did reach the bag limit were either from because you hit your bag limit or they were undersized, and so those are the trips where we're looking to try to find the proportion of discards. On headboats, there was the added factor of if you didn't -- On those trips that didn't reach the total trip bag limit, which is just the five-fish bag limit times however many anglers were on the headboat, there was the assumption that some of the anglers may have hit their bag limit and then those discards could have been from bag or size limit, and I will tell you how I estimated that in a bit.

I'm going to talk a little bit about selectivity from the assessment. This is a key part of how this analysis works, and so I will go through that and then see if you guys have any questions. The discard selectivity that was used in the assessment assumed that most of the discards are due to the size limit, and it applies to those fish that are selected to be discarded at age, and so it was a discard selectivity at age. It was dome-shaped.

Age three was the age of full selection, and so the selectivity was one. Ages zero to two were estimated directly in the model, and then ages four-plus, what they did was they calculated the probability at age that a black sea bass was below the size limit, by assuming, first of all, by using raw data from the MARMAP program, and I believe they also used some of the fishery-dependent information, but were able to correct for bias in sizes, because there was a size limit in place, and so they assume the normal distribution of size at age. That's how they estimated the discard selectivity for ages four-plus. That allows the descending limb to change as the minimum size changed, because it did change during the assessment period.

In the analysis, we need to use the discard selectivity, and what we're doing is we're taking the model-predicted numbers at age and the discard selectivity curve and we're using those estimate the size composition of discarded fish on trips that hit the bag limit, and I will explain how we go about doing that.

For black sea bass discard selectivity, we adjusted, because, after the 2013 update, they implemented a thirteen-inch size limit, which it was originally twelve inches, and the terminal year of the assessment was 2012, and it was a twelve-inch size limit. Then the council implemented a thirteen-inch size limit starting in 2013, and so we had to re-estimate the descending limb of the discard mortality rate, the discard mortality selectivity.

We used data from SERFS, and so all the measured fish at age, to calculate the probability at age that a black sea bass was below the minimum size, again assuming a normal distribution, as they did in the assessment. We also pooled all available years, which was done in the assessment as well, because there were not enough data -- There wasn't enough data in any particular year to calculate these probabilities, especially for the older age classes.

This just says to set that selectivity equal to those probabilities of being below the minimum size, and that gives the discard selectivity at age for fish below the minimum size. For ages three and younger, we just used the model estimate, and it was zero to two that the model estimated and three was a one.

For fish above the minimum size, we needed to multiply that probability at age of being above the minimum size by the proportion of trips that reach the bag limit, because it's only applying to those trips. Also, we assumed that, as was done in the assessment, that ages zero to three, there weren't any fish above the minimum size, and so the discard selectivity was set to zero. There are some samples of fish that are above the minimum size, but, overall, it's a very, very small proportion of the samples, and so we just used the same assumption that was used in the assessment for that.

This just shows the probabilities at age, blue being above thirteen inches and red being below thirteen inches. These are calculated probabilities. Then here are the discard selectivities that were calculated. These are for the MRIP data, and so we've got the green is discard selectivity of fish less than thirteen inches, and the blue is the discard selectivity for those fish greater than thirteen inches. You'll notice that there are two different scales for the Y-axis. The scale for the blue line on the right. The scale for the green line is on the left. There's a very, very, very tiny proportion of the fish that are being selected for discarding that are above thirteen inches. That was how it was done for the MRIP data. There are a few extra steps that had to be done for the headboat, but does anybody have any questions so far on the selectivity?

DR. SHAROV: The discard selectivities, is it estimated within the model, within the assessment model?

DR. ERRIGO: In the assessment, the selectivities for ages zero to two was estimated directly. Age three was assumed one, and then ages four-plus was done the same way that I did it. They calculated the probability at age of a fish being below thirteen inches, and that's how they calculated the discard selectivity, or below twelve inches actually, below whatever the minimum size was, and so there were time blocks for selectivity, because there were different minimum sizes.

DR. SHAROV: Right. I guess I'm asking if this was just an output of the -- This is an assessment element of the stock assessment model that has been used most recently, correct?

DR. ERRIGO: Yes, and I modified it for the thirteen-inch size limit, and I also had to, when calculating the discard selectivity for fish above thirteen inches, there was the assumption that for ages zero to three that that selectivity is zero, and that's due to the assumption -- It's an assumption made in the model as well, the assessment, that there were no fish greater than the minimum size at those age classes, but -- There were a few samples, but it was a very, very tiny proportion.

For headboats, there was an extra step for those trips that didn't reach the bag limit. We also need to multiply that by the proportion of anglers that reached the bag limit on those trips, and so the proportion of anglers that reached their personal bag limit, and I did it for each year separately, because I used 2013 and 2014 data and then took the average for the bag limit analysis, but you could have just combined the years and done it and it would have come out almost identical.

I had to estimate the number of anglers that would have reached their bag limit, and I did this by dividing the total number of fish landed on the trip by the trip's bag limit, or the total number of anglers by the trip bag limit, to get -- I had to divide the total number of fish landed by the total trip bag limit, and the total trip bag limit is the number of anglers times the five-fish bag limit. That gives you the maximum number of anglers.

Basically, you're saying take the first five fish and give them to this guy and you've hit your bag limit. Take the next five fish and give it to this guy and you've hit your bag limit, until you run out of fish. That estimates the maximum number of anglers that could have hit their bag limit, but there is no other way to estimate it, because they don't keep that -- That information is not recorded anywhere, and so this is just the maximum number of anglers that could have hit their bag limit on a trip. I have the example here of a five-fish bag limit, a hundred fish were landed, and there were fifty anglers onboard. I would have estimated twenty anglers would have reached their bag limit on that trip.

This is the selectivities for headboat. Like I said, I did 2013 and 2014 separately, but I could have created a single curve. It would have went right through the middle, and the ending analysis would have come out almost identical. Again, the Y-axis is very different for the greater than thirteen inches than it is for the less than thirteen inches, and this is the trips that did hit the bag limit. This was calculated separately. Are there any questions on that?

