SOUTH ATLANTIC FISHERY MANAGEMENT COUNCIL

SCIENTIFIC AND STATISTICAL COMMITTEE

Crowne Plaza Hotel
North Charleston, SC

October 28-30, 2014

SUMMARY MINUTES

SSC Committee

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Dr. Tracy Yandle
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Dr. Kari MacLauchlin
Dr. Julie Neer

Observers/Participants:

Mike Murphy
Dr. John Walter
Dave Newman
Rusty Hudson
Dr. Nick Farmer
Dr. Joe O'Hop
Dr. Lora Clarke

Other Attendees Attached

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The Science and Statistical Committee of the South Atlantic Fishery Management Council convened in the Crowne Plaza Hotel, North Charleston, South Carolina, Tuesday afternoon, October 28, 2014, and was called to order at 1:00 o'clock p.m. by Chairman Luiz Barbieri.

DR. BARBIERI: Good afternoon, everybody, and welcome to the October 2014 SSC meeting. I would like to draw your attention to our agenda; Item 1 being introductions. I know that we have already gone with some of those introductions at our ABC Control Rule Workshop; but since we are having a webinar broadcast as well and we have potential different attendees, I would like to go over the introductions again and voice recognition.

DR. GRIMES: Church Grimes; SSC.

DR. VAUGHAN: Doug Vaughn; SSC.

DR. SEDBERRY: George Sedberry; NOAA Sanctuaries.

DR. BUCKEL: Jeff Buckel; North Carolina State University.

DR. CROSSON: Scott Crosson; Southeast Fisheries Science Center.

DR. ERRIGO: Mike Errigo; South Atlantic Council staff.

MR. CARMICHAEL: John Carmichael; 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.

DR. LARKIN: Sherry Larkin; University of Florida.

DR. SMITH: Will Smith; North Carolina Division of Marine Fisheries.

DR. SCHUELLER: Amy Schueller; Southeast Fisheries Science Center, Beaufort Lab.

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

DR. YANDLE: Tracy Yandle; Emory University.

DR. BERKSON: Jim Berkson; NMFS Southeast Fisheries Science Center.

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

DR. BARBIERI: The first order of business after introductions is approval of our agenda. I would trust everybody has had a chance to review our agenda. We are going to have some adjustments and modifications of the agenda at some point;, but before we get there, I would like

to hear from the committee whether there are any comments, questions, or concerns regarding the agenda. John, is there anything that you want to bring up regarding the agenda?

MR. CARMICHAEL: No.

DR. BARBIERI: Well, I want to bring up just a couple of very small issues for this afternoon and then some more relevant ones for tomorrow. Item Number 5,; update on the 2015 National SSC workshop; I know that the update report I am listed as the person giving that update; but to be honest with you I have not been able to participate in most if not all of the conference calls.

I am aware of the agenda, but I feel that having somebody better informed give that report would be better. I asked John Boreman, since he has been more participatory in that, to give that report when we get to that agenda item. John graciously agreed to do that. Then one other thing that is not so small is that tomorrow we are supposed to have three assessments.

We have two benchmarks, the hogfish assessment that was led by FWC and then the king mackerel assessment review. This is the benchmark king mackerel assessment conducted by the Science Center. Now the mutton snapper assessment review, most likely if not certainly, we will not be able to present tomorrow. As you know, this has been the gift that keeps on giving. It has been sort of my nightmare; this issue of having this assessment completed.

Mutton snapper, like some of the other more Florida Keys specific species, has had a number of issues associated with the MRIP estimates, in the conversion of MRFSS to MRIP, that are not necessarily one-size-fits-all and plain vanilla. Our assessment team, the FWC/FWI Assessment Team has been working on this for a while.

We finally got to the point where we have an assessment completed. We have a base model chosen. Joe O'Hop, who led that assessment, developed actually a calibration of conversion methodology in association with the MRIP staff that is fairly different than what the Center traditionally uses for other stocks and MRIP applies across the board, because in the Florida Keys you have some particular situations that do not apply.

We have a model that converts; it is a base model. We have all the sensitivities run. We don't have any significant retrospective patterns. We have good residual patterns, fairly good fit to the model, and the MCMCs are all done, and we have some stochastic bootstrapping. We have all of that stuff done, but the report is not ready.

It wasn't ready in time for distribution to the committee. We are going to just have to postpone. This is something that we were supposed to see back in April and we had to postpone. Some of those issues were not completely resolved until recently. My apologies again for having another hole there in the agenda; but we are going to have to make do and see that assessment and when all of you have an assessment report that is complete and can provide you all the information needed for appropriate review.

This mutton snapper is an update assessment, so it did not go to CIE review. It is very important that you get as much documentation as possible, because the only review that this assessment is going to have is really the SSC review. With that, I think those are the only agenda items that I can think of that are different than what you have in front of you.

With those modifications, any concerns with us approving the agenda as presented? No comments or concerns, so the agenda is approved given the modifications as outlined previously. Then we have to have approval of our April 2014 meeting minutes. Hopefully, you folks had a chance to go through and make sure that those meeting minutes really capture the discussions that we had back in April. Are there any comments, questions, or concerns?

DR. REICHERT: I have a couple of minor edits. I can forward those to staff; they are basically correction of spelling of names. I will just forward them to staff.

DR. BARBIERI: Okay, thank you, Marcel. Are there any other comments, questions or concerns regarding the April, 2014 meeting minutes? If not, we consider that the meeting minutes stand approved given the modifications that are going to be provided by Marcel. This leads us to our next agenda item, number 2, public comment. Members of the public; we have Rusty Hudson.

MR. HUDSON: Thank you, Mr. Chairman and the SSC, for allowing the oral public comment and we have submitted a written comment a week ago. I hope that everybody was able to read that. Looking at the agenda items, we kind of just started at the top of the things that we had some comments about the SEDAR activities. As you can see the headboat logbook index problems that has caused us one year of delay, we feel is a little excessive as a remedy.

The in-house QA/QC peer review; it will be interesting to see how that evolves, but there is twofold to the bias. One, of course, through the voluntary system, the historic period that was shared during SEDAR 25 in 2011 and a remedy was found and utilized in SEDAR 25 to wait to inflate the errors of CVs in order to deal with that historic period.

The bias on reporting and recall, having personally participated in some of that period would be usually types of things like on the way in we would wind up filling out the log, not counting the fish as they were actually being caught. On vessels with a lot of people, that was a lot of stringer fish. It was like a ballpark estimation.

You may have had better recall with a bigger fish being that a lot of the hook sizes went down during the seventies and the eighties and that changed the nature of the catch. With that said, we also have a bias recall that we brought up at that time that was with regards to the NOAA port samplers in Florida. Since then we've also learned about South Carolina. That is something that was treated in-house; we never got to understand exactly what was done to remedy that problem.

We think that there needs to be a little bit more transparency with that solution. With that said, also through SEDAR 4, on the last day we also discovered some issues with the MRFSS/MRIP raw data. Four years in particular are red flagged in an analysis that we shared with the leader and the recreational working group. But because we ran out of time at the data workshop, there is not real solution forthcoming except that we did ask for some average weights for a year so that we were able to kind of do a little further analysis and set our minds about what the problem is. Sometimes it is a misidentification issue on the part of port samplers.

I will leave that at that; because we do feel things like golden tilefish, when I looked over that particular assessment, and it is only an update that was scheduled, there are only 381 intercepts

that were involved in that whole time period for the recreational. We really think that is the type of assessment update that can go forward without delay.

I feel bad for the blueline tile, because I've already noted some of the problems with that database that needs to be reexamined; and yet that is going to be delayed another year or two as well as red porgy, et cetera. I felt like there is a faster solution, because generally as I had been coached the last couple of years, we can have the data available as early as May; so that we don't have to wait until August, we can do something in June as an earlier start date.