DR. BUCKEL: Mike, maybe some folks and some of the industry reps could weigh in, but I think, often, if someone goes over their bag limit on a headboat and they catch their sixth or seventh fish, they will give that to someone else that hasn't caught their catch, and so I don't know how -- It almost is like you're not going to see discarding until the headboat has reached its limit, but other industry reps could weigh in on that. I've definitely seen that, but I'm sure there is cases in --

MR. BOWEN: You read my mind. That's exactly -- I just told Myra that before I even came to the table. That's exactly what happens. That's exactly what goes on, but I did have one question for Mike. When you look at the -- On the graph you showed, a couple before that, you assumed that, when you were talking about you had fifty anglers on the boat and they had a hundred fish, you assumed that twenty had their bag limit, and was it also the assumption that thirty people didn't have any fish at all?

DR. ERRIGO: If you calculate it that way, if that's what really happened, then the other people would not have gotten any fish, but that doesn't really change anything. What I needed was what proportion of people on a trip hit their bag limit and then might be discarding fish above the minimum size. Now, for charter boats, I assumed that everyone knew each other and, as you caught fish, you were basically going for the trip bag limit.

If you're telling me that headboats normally operate that way as well, then that will change the assumption here, and that would change -- I wouldn't try to calculate what proportion of people were hitting the bag limit in that sense. I would just do it the same way that I did it for charter boats.

MR. BOWEN: I think your assumption, as far as the charter boat, is correct, at least the ones that I'm aware of and mine, but I am still a little confused with the fifty people on a headboat with a

hundred fish and twenty of them having their max number of fish and thirty not having any. That's not how that goes, ever.

DR. ERRIGO: Right, and so it could be that everybody caught two fish. It could be that everybody landed two fish and nobody hit their bag limit.

MR. BOWEN: Correct.

DR. ERRIGO: This is actually something I was hoping to get feedback on. That's an estimate of the maximum number of people that could have hit the bag limit, and so that is a limitation of this. Now, I could do it the same way as for charter boats. If the trip didn't hit the trip bag limit, then it didn't hit the trip bag limit and it's not discarding fish above the minimum size. I could certainly do it that way.

MR. BOWEN: That would be correct as well.

DR. ERRIGO: I will say that, in the end, headboats make up a very small proportion, for black sea bass in particular, but it could be important for other species. If you think the assumption that if the trip didn't hit the bag limit -- Then I should treat it like a charter boat, then I can change that very easily.

DR. BARBIERI: Yes, and that's the kind of input that Mike E. is actually looking for, so he can refine the methodology.

DR. REICHERT: But that would not be a solution either, because a headboat doesn't function as a charter, and so you're not -- It's my understanding that you're not splitting that catch up over all the anglers, as you would in a charter boat, and so it is -- If any one of the individual anglers hit their bag limit, then the following black sea bass that's being caught is more likely to be discarded than given to others, but that's just a question I have. I'm not sure how headboats function to that detail.

MR. BOWEN: Can you repeat that question?

DR. REICHERT: If an angler on a headboat hits the bag limit and that angler catches another black sea bass, is that black sea bass generally discarded, or is that black sea bass being divided over the other anglers on the headboat? I'm not talking about charters. Charters, I understand. Everyone knows each other, but the headboats.

MR. BOWEN: From my experience, that sixth sea bass would be given to another angler.

DR. REICHERT: In that respect, the headboat functions the same as the charter boat, when it comes to when fish are started to be discarded.

MR. BOWEN: In that way, yes. The difference in a charter -- Again, I'm just speaking from my experience off of my coast, but the difference is, in a charter boat, all the fish go in one cooler or one box, versus, on a headboat, they are usually on stringers, and so they're strung individually. Years ago, we would put them together, and then law enforcement asked us to separate them on

stringers. That sixth sea bass that that angler caught would not go on -- It would go on another angler's stringer.

DR. REICHERT: Yes, and how that's done is less relevant as whether or not that fish goes back overboard.

MR. BOWEN: Yes, the sixth fish, if there's a person onboard that does not have their limit, that sixth fish will not be discarded.

DR. BARBIERI: That's very important. Thank you for that, Zack.

MR. BROWN: Mike, does your analysis take into consideration the depth too, because some areas, with half-days and fishing in closer and stuff, you're going to have a higher ratio of discards than you would be in the deeper water, and I was just wondering if that was across the board with this analysis or if you had a breakdown with depth.

DR. ERRIGO: When I was pulling the initial statistics to look at the black sea bass catch, I broke it up by -- MRIP only has state waters and federal waters, which is not terribly helpful in most states, especially because a lot of the catch is coming from the Carolinas, as well as Florida, and the shelf is so wide that the depth is similar, state waters and federal waters, until you get further out, but you were more likely to keep a black sea bass in federal waters than in state waters, and so there were higher discards in the state water trips of black sea bass.

I could have done the analysis separately on each of those trips, but the data would start to get pretty skimpy the more I broke it down, and I was only using two years of data, but black sea bass has more data than most, but there were significantly fewer trips that encountered black sea bass in federal waters, because there are just significantly fewer trips, in general, like offshore.

MR. BROWN: I hear what you're saying. I was just trying to think about the way that you were producing this analysis and everything we went through with the right whale stuff and the boundary on that and how that boundary kind of rode a line that inshore there you would see probably a lot more discards than you would offshore of that, and I didn't know if there was any consideration given to this analysis as far as depth or anything associated with it.

DR. ERRIGO: The other issue is that the discard selectivity that I have from the assessment is not depth-specific, because there's not enough depth information to create a depth-specific discard mortality selectivity. Even if I broke the data into inshore and offshore and used the same exact selectivity pattern, it would be the same as if I combined it, but I could look at the difference in the impact of inshore and offshore trips or state water/federal water trips, but that's all I would be able to do, because I only have one discard selectivity pattern. I don't have two separate ones. I don't have enough data to create two separate ones.

DR. BARBIERI: Thank you, Mark. If there are no other questions, go ahead, Mike.