The king mackerel assessment review; having participated in that for a lot of years now, the big question was recruitment. We were able to put together – thank you, Dr. Barile, for putting it together with regard to the TIP data, 2010 through 2014, but it mostly depended on the Georgia/Florida area, because there is a little bit of confidentiality scenarios with regards to the Carolinas and other situations.

That is off of landings. We have a lot of testimonial that was provided from king mackerel fishermen and other fishermen in other fisheries nearshore that are seeing a lot of the one year olds that can't be landed, but they are part of the cohorts. There has been a plentiful mix of them with the kings and the Spanish together.

With that said; I'm hoping that will help alleviate some of the concerns that came up here in the review workshop. Snapper Grouper Regulatory Amendment 16; the changes that reduced the fleet down to the 32 endorsement holders, the allowable pots down from maximums of 100 down to 35, the trip limit at 1,000 pounds gutted, the mandate to remove the black sea bass pots at the end of the fishing trip, which is a big deal – it is kind of like our Spanish mackerel nets; that when we worked that out with the whale people many years ago, it was a type of thing that you are virtually tending your gear now, which makes it less of a problem.

The only requirement that is if you see a whale within a mile of your Spanish mackerel gear, you pick up and you move. Well, with the black sea bass pot you have a lot different breaking strengths and stuff for the equipment that is used. I think that analysis hopefully can provide the momentum to be able to allow these people to fish their traditional productive period.

We've achieved a big increase in the black sea bass; and thank you to this body for helping make some of that happen and to go forward with it. But they can't fish, so this is the second season we're losing, and we're sure hoping that by the time we get to November 1, 2015, that this has gone away.

Bag limit analysis; we approve and support the recommended adjustments to the ABCs, ACLs, OY for both gag grouper and wreckfish. We still have a little problem with the allocation of 5 percent of wreckfish over to the recreational component. Like Dr. Crabtree had said, there have been really no intercepts from the recreational component; and the only place you would ever see that would be in south Florida.

But again back to gag; we are of the feeling that changing the bag limit to two gags, particularly in the midst of this rebuilding plan, might not be a real good thing to do. If we could stay with one; that might be a better thing to do until we can get to the next assessment. Thank you very much for taking our comments.

DR. BARBIERI: Are there any other members of the public who would like to make public comment? Seeing none; we are going to proceed. Before we go into the Agenda Item Number 3, it was my oversight during introductions that I did not introduce or give Jim an opportunity to introduce – we have a visitor, one of Jim's graduate students who came and is attending. As you know, Jim has done this in the past.

DR. BERKSON: Katyana Vert-pris is a doctoral student working with me at University of Florida. Katyana has an amazing background. She grew up splitting her time within the island of Martinique in France. Katyana has three Masters Degrees, including her most recent one from the University of Washington, working with Ray Hilborn and Andre Punt.

She has a paper that came out last year in the proceedings of the National Academy of Science; the paper is very well known and very highly cited. She already has an extremely strong background for just having completed her Masters a year ago. Anyway, I hope you will take the time to introduce yourself to her. She is working on a very fascinating project for her PhD, and I hope you will ask her about it. Thanks.

DR. BARBIERI: Thank you, Jim. Welcome to the meeting, Katyana, glad to have you here. Now back to our Agenda Item Number 3; SEDAR activities; I think John Carmichael is going to provide us a number of updates.

MR. CARMICHAEL: Our regular process is updating you on what has been going on in SEDAR. There was a Steering Committee meeting recently; addressed the schedule and progress on various assessments, which are underway, we'll fill you in on. First to talk about the Steering Committee; a couple of things that were addressed, they have been talking for a while about a way of improving the assessment workshop process, the phase in which assessments are actually conducted.

If you have been around the table long enough to remember, we used to do them through a workshop and then we started adding webinars. We've done quite a few of them solely through webinars. It has taken a lot of webinars to get through those and many, many webinars sometimes scheduled every two weeks over a three- or four-month period. It has been a lot.

It has been excessive. The reality is it has become counterproductive to the idea of being transparent and inclusive, because fishermen in particular are asking, well, which one of those is important I should try to be at, because I can't be at all of them. For a while now the Steering Committee has been trying to come up with an alternative way to make this function better.

One of the things looked to was very much the IPT process, which the council has used to develop FMPs. It is a way of getting people with technical expertise together and let them do that technical work and then get it to particular points at which you make it available for broader review. The National Standard Guidelines, which came out and addressed transparency, were very helpful and they backed off some from the idea that every piece of technical work, every technical decision has to be made in the public forum. They acknowledged that wasn't practical.

Well, that was certainly what we had experienced, because assessment work flows by its own schedule based on when problems arise and when challenges have to be solved. It is not really

conducive to the apriority every two-week scheduling. That was really just making the whole process untenable. We have been working on this solution.

What the Steering Committee has now approved is an approach where the webinars will be reduced to three. Each one has specific milestones which will be addressed at that webinar. Leading up to those webinars, the technical panel, the analytical group, analytical team as well as the SSC and other technical representatives that the council appoints will be able to interact over e-mail or conference call or what have you, as they need to, to solve the technical issues and reach each of those milestones; so really doing the kind of pre-decisional technical work that is necessary to get to those milestones.

We're hoping that by freeing that up a little bit and allowing the analysts to deal with problems as they arise rather than say, well, I've got to wait until the next webinar so I can talk about this with everyone; they will be able to reach those milestones much more efficiently. Hopefully, once this starts working, we'll be able to shorten the schedule a bit and get it back to the good old days. When SEDAR started, we did a whole assessment process in six months. Now it is stretched out to a year or more. It is just really slowing down progress.

That was a big part of the Steering Committee's discussion, and they have approved that. We are going to roll it out with the Gulf red grouper assessment that is somewhat underway. They are going to put this process in place, see how it works. The Steering Committee hasn't discussed as far as other assessments; when it will be done next.

We think the next ones to get planned and scheduled will follow this approach; and SEDAR 41, whether it follows this or the other, is yet to be determined, I suppose at this point; but we'll start working on the schedule. That is the next issue we'll talk about. The other Steering Committee thing, which is of interest to you guys, is another way of improving the process.

We talked about having procedures workshops; one devoted to data, developing data best practices; and then the next one will be devoted to assessment, developing assessment best practices. The idea is to look into some of the common decisions that are made across all assessments and see if there can't be some guidelines put out and some instructions put out; so that you don't have to spend so much time at every data workshop covering the same ground, having the same discussions.

There are certain holes in our datasets that are common across all assessments; there are certain issues with our datasets common across all assessments; and we would like to get some agreed to decisions apriority, so that those things can just be handled and folks can go into the data workshop more prepared to talk about the unique and interesting things of a particular set of data related to a stock and not so much how are you going to deal with the common issues that seem to dominate so much of their discussions.

We'll be looking for an SSC representative to be on the organizing committee, and then we'll probably be looking for more SSC representatives to take part in the actual workshop. The workshop, part of it is going to be looking at what are the common decisions that have been made, what are the common threads throughout assessments, and then what are the recommendations to solve this?

SEDAR 41, near and dear to everyone's heart, is South Atlantic red snapper and gray triggerfish. As Rusty alluded to, an issue was raised with the headboat data. It had to deal with the reporting that went on back in the earliest years of the program and when the reports were filed. There are a series of comments provided by fishermen.

It was raised in SEDAR 25 and a solution was proposed then. It has been brought up here again; and concerns raised on the call to consider whether or not SEDAR 25 with black sea bass doesn't cover the same area as red snapper, and a lot of the fishermen that provided the comments about how they reported were in the core sort of red snapper area. I think that raised a little bit more of alarm.

But the bottom line was – the indication is that the data may not be as accurate as thought. Things could have been done the day, the end of the day, the week. In some cases they mentioned sitting down at the end of the month and doing their reporting through the headboat survey. The bottom line is the SEDAR 41 panel felt this was a pretty serious issue.

It affects the indices, it affects the catch, it affects the historic catch pre-MRIP and pre-headboat, because that is used as part of the algorithm by which past catch was estimated. It really affects the whole assessment. The recommendation was made to solve this problem before proceeding with the assessment. The Science Center is in charge of that.

Bonnie reported to the council and again to the Steering Committee on their efforts. They've looked at some different ways of looking at it. There are concerns with recall now as you try to ask people what you did 30 years ago. If you are concerned about recall from 30 days, you are far more concerned about recall from 30 years.

My understanding is the Science Center is going to be trying to get a good handle on what sort of QA/QC was done at the time as well as mining the dataset itself trying to identify trends or something or find is there a point when you noticed changes in reporting or what have you. I think we will just wait and hear what comes out of that.

My understanding now is it is in the hands of the Science Center. When the council realized that this was going to be delayed, the schedule was such that the assessment needed to stay on track to impact the 2015 red snapper fishery. Any delay means that doesn't happen, so the council recommended that if it is going to be delayed, that you just delay it a full year so that you can bring in additional years of data; so bring in 2014 data.

That means an additional year of the new survey, which is going to be very helpful, because I think they only had three or four years at this point; so another year is a lot more data. We know that the MARMAP survey has been catching them; and so an additional year of their program getting some more fits is going to be helpful. Another year of what fishery data we have will be helpful as well.

The council's recommendation, and the Steering Committee supported it, was if it is going to be delayed, you need to bring in the extra data. Otherwise they are taking action on what is really pretty outdated assessment. If you've got 2013 data and you are taking action in 2016; that is a problem. That is SEDAR 41. You will be briefed on that as it develops.

Certainly, when we hear what comes out of the headboat survey, you will be briefed on that and get the schedule worked out. We'll have to look at who was appointed before and who can still make the new schedule and all that good stuff for the next meetings. The other effect on this is that there were a number of other assessments planned for 2015.

They are also delayed partially due to staff turnover at the Science Center Lab and also due to having to deal with the headboat data issues. There is kind of a ripple effect on the South Atlantic assessments, which you can see in Table 1 where things stand now and when we expect to get the assessments done. It has certainly put a big wrinkle on what you will see in 2015.

The other issue they addressed is Florida conducting assessments of black grouper, yellowtail snapper, and Goliath. The plan there is to transition these to stock synthesis and to consider doing it through the standard assessment; as it is called now a standard process that SEDAR has. The ruling there is a standard is a little bit more flexibility than an update.

You can consider things such as changing model packages as long as you don't change the model type; by which I mean if you have a catch-at-age model configured in one package, you can move it to a catch-at-age model in another package through a standard. Now that creates a bit of a gray area when you have to consider is there so many decisions being made that you would be more comfortable with a benchmark, with that full evaluation, with that independent peer review panel.

The deciding point in that process is the SSC; and that is per the SEDAR guidelines. Because you make the recommendations and you are integrally involved in the whole thing, the SEDAR Steering Committee Policy is that it is up to cooperators with SSC to decide when the gray area is too gray and you think it should be a benchmark.

At the Steering Committee – this was the second week in October, so it wasn't that long ago – this plan was put forth; and it was suggested that the process expects that those who do the assessment, in this case FWCC, would for the SSC indicate what type of things they believe are necessary to go to this different model package; so that you can then judge it and decide if you are comfortable doing that through standard or you want to do benchmark.

We put it on there to make sure we talk about the process and how it works. Obviously as Luiz said, they have had a lot of other assessments going on down there and are not able to do that justice, to figure out what exactly is all required to go from the other models in the stock synthesis. I expect in April we'll have good proposals form FWC for you guys to weigh and consider what approach is the best way for you.

DR. BARBIERI: John, just one point of clarification; for Goliath grouper we are not thinking about switching that to SS, because we are going to be using the same catch-free model; the Porch et al that Clay Porch developed. The catch-free model has been used in the past for Goliath, including the last benchmark assessment.

But there are some cleanup issues, to use Clay's terms, clean up issues of the code and some adjustments, because techniques have been developing. The model just needs to be updated to be in better shape. That transition needs to be made, John.

MR. CARMICHAEL: Thanks for that clarification, Luiz. Then the final item is the council's priorities for assessments, which are shown in this table, resulting what came out of the Steering Committee at their last meeting. Mutton snapper, which we will get I guess in April now; hogfish and king mackerel, which are completed and you will review them tomorrow.

Red snapper and gray triggerfish pushed back a year; red grouper update, 2014 data and hopefully it will be done in the fall of 2015 so that you get it in April 2016. Then the blueline tilefish as an update, you get that April 2017, with it being done in January unless we decide to have an earlier meeting so the council sees it in March.

Then the black grouper with the standard or benchmark question; golden tilefish the desire is for an update. The timing is not exactly completely figured out on all of these. Also, the council is looking at scamp and gray trigger, vermilion and greater amberjack updates, and then yellowtail snapper from Florida coming along. Now is a chance if people have some feelings on some stocks they would like to see added to the priorities, here is the opportunity.

DR. REICHERT: Just a clarification; you said scamp and gray trigger; that is scamp and gray snapper for those of us who are listening in.

DR. BARBIERI: As we look at our list of action items here in our overview document; our actions are to review black grouper, Goliath grouper, yellowtail snapper proposals and terms of reference; recommend whether these assessments are conducted through the standard or a benchmark process.

As John explained, it wouldn't be meaningful for me to bring anything here without us having some of the analysts kind of dig through the data and the main inputs and discuss where they want to go with this and develop something better documented for you to review. I didn't bring anything since that was just three weeks ago.

I am working with staff and we will bring something in April for your review and recommendations. The next action item is a review of those assessment priorities – that is the Table 1 that John just went through – in terms of the assessments that the Steering Committee has approved and identified as priorities for this next cycle of assessments. Then some of their future priorities, which I understand are listed in priority lists; from top to bottom, the second part of the table. Are there comments or questions from the committee?

DR. REICHERT: John, I remember red porgy being in there; was that dropped?

MR. CARMICHAEL: Yes; red porgy was pushed back even further.

DR. BARBIERI: Red porgy, the way I remember, it didn't meet rebuilding timelines, so it is right now at a catch level that was based on 75 percent of MSY yield at –

MR. CARMICHAEL: Yes; that is right, it was lagging on its rebuilding, but overfishing had been ceased; stopped.

DR. BERKSON: Has there been any discussion of a data-poor stock SEDAR or procedural workshop where a lot of methods would be reviewed. That could be part of that assessment procedural workshop that you talked about, but specifically for data-poor stocks.

MR. CARMICHAEL: There hasn't been recently at the South Atlantic. The Gulf is going to look at some red drum and some other stocks to be named next year applying data-poor methods. There are some underway in the Caribbean where data poor is the only data you have. There are some things being done there and they are making a little progress.

As far as identifying a group of stocks in the South Atlantic, it hasn't been done. You guys could recommend something for the council to consider in terms of taking an available slot and doing some data stocks, or maybe if you think it is a higher priority than some of those that are listed for the future now.

MR. HARTIG: It hasn't come up formally. The council continues to talk about it behind the scenes, about doing some kind of way to move some of these species forward from the data-poor section through exactly what you said, Jim, a procedural workshop where you have X amount of species that you review. We are very interested in doing that.