DR. ERRIGO: Now that I've created the discard selectivities at age, I multiplied those selectivities by the model-estimated numbers at age for the year that I'm interested in, and that gives me the predicted number of discards above and below the size limit, or the predicted number of fish that

could be selected for discarding above and below the size limit. I know there is also discard Fs, but they drop out of the equation, and I will show that later.

For black sea bass in particular, I only had the numbers at age through 2012, but, in my analysis, I was using MRIP data from 2013 and 2014, and so I had to make the assumption that the 2012 numbers at age -- Really, I only need the proportion of black sea bass at age. That was representative of 2013 and 2014. However, in the future, we've had a lot of conversations with the Center, and they will be able to output numbers at age for the projections, for each of the projection years, and so I could use -- Whatever year of data I have, I could use the estimated numbers at age for that year from the projections to do the analysis, and so I wouldn't have to make that assumption for future analyses.

Then what we would do is would sum up all the discards that I had calculated that were above the size limit and below the size limit and then I can divide by the total discards, so the sum of all the discards together, to get the proportion of discards that were above the size limit and below the size limit. Because I'm dividing the discards above thirteen inches by total discards, that's why things like -- I will show the equation, but that's why things like the discards Fs drop out and also the discard mortality, because the selectivity is for dead discards.

Once I have that proportion, then I can multiple the proportion of the discards that were above the minimum size by the total discards reported on trips that reach their bag limit to get the number of discards that would have been retained under a larger bag limit. If we increase to six black sea bass, this will tell me how many of those fish they reported discarding would have been added to the catch.

Here is where I talked about I didn't need to account for the discard F rates or discard mortality, and I will show you why. Here is the formula for calculating the proportion. This is specifically the proportion of discarded fish that were above the minimum size limit, and so S is the selectivity. SD is the selectivity of discards at age that are greater than the minimum size, and so selectivity of discards greater than the minimum size at age.

Normally, if you wanted to calculate the number of discards, you would need to multiply it by the F rate and the total number at age. Then you need to divide by the discard mortality to get the total number discarded rather than the dead discards. However, because you're doing this in a particular year for a particular fishery, they become constant and drop out, and so that's why all you really need is the selectivity and the numbers at age. I will ask for questions now, I guess, because that is basically the essential step, calculating of the selectivities and then this one, calculating the proportion of the discards greater than the minimum size.

DR. CADRIN: There's just one technical detail that really cancels out, but where you have fishing mortality, it really should be exploitation rate, since you're just taking the exploitation rate times the abundance to get the numbers caught, but, fortunately, whether it's exploitation rate or fishing mortality, it cancels out, but just for your documentation, I think that would help.

DR. ERRIGO: Thank you. Yes, that actually is true. I tend to use F, but yes, that would be exploitation rate, the way it's being used. These tables are just to show some of the -- Here are the selectivities. Size limit selectivity means the discard selectivity for fish less than the minimum size there, and then the bag limit selectivity is for selectivity of discards greater than the minimum

size. You'll see it's much, much smaller. Then these are the estimated numbers at age for 2012 from the update.

This is the same information for headboats, except for headboats we have more discard selectivities, but, in the future, this is going to look a lot like the MRIP, if I make the assumption on a headboat that they're going for the trip bag limit. Then it will look a lot more like this instead of like this.

From that, this is what I calculated the proportion of total discards. Blue is less than the minimum size, and red is greater than the minimum size of, in this case, thirteen inches. It was really small. Basically, when I did the analysis, it showed that increasing the bag limit would make a very negligible impact on the landings.

In conclusion, this method does allow the estimation of the proportion of discards due to hitting your bag limit, as opposed to being under the size limit, and it allows for analysis of a bag limit increase by basing assumptions on observed and reported data. This could also be used to analyze size limit decreases, because I can recalculate the selectivity patterns based on different size limits and then apply those to the discards and get the proportion of fish above and below. Therefore, I would know how many of the fish could be added to the catch as you decrease the size limit, and so we can use the same process, basically, and that's it. Any more questions?

DR. BARBIERI: Thank you, Mike E. First, opening up for general questions. I think you were very clear. You went step-by-step and broke it down into all the different components, and it was very easy to follow.

DR. ERRIGO: A lot easier than my method of actually coming to this final analysis. It took a lot of twists and turns and a lot of discussions. It was refined several times.

DR. CROSSON: As the age classes increase, as the population gets older and you're going further past the point at which you're sampling from, how does that affect your analysis?

DR. ERRIGO: As we accrue more data, it should be that the probability of being above thirteen ages for different age classes should increase, as the population grows older, and if there is a reason to assume that I shouldn't include certain years of the data, let's say when there was no minimum size, then I could truncate the dataset I used, but I would be very careful, because the older ages -- Age eleven-plus, I had to do some fancy ways of trying to get that probability, because there was a single individual that was observed that was older than eleven, or it was eleven, in the entire time series. Then ages nine and ten, there were very small amounts of observations, and so the more I truncate -- I would hopefully get more years of data before truncating.

DR. SCHUELLER: I guess I'm sitting here wondering, are we just reviewing this method, as in once we review it, it will be used for other species, or is this -- This is specific for black sea bass, and I guess I'm scratching my head on why we're even talking about bag limits, if it doesn't seem like there's any impact at this point, and increasing it is sort of -- It seems like a lot of paperwork the council might want to avoid if it's not really going to provide them with anything.

DR. ERRIGO: The hope here is, one, is hopefully, if they do consider a size limit decrease, that will make a significant impact on the landings, but, also, if -- We're hoping to be able to apply this

to other species, understanding that the way that other discard selectivities are calculated may be different, and so I have to look into that, but the hope is this methodology would apply to other species, and also the size limit analysis will become very important.

DR. BARBIERI: Just to supplement what Mike E. just said, everything that he's presenting is analytical in nature, and it's being presented as a result to the council. Ideally, it gets either a seal of approval or review and suggestions, and so the outcome of that analysis could be questioned. I mean, is that really the case and people are saying no, I don't believe that's the case and I disagree with that, and now, at least they see that we actually went through it step-by-step and reviewed, and we are confident that this producing something that's reasonable, and so I guess for that reason.

DR. SCHUELLER: One other comment is there's no uncertainty in any of this, and so I mean that might be something to think about as this moves forward. There is definitely uncertainty in mean sizes at age, and there is some sort of envelope around that, and so you can probably have an envelope around some of this stuff, to inform the council decisions a little more thoroughly.

DR. ERRIGO: Yes, I have been thinking about ways to incorporate that, when we do get estimates of numbers at age. I will be getting also median and then 95 percent confidence intervals from the MCB runs for brackets for that, and yes, in order to get the probabilities at age, I had to do some normal distribution, as they did there, with a mean and a standard deviation, and so there is uncertainty there that can be incorporated as well.

DR. BARBIERI: That was exactly on point of one of our action items to address, and so I'm glad you brought that up, Amy. It's a good recommendation to be evaluated as we go into the future.

DR. REICHERT: To that point, I think that should be a recommendation of the SSC, to include that, and make sure that council incorporates that in their decision, when they're going to be using this tool or the outcome of this tool.

DR. BARBIERI: Any other questions or comments or suggestions or recommendations?

DR. JOHNSON: I would just, at this point, reiterate what the industry folks and what Jeff brought up, is that understanding how the fishery is actually operating and how the discards are doing in the fishery is an important component to get for any species.

DR. BARBIERI: Thank you, Eric. I mean those little bullet statements do help us start building our report outline there. Given the caveats and the issues already highlighted, whether this can be considered the best scientific information available, and is it appropriate for use in managing fisheries resources? Any concerns from the committee?

DR. BOREMAN: What are you asking us to pass judgment on, the black sea bass analysis or the tool itself?

DR. BARBIERI: I think both. One is the methodology itself, as he described, but, in this case, I think it would be important for the council, is that outcome or result of this analysis, is presented to the council, for them to know that, as far as this analysis is concerned, we felt that it was best available science.

DR. SHAROV: I am just curious, but would this be a new finding for the council members that the bag limit is not a limitation currently?

DR. ERRIGO: Well, it was clear, from the initial, like just the statistics that were done, and even the original -- I also ran the original bag limit analysis that we had, where if a trip hit the bag limit, it would hit the next one, up to the reported discards on the trip. It still had a negligible impact. It was higher. The landings were higher than what this approach estimated, but it was still very, very small, because of the number of trips hitting the bag limit was so small.

Because of that, the council was able to go ahead with their amendment as it was, because, no matter how you did it for black sea bass, it didn't make a difference, but I am hoping to get an okay on the methodology if the council comes back and says we want to decrease the size limit. If they actually do want to have an impact in the fishery, right now, we have absolutely no way of doing that analysis. I guess we would do it the same way we do bag limit analysis, where if you hit the bag limit, you assume that you would -- I don't know. We can't do it that way, and so I don't know how we would do it without something like this. For black sea bass, it's good to see if you guys are okay with this methodology for that, but, also in general. If the methodology is sound, I can attempt to apply it to other species. I don't know what success I would have, but I can try.

DR. BARBIERI: Besides the black sea bass example or case study here, as far as the methodology is concerned, any concerns from the committee?

MR. COLLIER: One recommendation could potentially, since this is a new method and it's been just explored primarily for black sea bass, at the next use of it, have it come back to the SSC for a review again, to make sure you guys -- As people learn about it, it might be beneficial to review it again.

DR. BARBIERI: That's an excellent suggestion, yes.

DR. SCHUELLER: I like that suggestion, but then, also, I feel like if the concerns that are listed in the first bullet are addressed, it needs to come back for us to see what was done and have a look at that as well.

DR. BARBIERI: Okay, and that might even be able to come back then in October, depending on how much time you have between now and then, and for us to see, after you adjust the methodologies to account for those points. Okay. Thank you, Mike E. That completes that agenda item. Now, we move on, Steve, if you're ready, to the ABC Control Rule Revision Group Report.

DR. CADRIN: I will give some background while that is loading up. As some background, we had, of course, the deadline for annual catch limits in 2010, and the SSC and the council interacted to develop an ABC control rule. You're all very familiar with it. We've used it several times already this week, based on tiers of stock assessment characterization of uncertainty, stock status, and vulnerability.

As we were implementing that over the last few years, we ran into a couple of road bumps, where we were asked to revise or we were asked to reconsider. We ourselves found that the control rule wasn't always perfect, and so there was a workshop convened in 2014, and the PowerPoint that

was sent out yesterday has a quick review of that. I will skip that, because I think most people have read that, but I will refer you to that, and the basic conclusions of that workshop was that we don't yet have enough SEDAR assessments or benchmarks or updates since the ACLs in 2010 to evaluate performance comprehensively, but there is probably room for improvement in the control rule, and so really, what this subcommittee has done -- Last year, I made the mistake of leaving the meeting early, and our Chairman appointed me to this ABC control rule group, but it's something I really am interested in, and so I was happy to accept that, because it's something I was working on a related thing with anyway.

First, I would recognize the people on the subcommittee, John Boreman, Amy, Tracy, Eric, Carolyn, and Fred. Also, Luiz and John Carmichael have joined our conference calls, but, most specifically, I want to thank Mike Errigo for compiling most of the information that I'll be showing you today.

These are the attachments that are in our background documents, Attachments 29 to 35, and this is also an outline of what I will be presenting. It's a review of P* derivations. This was John Boreman's idea, to really just go back through, for the South Atlantic-managed stocks, what are the values of P*, what are the different scorings within the different dimensions, to compare them. I think that is pretty informative, when you look at that.

Then going back through the SEDAR assessments that we have to evaluate things like fishing mortalities that were produced by managing with an ABC and ACL and did they avoid overfishing and did they allow rebuilding. The subcommittee, if I can speak for the subcommittee, with some apologies for not getting this out in time for them to give a detailed review, but we kind of confirmed that we still have a limited number of SEDAR assessments since the ACLs were enacted, and so this is still -- These are preliminary, but what we should start to do is to develop the criteria for evaluation, so that as we build new assessments and new evaluations that we can start to monitor the performance statistics, to either say the ABC Control Rule is working exactly as we would like, it has room for improvement, and what direction of improvement we could do.