It is just, my gosh, we are just so trapped in this SEDAR schedule that we have now; and the importance of some of these species is driving the conversations about how we move ahead with this. I think some conversations we had last night; I think possibly the council may actually try and do something to move this ahead to try and come bring this together.

This is just in the talking stages still, but I think the council is interested in moving ahead with this. I think maybe we could develop a process on how we move ahead with it and not wait on some of the – the Center in particular where they are strapped doing what we have now. Some other mechanism I think; and probably talking to you and others would be helpful on how we do this.

DR. BERKSON: I think that is a really good idea. I think we could make a lot of progress in a relatively short amount of time; definitely a shorter amount of time than a traditional SEDAR. A procedural workshop would be a great avenue to review these methods, especially now that we have something like Tool Kit and other advanced ways of doing these things relatively quickly.

DR. BARBIERI: Michael is just capturing that suggestion, Jim; and we are going to have a chance to flesh that out a little more. But basically that is being captured as something that is going to be an SSC recommendation to the council.

MR. CARMICHAEL: I think it has a better chance of success if it is recommended as a South Atlantic Workshop. Procedural workshops are really geared towards things across all the assessments. When you talk about the assessments procedural workshop, it is going to look at primarily the big catch-at-age assessments, which take most of the time, and how to refine that process.

I think doing like the Caribbean and like the Gulf are looking at doing, which is taking a slot and devoting it to South Atlantic data-poor stocks, and then to get out of SEDAR; you know, finding a way to apply the South Atlantic's assessment process to a group of data-poor stocks.

Procedural workshops certainly has the SEDAR connotation, especially given it is mention in this text.

DR. BERKSON: That sounds like a better strategy. I don't care what you call it as long as it gets done.

DR. BARBIERI: Okay, are there any other questions or comments for John regarding the South Atlantic Council's assessment priorities and the SEDAR schedule proposed? As part of our action items, we are supposed to review this priority list and provide some feedback to the council on whether we approve it or we suggest any modifications or any other comments or whether we accept it as presented.

DR. SCHUELLER: I guess I would say I am happy to see some species on there; for instance, the scamp that hasn't been on there in the past and need to be. Then I have a question. If we take a recommendation, this goes where? It goes to the council and then they take that under consideration when their recommendation goes to the SEDAR Steering Committee; is that correct?

MR. CARMICHAEL: Yes, that is the process. Each cooperator. as we call them, which are the councils and the commissions and HMS, set their priorities, and then they come to the Steering Committee to figure out balance of workload and who can do what and what the Science Center and others can accommodate.

Each cooperator is able to make its own process for how it involves its technical and fishery advisors in the prioritization. In our case every time the SSC meets, we show you where things stand and your recommendations on what the priorities should be go to the council. The council has a SEDAR Committee that meets every meeting.

In December we will brief them on whatever comments you have on this schedule. The Steering Committee, when they meet in the spring, will look at the 2016 priorities, and then we want to finalize those come fall; to sort of say this is what is on the horizon. They would be looking for any major issues with the 2015 schedule that they have to resolve and 2016 schedule they have to resolve when they meet in April and then look ahead to the future. You report to the council officially in this capacity.

DR. BARBIERI: If no additional questions or comments from the SSC; I think we will then accept the assessment priorities and schedule as presented. Is there any disagreement with that statement?

MR. CARMICHAEL: One other bit that should have been there I guess, Luiz, is who would be interested in being a representative for the Data Procedures Workshop; to help organize the Data Procedures Workshop. We will get participants later when we know when it is. Each cooperator is going to have a representative to help SEDAR staff in organizing it. Do you want to maybe see who is interested and then you can decide who the representative will be?

DR. REICHERT: I thought you were looking for people participating in that workshop. I didn't realize you were looking for one member to be on the Steering Committee. I stuck my hand up too quickly.

DR. BARBIERI: In my case, I discussed this with Chairman Patterson for the Gulf that I would be participating as a Gulf Council SSC member. That opens another opportunity for folks here to be involved in the South Atlantic end of things.

MR. CARMICHAEL: Or not.

DR. BARBIERI: Or not. Well, there will be additional opportunities. There will be a Steering Committee of sorts that is going to be put together to discuss how to structure that workshop that John mentioned; develop the scheduling and start drafting terms of reference, and all sorts of things; and develop a list of participants and kind of somebody has to develop that basic plan to be followed. If anybody here would be interested in participating, there is an opportunity there. My assumption is that participating in the Steering Committee or this organizing committee does not preclude you from participating in the workshop itself.

MR. CARMICHAEL: No, you can participate in a workshop or you can decide you don't want to participate in a workshop. That is really up to you; but we would like someone who has been through data workshops and kind of understands the challenges and can help figure out how we are going to crack this nut; definitely someone who has been through them will be most beneficial.

DR. CROSSON: I would volunteer for this actually, but I have not been through the data workshop. I've only been through the review workshops for SEDARS twice now, but I have not yet participated in that. Otherwise, I'm not sure that I would be of help.

DR. BERKSON: If I understand correctly – and I just heard about this now, so I haven't been a part of this – it sounds like this workshop is more about the ways in which the data are selected, analyzed, how they will be incorporated rather than the process through which the data workshop would work in the future. Okay, two very different things; and that should have something to do with who is selected to be on the Steering Committee. It should be someone who has been involved in the data workshops in terms of the actual crunching of the numbers.

DR. BARBIERI: Anybody who has been leading a very large data collection program, if there is anybody.

MR. CARMICHAEL: Luiz, I was going to say given the crickets, maybe we could turn to some folks who are from the states as kind of an SSC designee who do so much of the heavy work in these workshops. I'm thinking your shop in particular has some people that are always there, as does North Carolina.

Maybe between the two of you guys you find someone. I'm kind of thinking of Beverly actually in your case, because she has been to like every one, and it affects her life. I think someone like Beverly Sauls – and she does Gulf, and South Atlantic, so she has a huge reference of participation. I don't know what people think about that; we just throw Bev right under the bus and volunteer for something else.

DR. BARBIERI: Yes, but that is a good idea. I guess Marcel and I can talk offline on perhaps identifying a list of people that we could just invite or present them the opportunity to be part of this.

DR. CROSSON: Not to volunteer any of my former co-workers, but, Will, you might talk to Alan, because Alan has done a ton of these things. Make sure you let him know that I helped volunteer him.

DR. BARBIERI: Okay, that is a very good suggestion. I think that completes Agenda Item Number 3. We have a set of bullet points that Mike has captured that, of course, all of us will be working on fleshing out and developing a narrative. I think we are ready, John, for Item Number 4; MRIP Calibration Workshop Update.

MR. CARMICHAEL: As folks may or may not be aware, there was another MRIP Calibration Workshop. This was the 1st of February here in Charleston, Calibration Workshop 2.

DR. CROSSON: I'm sorry, can I back up to the previous? I had one other concern. Is there going to be an effort made to make sure that some people that are maybe familiar with the recreational time series, since those have been so problematic, and not just commercial dataset folks; given the MRIP/MRFSS confusion?

MR. CARMICHAEL: Yes, it will be all aspects of the data. It will be the commercial, the recreational, the life history, and the indices all together.

DR. BARBIERI: Beverly in particular has – she has been involved in the MRIP process, MRFSS and MRIP processes. She was involved I guess in both calibration workshops.

MR. CARMICHAEL: MRIP calibration; the first workshop addressed the changes in the estimation methods when they resolved the issues with weighting and people; how they went to sampling sites and all of that. Well, another change was made, and there are further changes coming. MRIP is a work in progress, rolling out things and doing pilots all the time; but this dealt with the Access Point Angler Intercept Survey.

That is when the folks go down to the docks and talk to fishermen to see what they've got. As a result of the work that was done to modify the survey and improve it, there have been some changes in how people are distributed to sites, the timing of people going to sites. I am thinking it was 2013 when it first came out, and then there were a lot of concerns with how the shifts were ranged and for-hire vessels and stuff in particular.

Then there were some further changes to get more out of that. There is a prime area overlaid the other shifts and it has all gotten very complicated. As a result, though, you do the estimates using this new sampling and look at estimates from past samplings, and it is kind of an apples and oranges situation. They may not exactly match up, which wouldn't be such a big deal other than the fact that you have assessments and ACLs based on that pre-changes in the survey.

Now you are judging them by applying ACLs and applying accountability measures using a changed way of doing the data. You obviously want to make sure that a fishery isn't shut down simply because you've got a different way of estimating the catch. That is sort of the point of the whole calibration exercises. As you can imagine, because it deals with the intercept survey component itself and it has been a transition over a while, it is a little more complicated than the earlier calibration effort, unfortunately.

The workshop was held and there were a number of contractors which MRIP has worked with, experts in the field and their staff as well. Several ideas were put forth for how to deal with this. They ranged from pretty simple ratio approaches to more of model GLM type approaches where even they can learn more as years pass and they see the factors that affect the catch estimates now compared to the past and they will be able to get even better calibration.

But obviously that being the 1st of September, it takes a while to get this kind of stuff done. They focused on Gulf red snapper and Gulf red grouper because of updates that are underway. Gulf red snapper is one of the stocks that had some of the most pronounced changes in the results. There is a lot of interest in getting it done for that.

They are trying to really focus on those first and get some information available preliminarily at least to allow those assessments to move ahead and then take a bigger, broader look at everything else with the goal of having calibration techniques approved and recommended for early spring next year to deal with the assessments coming up next year.

We hope to have a workshop report summarizing what was done. It has been a little delayed because it has kind of grown a bit. There are going to be some appendices that deal with the different ways of doing the calibration and will include them all in the one report. Those aren't done. The contractors are working on them and some of their staff is working on them.

I think focusing on red snapper and red grouper and getting them done has left some of the general work undone. Unfortunately, we don't have a report to look at yet, but I hope that in the next month or so we are able to at least have a draft and put out and have some of these appendices wrapped up and at least show some of the results.

That is really the big thing is seeing how it actually affects the different species. It is very variable. That is one of the problems. This is happening kind of late, because at some point there was a thought maybe you wouldn't have to do it, it wouldn't necessarily need to be a big calibration effort, and different regions could just handle it on their own.

But such big changes came out from a couple stocks that we decided we had to have this calibration effort comprehensively to deal with it. The other big thing that the group talked about during the workshop was not just this problem but how you deal with the future. The next big change that is going to come out will be a change in the effort survey, which is the part that is now done through the random dialing of households and informed by license frames and things like that to improve the universe.

There are going to be changes in that coming, too. A subgroup during this workshop actually talked about recommendations on how best to handle calibration when that is occurring and talked about having a plan in place to deal with calibration; maybe a better way of doing the comparison of running the side-by-side, so in the APAIS there was some side-by-side done. North Carolina did a pilot for a full year; but it turns out the impacts of North Carolina were vastly different from the impacts in the Florida Keys.

As we know from before, the Florida Keys is just Monroe County. With the way it falls, it is just one of those places where it seems particularly prone to big changes in this estimation process. One of the recommendations I thought was really potentially useful was rather than focus on one

state and be very intensive there, spread out and cover the whole year and cover different areas, because the Gulf is not like the South Atlantic. It is not like the Mid-Atlantic.

Then doing a lot more outreach in terms of what the changes are and maintaining the ability to provide estimates in the old currency, as it's called, as well as the new until you get the new fully done. There is also some concern with not calibrating calibration and having, you know, this is the basic data and then apply a set of calibrations and get the new data.

But given that these changes are going to be incremental from year after year, I don't know that that is really going to be possible, but certainly it is laudable to not calibrate calibrations. Logically it makes sense, but we'll see where that goes. I think that will be an interesting part of it too. Hopefully, it helps inform the future changes, because I expect the effort survey is going to have even bigger changes. Hold on to your hats, I would say. Are there any questions on this?

DR. BARBIERI: I just had one quick one. Do you think we are going to have a report to discuss in more detail in April?

MR. CARMICHAEL: Definitely. We should have a report and we should have good results by April as well if we can look at how it is actually panning out.

DR. BOREMAN: By April we should have the fishing effort survey, the new one. It is not going to be implemented by April, but at least the report will be out with an implementation plan. What we've done on MRIP is we've established another team called the Transition Team. That team, one of their charges is to develop a process for calibration and how do you introduce new methodologies and so on.

The pushback they are getting from NOAA leadership is they don't want this incremental approach of every week we turn around and have a new method coming out and so on. They want to do like once and then give time to adjust to it before we do it again, maybe years. That is a complicating factor, because you always want to get out the best methods.

MRIP was designed so we can be flexible enough to incorporate new methods once they come out. The major difference between the new access point intercept survey and the old one, which is causing issues especially with the Gulf snapper, is that the NRC Panel pointed out that the original thought behind MRFSS was that the sampling unit was the interview, the number of interviews. The more interviews you conduct dockside, the better data you will have.

Well, the panel pointed out that is not how the methodology was designed. The sampling unit is the time period, the day. You should be stratifying by time of day and making sure you touch the appropriate – you concentrate effort during the times of day when you expect to intercept the most people or the most fishermen.

But you don't have the people out on the docks going let's see how many interviews I can get; oh, somebody is pulling their boat in, I'm supposed to be over here on this pier; but since somebody is pulling their boat in, I will just run over and grab that interview, too. That created all kinds of problems; and that led to the first calibration workshop where our consultants were

able to come up with a methodology at least to remove most of that bias caused by the individual decision being made by the interviewer.

But we only got back to like 2003 for the data to do that and hadn't gotten back any earlier than that. But this one, especially with the Gulf snapper, what happened as far as the simple explanation that I can see with Gulf snapper is that when we started stratifying the sample by time of day, we had people out on the docks after five o'clock.

All the offshore vessels on snapper weren't coming into the dock until after five; so they were missing that part of the catch in MRFSS. All of a sudden with the new stratification scheme, we had people out on the docks at six and seven and eight o'clock; and the catch just jumped up. Now, did it jump up because – did the assessments underestimate or overestimate fishing mortality because of that?

That is something that causes an issue; and, of course, we had three states secede from the Union in the Gulf because of that; led by Louisiana. Texas has always been out and Alabama and Mississippi too, were toying with the idea. We've got to bring them back into the Union, and one idea is to have this calibration workshop to get it out there to see how we can handle these data.

That is what they call best science information available, and we're using it and this is one of the fallouts from that. Sometimes you've got to throw out the old method; it just doesn't work. It was really biased and giving the wrong answers.

DR. BARBIERI: If no additional comments or questions, I think your part of the report is done too, right, JC? There is no action, no specific actions that are required. This is really an informational agenda item. That brings us to Agenda Item Number 5, the 2015 National SSC Workshop. As they explained at the very beginning, John Boreman has graciously agreed to provide us with a general update of what is going on there.

DR. BOREMAN: I will start with SSC 4, National Workshop 4, which was held in Williamsburg, Virginia in 2012. The themes of that workshop were social sciences and ecosystem. When that workshop ended, the recommendation was not to have another workshop at that point, another national workshop; the reason being that Managing Our Nations Fisheries 3 was in the works and being planned at that time.