Really, that's the nature of this, is to give you a first look at the performance statistics that we've proposed, and they are fairly simple, but then, eventually, to expand these to include socioeconomic indicators, and so we have several of our socioeconomic experts on our subcommittee, and we also have, as referenced there, and this was a suggestion of John Boreman, again, that the Mid-Atlantic Fishery Management Council has some fishery performance reports from their advisory committees, and the Northeast Center has also done a groundfish fishery performance report that includes jobs, income, profits. There is a lot of socioeconomic data in that, and so, if we could go in that direction, that's really what we want feedback on at this point.

I think there's a little bit of a false advertisement in our briefing book that we are the ABC Control Rule Revision Subcommittee, and we're really not recommending revisions at this point. We're recommending ways to evaluate the performance, so that eventually we can consider revisions.

Again, I will try to pick up time, given the late hour of this. This is our control rule with the four dimensions and the different tiers, a perfect score of the 50th percentile, and then it goes down to a minimum score of the 10th percentile for the least informative, most uncertain, most vulnerable stocks. I am not going to go through the workshop. Really, again, there was a conclusion that we

didn't have enough assessments yet, but there were some cases, like blueline and wreckfish, where we did reconsider the ABC after a new assessment was done. Again, I will refer you to that report.

Here is our approach. It's to go through the P* derivations, the SEDAR stock assessments available, and I will refer you to this Attachment 29, which are the P*s, Attachments 29 and 30, I should say. Attachment 29 is John Boreman's summary that he had offered of Mike Errigo's spreadsheet, and those are the two attachments, and they essentially have twenty-nine ABC recommendations from this committee. This is the distribution of the P*s. You can see that we have a few of them, only two of them, at the low end, 15 percent. One is at 25 percent, and then most of them are in the 30 to 40 percent range, and so, by going through this tiering, most of our managed fisheries are getting about a 30 to 40th percentile P* range.

Then Mike even broke it down to what were the basis of those P* derivations, and so each of those columns is a different dimension, the assessment information, uncertainty characterization, stock status, and the vulnerability analysis, and the frequency, the relative frequency, of the tiers within them, and so this is for all of those twenty-nine ABC recommendations. You can see that, for the assessment information, that about half of them are Tier 1, about 40 percent are Tier 2, and then a few Tier 3 and 4, uncertainty high, and most of them are medium. Again, there's a few low and none. Stock status is a pretty wide split there, and, for our PSE risk analysis, it's a pretty even split between medium and high risk, and so those were the basis, but I think Mike also found some interesting time trends in this.

Probably the biggest block was between the first iterations that were done in 2009 and then 2010 to 2015, and one of the biggest breakpoints in deciding those different times is that the Monte Carlo Bootstrap was developed in 2010. That was designed to address the uncertainty characterization.

John had picked up on this in his evaluation of Mike's results as well, is that there was generally an improvement. Our P* scorings have become less conservative, shifting from lower numbered tiers over time. It may reflect that some of the research recommendations in the assessments have been addressed, and so better sampling and surveying, but, also, I think a positive management/science feedback.

In the precautionary approach literature, there is intended to be an incentive system in which uncertainties have wider uncertainty buffers, and that incentives the investment of more research to improve the science, and I think, with the Monte Carlo Bootstrap, that's a good example. The Beaufort Lab developed that in order to address this characterization of uncertainty, and so I think there's some positive feedback here. I don't think, necessarily, that we're getting more risky with our P*s. I think that there has been some positive feedback in this.

John had broken this out, and so that was just looking at all of our ABC recommendations as a whole, but he also looked at each of the iterations within a stock, and so black sea bass, for example, has had three different P* derivations, and the P* has gone up and up. In fact, only one of them has gone down, mutton snapper. One stayed the same, gag, and all of them have gone up, and so this wasn't -- This is happening both individually and across stocks. That was the first stage of this analysis.

The next one was to go to the SEDAR data and see how our ABC recommendations have been doing with respect, primarily, to ending overfishing, and so this was a review I had initially done for the Lowell Wakefield Symposium on data-limited stocks, and so I went back to the most recent six snapper grouper stocks to look at them, and it was an example of fisheries with a high recreational fishing component, which is why it selected those.

Those were done in that, but Mike showed me up. He sent out, to you, Attachment 31, which is a goldmine of information from SEDAR. It includes all of the South-Atlantic-managed stocks and their SEDAR histories. He categorized them as data rich, and that is that they -- It's an analytical assessment with estimated reference points and stock status. Unassessed and Catch-22s, which are somewhere in the middle, they have essentially unknown stock status. That really expanded mine, and I know you can't read that, but if you open that Attachment 31, I think you will see that it is a good reference.

With Mike's data, I was able to expand the review I did quite a bit. I threw in red snapper this week, just based on the assessment that we reviewed. I picked up four more snapper grouper, and I was able to include the three coastal migratory, and so it really expanded this quite a bit. Now, this is fourteen stocks with twenty-seven ABC recommendations, and so this is getting up there.

I won't spend a lot of time on any one stock, but this is the simple indicators that we have. Essentially, fishing mortality, compared to FMSY, the red line, and this is for red porgy, you can see that, for 2010 and 2011, the ABCs avoided overfishing, and so zero of two ABCs led to overfishing. However, they didn't allow rebuilding. That may have more to do with Mother Nature than management, but those are the performance statistics we're looking at, the frequency of overfishing and the direction of rebuilding, and we also want an indicator of yield, to try to get towards our optimum yield objectives, but, in this case, yield was deliberately set at much less than MSY, because of the rebuilding plan.

Vermilion snapper, again, two years of ABCs. Neither of them led to overfishing. It was not in a rebuilding plan, and the yield was around maximum sustainable yield. For blueline, this one is a little odd. This is the one that was identified at the workshop. It started as a Level 5, just catch information, and it was advanced to a Level 2 from the benchmark assessment, and so we really don't have bookends of an assessment method ABC recommendations and an update of the method. Here, we have two different methods. This is essentially evaluating the performance of our data-limited ABC with a retrospective data-rich assessment.