In 2013 they had Managing Our Nations Fisheries 3; so that kind of superseded the idea of having a National SSC Workshop. We did have Managing Our Nations Fisheries 3. Then this past spring a smaller group of us, not all the chairs, but several from the east coast and west coast; we were talking with each other and Rich Methot, and now we have Doug Lipton on board, who is the chief economist for NMFS.

Jason Link is now the chief ecosystem scientist for NMFS and Rick Methot is the chief stock assessment scientist for NMFS. Well, all three of them got together with a handful of SSC Chairs and decided maybe it is time for another National SSC Workshop. We threw out some topics.

What you will see in terms of the themes for that workshop, it is basically a horse designed by a committee. We had to compromise, because the east coast contingent was pushing for model challenged and data-poor stocks; again, trying to wrestle those and how do we deal with the ABC setting. The west coast was pushing more on the climate and ecosystem side of things.

That is why we have now two major themes. Actually it is one major theme. What are they calling the workshop – every time I see this – providing ABC specifications in the face of uncertainty; from data to climate and ecosystems. That covers it. What we're going to do in terms of the general themes is again start with a round robin on now that all the SSCs have had this ABC in practice for three/four years, just a round robin on how we're doing; a self-evaluation, share any issues. Nobody has really gone to court yet over their ABCs; that is interesting.

Of course, we keep waiting for the court actions, because that will really define whether the ABC process is working or not; but just to see where we are, what the challenge is, what the roadblocks. I am sure that Luiz will bring to the meeting our discussion in the past couple of days, and I will bring what we talked about at our ABC Evaluation Workshop last month and get those out on the table.

Then there are subthemes, and that is basically Subtheme 1, which is to evaluate existing ABC control rule issues, challenges, and solution. It should be solutions, plural, I hope there. The solution is quit and back it. The subthemes are again ABC specification for data-limited and model-resistant stocks. What we're running into at least in the northeast is some model resistant.

We have a lot of data; it is just the modeling, and we just can't find a way to get the models to fit the data. Black sea bass is a prime example of that; how do you deal with that? Then it says here the subtheme is implementation of National Standard 2 in the face of uncertainties. Well, Rick Methot is going to come in and update everybody on what the status is of that National Standard 2.

Then Subtheme 3 is incorporating ecological, environmental, and climate variability in stock assessment and ecosystem-based fisheries management. After that there is also a habitat issue, separating out habitat from ecosystems, because I see habitat as a bottom-up approach where ecosystems is a top-down approach.

We still hadn't figured out where those two meet in the middle; but they both are important, especially when you are setting ABCs. You've got habitat level impacts you have got to factor into stock assessments and then the broader ecosystem challenges of trophic dynamics and climate change and physical and chemical changes in the environment and so on.

That is where we stand. There is $150,000 set aside that is going out to Kitty and her group in the Western Pacific to run the workshop. That is enough funding to fund five people from every SSC. That is my contribution. They wanted three and I pushed for five, because last time we had a budget of $150,000 and we easily accommodated five from each SSC.

Again, you've got ecosystems and you have data-poor stocks and model-challenged stock. You need several people with different types of backgrounds from the SSC to be there. It will be up to the SSCs deciding who they are going to send and who is available. It is Honolulu, so not

everybody's hand go up at once. It should be a good meeting. Kitty puts on a good spread in terms of catering and stuff, because she sponsored SSC 1; that was our first one.

Then when I was still back in S&T there were three people from NMFS at that workshop. There was Sam Pooley, the Center Director; Rick Methot, and myself. I wouldn't let anybody else from NMFS go, because I figured this is a chance for the SSCs to get together. They all have a common enemy, and this is their chance to meet not behind closed doors, because it was a public meeting; but it is their meeting, and I still see this as this is an SSC-driven meeting. Try to keep the influence and the attendance of NMFS to a minimum, just where it helps us, but this is our meeting, so we should adopt it as such. I don't know what else you want me to talk about.

DR. BARBIERI: That is it. This is a wonderful overview and it gave some background and perspective on how this whole thing came up and what we're looking into accomplishing. We are going to be in the process of identifying folks to attend the meeting to represent our SSC, between SSC members and staff. For folks who are interested, I would say the best thing would be to e-mail myself, JC, and Marcel.

We are going to collect all of those and we're going to see between staff attendance and SSC member attendance how many we are going to be able to accommodate. Of course, we want to be as inclusive as possible and have as many of us there as we can. The meeting date is February 23rd through 25th. Look at your calendars; and if you are interested and available, please let us know. John, you had another?

DR. BOREMAN: Yes, one of those five slots is staff. John needs to go, because we need rapporteurs and people to keep everybody else honest there.

MR. CARMICHAEL: Yes, I'm sure I could do it.

DR. CROSSON: I am assuming that you don't mean NMFS members that are on the SSC as being the enemy; but I have a question for you.

DR. BOREMAN: I missed that.

DR. CROSSON: You were talking about NMFS being the enemy; and I said I am assuming you are not talking about NMFS members that are on the SSC.

DR. BOREMAN: No, I mean the common enemy is the people writing these national guidelines that we have to follow.

DR. CROSSON: I do have a question about a specific item that you have listed on there. You have something on the subtheme about finding ABC specification for data-limited stocks, and there is something in there about how socio-economic information might be incorporated into that process. Is that Doug's doing? What was the impetus for putting that on there, and is that something that you are going to try and get a number of SSC economists and social scientists to chip in on?

DR. BOREMAN: We talked about that. Doug Lipton was on our calls. We kind of see the social science as woven and not have – like the last workshop, we had special session on social

science; and a lot of people felt I have in the concurrent sessions that we missed a lot. We are going to have just general sessions and have social science kind of woven into all these themes. That in particular; that was something that Richard Seagraves and I put in to kind of expand again on our fishery performance reports that we have done, because there is a lot of interest in other councils to see how well they are working for us since we've been doing them now for a few years, and so kind of bring that up in the round table discussion to start with.

It was in, it was out and now it is back in again. There was a little confusion about, well, do you mean we're going to have sessions on social science? No, it was just how that information now is how we can get that information into the ABC process.

DR. GRIMES: John, you remember, was it around 2010 there was a National Habitat Assessment Workshop? I think you actually made a presentation along with me. The purpose of that was to talk about how you were going to integrate habitat information into stock assessments and so on. Has there ever been any sort of cross-fertilization between that process and the documents they produced and the workshop here and the plans and so on?

DR. BOREMAN: Not as far as I know. It is a whole new cast of characters, as you know, in headquarters on the habitat side. It is a former deputy from the Regional Office is up there now, Buck Sutter. Buck is very familiar with what was proposed at that workshop in terms of getting habitat information into stock assessments. I've been talking with him about it. I'm sure that that is going to factor into that subtheme here in this workshop somehow; but they only had that one Habitat Assessment Workshop. I don't know if they've had another one since then, since 2010.

DR. BARBIERI: By the way, I think those are some of the trigger questions is what those are being called, right, for the Subtheme 1? Thinking about those relative to what we discussed yesterday afternoon and this morning and some of the things that we want to integrate into our own report from our ABC Control Rule Workshop – issues that are being raised that would give us folks attending the National SSC meeting; it would give us some material to present our perspective on the performance and the components of our own ABC Control Rule.

John, thank you so much for that thorough overview. That is very nice to have that complete perspective and history. It is funny, because I remember that first National SSC workshop. It was orders of magnitude smaller than the subsequent ones. It was amazing. It was a relatively small room.

DR. BOREMAN: Well, the third one was here in Charleston; we had, what; 30 or 40 people from NMFS attend that. They just took it upon themselves to show up. That was overkill. I thought they overwhelmed the meeting and basically this happens in the government. I am a bit cynical because having worked for them for 35 years, but they smell the captured audience.

They wanted to use the SSC to roll out some new ideas and so on, which is fine. We are going to see a little bit of that. You see a little bit of that in this agenda, too. Rick is going to talk about his prioritization process and the updating of the NS1 Guidelines and so on. But we want to keep that to a minimum; only as it is relevant to what we see as the themes of our workshop and not the other way around.

DR. BARBIERI: Again, for those of you who are available and interested in participating, please send us your name and we are going to collect those and then evaluate and develop our list of recommendations for attendees from our SSC. I am being reminded here, and rightly so, that this might be time for us to have a little bit of a break. It is almost 2:20; let's have a ten-minute break and reconvene at 2:30. This is not an action.

DR. BOREMAN: I guess it is – take a look at the agenda. If you see anything there that causes heartburn, get back because this is still a work in progress, this agenda.

DR. BARBIERI: In terms of where we are, we have to dodge some issues here regarding the flow of work for our agenda given the fact that we had the schedule set up in a way that we have different people coming in at different times. We are not going to have everybody available throughout the meeting to sort of move forward and advance some of these agenda items and get us out of here earlier.

We are trying to deal with this and trying to call some of the presenters to try to get as many people here as soon as possible to see if we can get that moving. Since we just finished this discussion about the National SSC Workshop and some of the discussions that led to the points to be included as themes for the National SSC Workshop, having to do with the National Stock Assessment Workshop and the National Habitat Assessment Workshop and the merging of the two; it so happens that we have in the room a newcomer to the region, Lora Clarke.

Lora used to work up in NMFS Headquarters, and she was actually in charge of dealing with those reports. She is now here living in Charleston and working now for the Pew Charitable Trusts and participating now in this process in a different capacity. John Boreman was kind of chatting with Lora earlier.

Some of the things that John alluded to during his overview and comments regarding the National SSC Workshop, Lora can expand on because she has more information of where some of those things are and how the process took place. Lora, if we're not imposing on you, we ask you to approach the table if you don't mind, since we have some extra time, and get some additional information on some of those items. My apologies for putting you on the spot, but we're just glad to have you here and able to provide some of that input and feedback.

DR. CLARKE: This is what happens when you admit things during the break, right, you get called upon. I previously was the National Habitat Science Coordinator at NMFS. I left that position in 2013, so I don't have the latest, but I can update you on what it was then. The first NHAW or National Habitat Assessment Workshop was held in 2010.

There was a second follow up workshop held in 2012. That workshop sort of had three themes. One was incorporating habitat information into stock assessments, the second one was refining EFH designations, and then the third was improving the flow of science information into management.

One of the major follow-ups from that second workshop was NMFS decided they wanted to design or work on a model to show how inshore habitat affects offshore productivity. I don't know who all is involved. I know Howard Townsend at the Chesapeake Bay Office is one of the lead modelers for that.

I can also say because of these workshops, NMFS Office of Science and Technology is doing an internal RFP; I think they are in their fourth year now. They fund about 25 projects on how to incorporate habitat science into stock assessments. I would also add that I think the best example they have of that so far is John Manderson's work.

He is doing some work looking at thermal habitats with butterfish. I think that is probably the most advanced study and best example I have. I can try to answer any questions; but again I think if you really wanted to get into this, Kirsten Larsen is the current Habitat Science Coordinator for NMFS.

DR. BARBIERI: Thank you so much, Lora. If you can stick around for a minute, we are going to see if there are any questions or additional comments from the committee regarding this issue. I remembered that several of us attended at least one of those workshops, the National Stock Assessment and Habitat.

DR. CLARKE: Yes, the first one was a joint one. The second one they did separately.

DR. BARBIERI: It tried to integrate habitat issues into the stock assessments. Are there any questions or comments for Lora on that?

DR. SCHUELLER: Yes, I just have a quick question. If there have been projects funded -- and maybe I should know this – is there a place on the web where we can look at what has been funded?

DR. CLARKE: There is a project description on each one. I can send the link; it is NMFS Office of Science and Technology under their ecosystem subdivision under habitat. I am happy to send that to somebody.

DR. BARBIERI: I would imagine this is something that was under the habitat umbrella, but kind of similar to the FATE program, fisheries and environment. The agency used to have some external RFDs as well, but they have some internal RFDs that folks can cooperate. It is something that regionally a lot of people have worked on. It would be nice to have this opportunity for some work that is more directed at integrating that habitat component into that as well.

DR. CLARKE: Also on that page was the workshop report from NHAW 1 and NHAW 2, if you are interested.

DR. BARBIERI: Thank you so much, Lora, we really appreciate it. I know you are wearing a different hat now, but it was convenient to have you here and willing to give us a quick report. With that clarification and additional information, that brings us to Agenda Item Number 6, ABC Control Rule Workshop Report.

Obviously, there is nothing to report at this point, and all of you have been involved in this exercise; and perhaps not all of you. I think Sherry and Eric have not been able to participate in the workshop. But the bottom line of this was yesterday afternoon and this morning we went into our ABC Control Rule. You may remember that we have Steve Cadrin, Marcel, and I as a little breakout subgroup of the SSC that start putting together and organizing some of those

issues for discussion and identify an evaluation; basically like a pit stop. After five years of having implemented and revised our ABC Control Rule, stop and check; where are we?

Are we in a good place; is there anything here that could be refined or upgraded. New techniques have been developed. The science of ABC determination has evolved over time. That is what we did. By and large, the group really didn't see any major reason for large fundamental changes to the ABC Control Rule.

Everybody feels that basically application of the control rule as it is right now is hitting the spot. There are a few things that need to be upgraded and updated. You are going to see a draft report that is going to be circulated soon that provides you some of those issues, some of the issues that the committee recommended for modification.

Then there is a streamlining of the entire process and kind of developing a document that is more clearly communicating to the council members and fishing public what are the main components of the ABC Control Rule and what are the issues that are taken into account for application of different portions of the rule and the situations and all.

As we completed the process late morning today, we basically agreed that this draft document that Mike sort of helped capture some of our comments and notes, just like he does here with our meeting report; Marcel, myself and Steve Cadrin, we're going to be working into fleshing that out and to developing more of a narrative format for what is there.

Then we are going to coordinate, of course, with John and Mike in terms of integrating some tables and some different configurations of tables and options, things that the committee can have more to review, and then distribute that for the full committee. Most likely we are not going to be able to be presenting anything at the December council meeting on this, because a lot still needs to be done and fleshed out before we get to that stage.

But we are going to probably revisit all of this at our April meeting and present to the council at our June council meeting report. In a nutshell, that is the bottom line of how the last day went and the main products and steps forward that were identified as part of our ABC Control Rule Workshop Report.

With that, I am going to open up for those that may have remembered additional topics that I didn't think of that you would like to add so Sherry and Eric can have that information as well as all the folks who are probably listening to the webinar. All right, looking at our agenda, I had mentioned before that we had not planned on starting the stock assessment presentations and detailed review of those reports and assessments until tomorrow.

Basically we had a dedicated day earmarked tomorrow to handle just the assessments. There will be, I am sure, quite a bit of discussion on some of those issues. Then in terms of Snapper Grouper Regulatory Amendment 16 and the bag limit analysis for gag and blueline tilefish; we're really not going to be able to get started on this until we have – Nick Farmer I think is going to be the Fisheries Service representative that is going to come and present that analysis for us and lead us through that discussion. We're trying to see if we can get Nick here earlier than tomorrow evening, so if we have an opportunity to get into some of those items mid-

afternoon tomorrow, we can. But at this point that is still in question. That leads us into potential discussion this afternoon of the council work plan update. Gregg usually presents that.