From that, two of the two data-poor ABCs led to overfishing. It was not in a rebuilding plan, and the 2010 yield was much greater than MSY. The 2011 yield was less than FMSY. Snowy grouper, one of three ABCs led to overfishing, but, when we consider the 2012, the yield was greater than the ACL. That's not necessarily a performance of the ABC control rule. That's more of a performance of the management system and the implementation of annual catch limits. If you remove that one year, there's two more years left, and neither of them led to overfishing. It allowed slight rebuilding, and yield was held to much less than MSY, because of rebuilding.

Georgia/North Carolina hogfish, this one is a little complicated, because, as we just talked about this morning, the ACL is for combined stock assessment units. There was a combined ABC. It led to overfishing, but that's because the 2012 yield of the combined stocks was greater than the ACL. Again, that's not necessarily an evaluation of the ABC control rule. The other hogfish

stock, the same exact thing. In combination, their yield was greater than ACL and led to overfishing, which makes sense.

For black sea bass, one of three years led to overfishing, but, that first year, the yield was much greater than the ACL. When the catch was limited to the ACL, neither year led to overfishing, and it did allow for a successful rebuilding, because the yield was held to much less than MSY. I just added red snapper this week. This one is not such good news. All five years of ABC recommendations led to overfishing. 2014, the yield was greater than the ACL. If you remove that one, we're still four out of four years led to overfishing, and it only allowed slight rebuilding.

These are moving into Mike's stocks now that he had done, and so I don't have the total yield plotted. I only have biomass and fishing mortality. For golden tilefish, we had one year that did not lead to overfishing and didn't need rebuilding. Yellowtail snapper, one year did not lead to overfishing. Wreckfish, the same thing. One year did not lead to overfishing.

Now moving into the coastal pelagics, these have a very good performance. None of the three ABCs for king mackerel led to overfishing, none of the two Spanish mackerel ABCs led to overfishing, and none of the two cobia ABCs led to overfishing.

Now, looking at these twenty-seven ABC recommendations for fourteen stocks, if we just take them in aggregate, eleven out of twenty-seven resulted in overfishing. That's about 41 percent, but, as I showed with several examples, snowy grouper, hogfish, black sea bass, red snapper, some of those overfishing results were because the catch exceeded the ACL. When we remove those from it and really just look at the ABC Control Rule performance, we go down to six of twenty-two ABC recommendations led to overfishing. That's 27 percent, and even though twenty-two or six is not a high sample size, we're in the neighborhood we expect to be, especially where most of our P*s are between 0.3 and 0.4. 27 percent is about what we should expect.

Coastal migratory pelagics, there was much less than expected frequency of overfishing. If we now just look at the snapper grouper stocks, we're six out of sixteen, 38 percent. Again, with this limited sample size, it's within the expected frequency. Rebuilding, we really need more time. It allowed for growth of some overfished stocks, sea bass and snowy grouper, but not others like red porgy.

That one will take more time, and so, really, this is just the conclusion slide. These results are preliminary. They're still insufficient for definitive evaluation, but I think it's time to start looking at these performance criteria as we can accumulate a sufficient number, and we should also expand them for more performance metrics, including socioeconomics. Of course, these assume that our stock assessments are correct for estimating fishing mortality and the reference points, but that's why we do the stock assessments. We would like to go toward management strategy evaluation, which actually could give us -- We could include some perception error, where our assessments are not always the correct reflection of the truth, and we could do this through management strategy evaluation as well.

My last slide is just what was in our briefing book for actions. I replaced "control rule revision" with "performance information", to consider them and provide recommendations. From my perspective, I don't think we're ready to make recommendations. Really, what we need to do is

see how the ABC Control Rule is performing, and there was no evidence, to me, that it's performing badly. It seems to be meeting the council's needs. Thank you, Mr. Chair.

DR. BARBIERI: Steve, thank you. I think this is much, much more, I think, than we expected, in terms of having this item discussed, and so we really appreciate all the time and effort you put into this, and your colleagues as well. I want to thank all the other folks who have participated in this working group, and, of course, Mike Errigo, who did a bunch of the heavy lifting here in pulling all this information together.

The first time around, things were not flowing very well, and it was good that more folks engaged into this process and actually got it moving. I think that this slide makes it clear that we are not at a point of action items, and so I'm going to just open the floor for any questions or discussions or suggestions from the committee.

DR. CROSSON: The performance metrics on the socioeconomics, Tracy and I were working the other day and talking about the SES, the socioecological systems analysis that we've been doing for some stocks, like golden crab and wreckfish, and so I'm going to be presenting just the beginnings of some thoughts on that at a conference next month, and so she and I met the other night and were talking about this aspect of just thinking about, again, how that information can inform ABC recommendations, especially when you have a lack of stock assessment or some other biological information. How can you look at what the markets are doing or sort of the social structure and get an idea of how the fishery may be functioning? Are dips in demand caused by a lack of fish or caused by a lack of demand for the fish? That analysis is going to take a while, and we're talking about reaching out to the SSC members from other councils on this, but this is something we are thinking about over the long term.

DR. BARBIERI: Scott, would it be good to give us an update on that in October, perhaps, or is that too soon?

DR. CROSSON: Perhaps. We'll see how things go. I am reminded that -- This conference is overseas, and I'm reminded how far ahead we are of the way a lot of other countries do things, when we have these ABC recommendations. You look at the way some of the European Union stuff goes down, and they take socioeconomic considerations in for hacking away at preventing more sustainable stock policies, I think, and I certainly don't want to go down that route, but I just think it's important that, when you have a lack of data, that you can look at economics and you can look at social aspects and get a better idea of what's going on, sometimes, than just looking at some of the simpler approaches that we've used.

DR. BARBIERI: Very well. Any other comments or questions from the committee?

DR. BOREMAN: I think we should think about recharging the committee. Steve offered up an idea, which I think is an excellent one, and that is to start thinking about performance metrics and how we would, in the future, evaluate how well we're doing, in terms of our ABC recommendations, and so at least continue to give it some thought. No specific timeline that I can see, but just keep the ball rolling and keep the process moving forward and think about this. It would be very useful for other councils, too. As a matter of a fact, it may be a great topic for the National SSC Workshop, but at least we should think about it here, and I'm going to be doing the same for the Mid.

DR. BARBIERI: Thank you, John. I mean I think that's a very good suggestion. If you think way back to 2008, I guess it was, that first National SSC Workshop, and it was really all about ABC control rules and ABC determinations, NS 1, and here were are, at a point where we have applied, not to the number that we would like to have, but we are at a place where we can see how much progress, and having that discussion at the next National SSC Workshop would be great and very, very helpful.

Your other suggestion, I agree that if we can set up periodically, you know every whatever many years, that we kind of go back and look at this performance and provide us some guidance on how we're going and doing it, providing this scientific advice to management. Any other questions or comments or suggestions? Thank you again, Steve, for volunteering for this assignment and for all the subcommittee members and staff who did a lot of work to provide a very, very good result.

Our next item, to make sure that we don't forget to get that done, would be our Chair and Vice Chair Elections. We should do this, so this gets addressed, as it should, and, basically, we do this every two years, the terms for Chair and Vice Chair.

Even though the Vice Chair role is sort of like a grooming, of sorts, in terms of the person learning all that comes with playing this role, we have, as part of our bylaws or standard operating procedures, and we had this outlined, that we will have elections for both posts every two years, at the end of the terms. With that, I think we are open for nominations for Chair and Vice Chair for the South Atlantic SSC Committee. I don't know how many people we have on the webinar, if any, but make sure that we collect their votes as well.

DR. BELCHER: I would like to nominate George Sedberry for Vice Chair.

DR. BARBIERI: We have a nomination for Vice Chair of George Sedberry.

DR. BELCHER: For the record, George is aware, since he's not in the room.

DR. SEDBERRY: I am aware.

DR. BARBIERI: Thank you, George.

MR. COLLIER: Does he agree?

DR. SEDBERRY: I do agree.

DR. BARBIERI: Wonderful.

DR. SCHUELLER: I will second that nomination.

DR. BARBIERI: The nomination is seconded. Any opposition from the -- I don't think we need to take a formal vote, or do we?

DR. BOREMAN: I will put on my constitutional consultant hat here. First of all, you develop a slate of nominees. If there are no more nominees, then you have one and you don't need to vote,

and so you can close -- Somebody can move to close the nomination process, or, if there's no other names offered, then it automatically closes with just the one name.

DR. BARBIERI: In this case, we started with the Vice Chair, but we do need a nomination for Chair as well, I believe.

DR. ERRIGO: I think we can handle each one separately, and so if you wanted to go through the Vice Chair and then Chair.

DR. BARBIERI: For the Vice Chair, any other nominations for Vice Chair? Seeing none, let's close the nomination process for Vice Chair, and congratulations, George Sedberry. You have been, by acclamation, nominated to be and appointed to be the Vice Chair of the South Atlantic SSC. With that completed, let's open the nomination process for Chair of the SSC.

MS. LANGE: I nominate Marcel.

DR. BARBIERI: We've got a nomination for Marcel Reichert. Thank you so much.

DR. BELCHER: I will second that.

DR. BARBIERI: It's seconded by Carolyn Belcher. Thank you. Any other nominations for Chair? Seeing none, congratulations, Dr. Reichert. You have been elected, by acclamation, as the Chair of the South Atlantic Council's Scientific and Statistical Committee.

DR. REICHERT: Thank you, Luiz, and I want to thank you for your hard work and service the last four years as Chair of the SSC, and so thank you for that role. After the adjournment, we have a very short, informal presentation. I also want to mention that this is the last SSC meeting for Steve Cadrin, and I also want to thank him for his years of contributions to the SSC. We will miss him, but we understand the decision not to apply again for the SSC, and so thank you, Steve, for all the work you've done for the SSC.

DR. CADRIN: It's been my pleasure, and it's been really good getting to know you all.

DR. REICHERT: With that, I think the two remaining agenda items are Public Comment, and so I'm going to ask if there is anyone from the public. Chairman Michelle Duval.

DR. DUVAL: Mr. Chairman, that look made it seem like I really don't count as a member of the public, and so I was going to have to give you some grief about that, but I just wanted to congratulate both you, Marcel, and Luiz on jobs well done, as the Chair and Vice Chair of the SSC over the past several years. I congratulate you, Marcel, on your ascending into the Chair role. Congratulations to Dr. Sedberry, who will be your sidekick for at least the next couple of years.

The work that you all do is extremely important. You all had very difficult conversations and really heavy topics to discuss over the past several days, and so I, for one, appreciate all the effort that each and every one of you put into this. I know I can empathize completely with the gigabytes of reading material that you all receive for these meetings. It's akin to what we receive on a quarterly basis for the council meetings, and so thank you.

Also, thank you to Dr. Cadrin. We will definitely miss you, and who knows? Hopefully we may see your face around at some other point in another meeting, and so I very much appreciate all of your efforts as well, and so thank you, Mr. Chairman. I just wanted to congratulate you all on all of your efforts over the past several days. Thank you very much.

DR. REICHERT: Thank you. Are there any other members of the public who wish to make public comments? Seeing none, I briefly want to mention to the point that Michelle just mentioned. John Carmichael, Mike Errigo, George Sedberry, myself, and a couple of other people had met with some key council members during the last council meeting to talk about the briefing book, the size of the briefing book. I had planned to update the committee on that meeting, but, because of the late time, I will do that next time.

You may have seen some results already, and I want to commend Myra, who provided some of the documentation that we unfortunately haven't had time to get to, but you may have noticed that there were some summaries specifically for the SSC, and that was one of the results of the conversation we had, and hopefully we'll see a little bit more of that, to just help us guide through the large volume of documentation that we have in our briefing book.

I hope to work with all of you to see how we can potentially streamline that, with council staff, a little further, and so I really appreciate the efforts of council staff and council members in working with us on that. Potentially, we will also address the timeliness of some of the documents that we are getting in preparation of the SSC meeting, and so hopefully, collectively, we can work on that and further increase the efficiency of our meetings.

With that, the other remaining agenda item is the Report and Recommendations Review. As we have done in previous meetings, we've seen some of the notes that Chip has made on the screen. Thank you for filling that role, Chip. We really appreciate that. We have looked at that during the meeting, and so I propose that I will work with Chip on getting those notes from him and adjust them and send them out to the committee for a review, rather than go through them right now, given the late hour, unless anyone has any comments to the notes that we've already seen.

DR. SCHUELLER: I just wondered -- One of the things that I had to come back to were the terms of reference for blueline, related to the climate change and ecosystem components, and I wasn't sure if we should chat about that quickly, while we're still sitting here.

DR. REICHERT: We certainly can.

DR. SCHUELLER: Not to belabor the meeting, but --

DR. REICHERT: It's 2:30. We still have some time, and so let's bring those up to the screen.

DR. BARBIERI: I just wanted to say that, offline, Mike E. and Julia were discussing some of these issues, and Julia put together some suggestions, and she compiled --

MS. BYRD: What I did was just -- Since it seems like the ecosystem TOR is typically used in the Gulf and in the SAW/SARC, and so I just pulled some language from the Gulf and from a recent SAW/SARC terms of reference, so you all can look at that, and then maybe we can adjust it, as necessary, and then that could be added to the terms of reference.

DR. BOREMAN: I looked at those, and I may be a little biased, but I prefer the wording from the SAW/SARC. It's a little more flexible, in terms of allowing the -- It's more relevant to the ABC, too. It ties it into the ABC, but it gives some flexibility there. I can live with either one, but I prefer the SAW/SARC one.

MS. BYRD: Okay, and so if you look at the main screen, so you all can all see the language, and if I need to make it a little bigger, just let me know. The one on top is from the Gulf of Mexico, and this was pulled from SEDAR 42, which was the red grouper assessment, and what they did was they added one TOR to the data workshop and a separate TOR to the assessment workshop. I think the SAW/SARC -- It seems like there's just one set of TORs for the whole assessment, and so this one is from witch flounder, from the 2016 TORs, and so you all can look over that language and see if you prefer one or the other or want to kind of pull from both or make any adjustments.

MS. LANGE: I agree with John. I think the SAW/SARC one is more flexible, and, if we need to, we can break that into two. I don't know if we need to have a data workshop and an assessment workshop one, but I prefer that one.

DR. CADRIN: The witch flounder example is not the generic one. I like that one, but there are more things that can be added. Where it has recruitment, growth, and natural mortality, I would add geographic distribution.

DR. REICHERT: Any other comments? I agree that I like the language that's currently on the screen, and I agree that we could potentially look and see if we need to make some slight adjustments if different language is needed for the data workshop and the assessment workshop.

DR. CADRIN: The other aspect, again, is I think there is a more generic one that I can look for in the Northeast, but this is entirely abiotic effects. This is climate change. It does say environmental factors, and so things like multispecies effects, predator/prey. I guess you could put in environmental factors, but maybe we could be more explicit to say effects of abiotic and biotic environmental factors, which would include species interactions.

DR. REICHERT: So should we move "environmental" to after "abiotic and biotic factors", because, for instance, predator/prey interactions are not necessarily environmental factors, or that may need wordsmithing there.

DR. BUCKEL: I think you could get rid of the word "environmental" and "climate change", and then you could have a -- It's just abiotic and biotic factors, and then, for example, climate change and predator/prey interactions, and that leaves it flexible.

DR. REICHERT: Julia, a quick question. Is this language that we need to finalize right now, or can this be part of the SSC report and then provide some feedback to you, relative to the timeline for the workshops?

MS. BYRD: The final TORs will need to go to the council at their June meeting, and so you all can -- I'm glad that we have at least looked at that and have close to the final language, but you all can wordsmith and then include the final in the report. That would be fine on timing.

Also, I will just have everyone note that I worked with Erik Williams and we put together some draft changes to like the red grouper schedule, and so there are actually weeks for webinars and things like that. For blueline tilefish, we put in a week for an assessment workshop and kind of jiggered the assessment webinars around as well, and so maybe that can -- I know we're looking for a few more volunteers for both of those, and so maybe those can be shared with the SSC members and then kind of final volunteers could be included in the SSC's report as well.

DR. REICHERT: Certainly.

MR. CARMICHAEL: We definitely would like final volunteers before the council meeting in June. If you have to tell me at the last minute, right before they go in and make appointments, that will be fine.

MS. BYRD: One thing I will mention is Scott Crosson volunteered to be the Chair of the Blueline Tilefish Review, and so I know that was kind of offline, and so I just wanted to let the group know that and say thank you to Scott.

DR. REICHERT: Thanks, Scott.

MR. CARMICHAEL: We wanted to circle back around to the stock prioritization, in the sense of what is the SSC's thoughts on how we may fill in a number of those criteria which require expert judgment. One of the biggest ones, of course, is, for example, the recreational importance. We just wondered about the idea of doing something similar as we've done with ORCS, where we've brought in some advisors from the fisheries to help inform the SSC on those types of values. Would the SSC maybe be interested in perhaps a half-day workshop type of thing with some AP members and maybe some council members who are heavily involved in the fisheries, to try and fill in reasonable values, at least starting places, for more of those things which are judgment-based?

DR. REICHERT: I am certainly open to that idea. Are you thinking about maybe prior to the October SSC or is that too early?

MR. CARMICHAEL: I am thinking at the October SSC, so it doesn't require extra travel. It may not be the pace that NMFS has hoped this would progress on, from Headquarters, but I think we're as far along, if not farther, than most of the other regions, and so I'm pretty pleased with that, and I think that would be a good time to do it, because I know everyone's schedules are stretched all the time.

DR. REICHERT: I suggest that I will work with you and Mike and council staff and George to set that up and inform the SSC. Anything else for the good of the order? Seeing none, then I suggest we adjourn, and we are adjourned. Thank you, all.

(Whereupon, the meeting was adjourned on May 5, 2016.)

Certified By: _____ Date: _____

Transcribed By:
Amanda Thomas
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