MR. CARMICHAEL: Not so much a presentation as just to fill you in on the planning documents and the status documents and opportunity to ask any questions about where any of the actions stand. Brian will have some things to bring up. He wanted to raise an issue with the fillets and dolphins probably here under this, but maybe we can see about getting him over here tomorrow.

DR. BARBIERI: That would be the fillets from The Bahamas?

MR. CARMICHAEL: Yes, what does it say in there? Yes, the counting of fillets towards possession limits. There is an action; Dolphin Wahoo 7/Snapper Grouper 33. I think two fillets count toward one fish. It seems pretty straightforward.

DR. BARBIERI: Yes, it should be. If I can draw your attention to the council work plan update – this is the beginning of the very bottom of Page 12 and spilling into the top of Page 13 – you can see where some of the FMPs that is in progress right now, in process and the staff member taking the lead on those FMPs.

MR. CARMICHAEL: We do have a new staff person in the tech staff group, Chip Collier, formally of this body and North Carolina Marine Fisheries, who is now a staff person. Maybe Gregg will say a bit about where he fits in, introduce him to you, but he is up in D.C. getting oriented by NMFS to the council system today, so he will come back very intelligent.

MR. WAUGH: We did tell him not to put any hats on that they give him or anything up there. Chip is in charge of our coral and will be working with Roger on habitat and the ecosystem work as well. His main focus is going to be coral; but as you all know from working with Chip, he is very sharp and we intend to use his expertise as much as we can.

In terms of that list there, I just wanted to mention that the council has switched. In terms of looking at MPAs from Snapper Grouper Regulatory Amendment 17, we are now looking at this in Amendment 36 where we are looking at more targeted, what we are calling spawning SMZs. We are looking for areas that are very critical to spawning for a number of species, including speckled hind and Warsaw grouper.

You will be seeing that at your April meeting, getting an update. Spiny lobster; we have two SSC members who are going to be participating in that process looking at the ACL. We didn't feel that ACLs were appropriate for spiny lobster, but it met the criteria so we had to put in one. Similar to our pink shrimp, when the ACL is met, the trigger is a review of that to determine if there is something biological going on and we need to adjust the ACL.

That is something that we will be working on. Of course, later in this meeting you will be looking at the black sea bass pot analyses. That is a big issue within snapper grouper right now. Of course, mackerels we will be dealing with what comes out of the king mackerel stock assessment. That is a quick overview. One thing I mentioned to a couple of you, we've had an extremely large number of amendments the past couple of years; and the prospect for next year is a more reasonable workload. Hopefully, we'll keep with that.

I think the council, you know, trying to deal with seven amendments for formal review at our last meeting, I think everybody has hit the wall and just realized we've got to slow down some. I think that combined with the lack of any stock assessments for a little while will give us a chance to get caught up. I will be glad to answer any questions, anymore specific information. Also, outside of this meeting if you have any questions about any of these, feel free to call the appropriate staff person.

DR. CROSSON: I am assuming this is fairly open, so this question might not be just for Gregg as much as it is for Ben or Michelle. How is the council visioning process coming along? I knew that you had a special council meeting about that recently.

DR. DUVAL: I'm not prepared to talk about that. My vision might not align with everybody else's vision. No, we had a great special visioning council meeting two weeks ago, so I have been living at this hotel for the past three weeks, pretty much. Staff did an amazing job of developing sort of mini-issue papers for each of the different topic areas that we discussed, which ranged from access to the fishery to discards, to data, to sub-regional management, stakeholder outreach, and for each one of those we had that background issue paper.

We had a variety of sort of trigger questions to kind of get people talking, and then we were at small tables in breakout groups and really trying to come up with strategies to address that particular issue. It was sort of like a huge raw data brain dump with regard to all of those issues. Amber Von Harten on council staff is digging into that stuff right now.

Staff is going to be doing what looks to be like a gap analysis; so basically looking at all the strategies that council members came up with and comparing those to what we received from the public during all the visioning port meetings that we had in March and seeing are there some strategies that constituents provided that really just didn't surface in our conversations, that sort of thing.

We'll get something of an update at the December council meeting. We have kind of a rough timeline for moving forward and getting what we're calling a draft blueprint for the snapper grouper fishery and then soliciting public input on that and then having like another sort of special two-day council meeting. Because our meetings tend to be so full, we just don't have time to sit down and devote that much space during a normal council meeting week to doing that.

It just sort of depends on how much is completed between now and December and the December briefing book. All the materials are due just in two and a half weeks. It is probably going to require some additional work, but that is where we are. It is going to require some really difficult decisions by the council.

We received a lot of great input on strategies, some of which are diametrically opposed to one another; so you want to make sure that you're being responsive to the input we received from stakeholders, but also trying to mesh those together into something coherent that can really be used as a planning document for the future is going to be difficult.

DR. BARBIERI: It is good to have good questions and good to have that update. Is there anything else, JC?

MR. CARMICHAEL: We are sort of stuck with the presentation timing.

DR. BARBIERI: Yes, the presentation timing is really constraining our ability to adjust the agenda, the order and timing of the presentations.

DR. CROSSON: I'm not sure; we're just sort of moving around on these other items now. Did the April SSC meeting move? Wasn't it supposed to be in May at some point? Now I'm seeing something n April.

MR. CARMICHAEL: There was discussion of moving it back to accommodate the SEDAR 41 review, to try and get that to the council by June. But since now that has been delayed, we can fall back closer to our normal timing; which gives you more time to get your report done for the June council meeting briefing book. We were going to have to really shorten everything to try and meet that schedule, which we were willing to do but now we don't have to. We can go back and get another week. It is the last week of April.

Myra has a presentation overview of what Regulatory Amendment 16 does in terms of management actions and all. If we go over that now, you can start thinking about it and then it will free us up some time on Thursday, which is likely to become quite busy. The more we can do now the better.

DR. BARBIERI: My understanding, John, is that Nick Farmer also will give a presentation that is more technically oriented and more detailed and gets more into the weeds on those things. For us right now to get the more sort of management goals and oriented-type of a presentation; I think would be good. It will get us focused on some of the issues that we need to think about for Nick's presentation on Thursday morning.

MS. BROUWER: I wasn't planning on giving this presentation today, but here goes. You don't have this and it was not included in your briefing book, because what I included in there was simply Chapters 1 and 2 of the Draft Regulatory Amendment 16, because there is really nothing else in the document.

All the council has had the chance to do is revise the various alternatives; but then we were talking in-house and talking about how you all probably needed some background information; what was put in place through 18A, and all that kind of stuff. I figured it would be easier to just do a presentation, so here it is.

Currently about 55 to 70 percent of the landings of black sea bass comes from the pot fishery of the commercials. In 2012 the council approved Amendment 18A, and that is the amendment that put in place the current endorsement program. There were 32 folks that met the eligibility criteria that the council established and received endorsements.

The breakdown is up there on the screen. Fifteen of those endorsements are in North Carolina, nine in South Carolina, and seven are currently in Florida. There have been some transfers, there have been some sales and stuff; but that is I think currently the distribution of those endorsements. The other thing that Amendment 18A did was set a limit to the number of pots that could be fished at any one time, and that is 35. In talking with some of the fishermen, we had the Snapper Grouper AP here last week.

We discussed this amendment with them, and they were relating that back before 18A when the winter fishery in North Carolina was going strong for black sea bass, sometimes they had over 100 pots that were fished at one time. Amendment 18A made a substantial reduction in effort for that fishery. There was also a requirement to bring the pots back at the end of each trip.

Folks used to just leave them out there, sometimes for weeks at a time. That is no longer allowed. There is a trip limit of 1,000 pounds gutted weight for the pots. Then, of course, they have to abide by all the requirements under the Atlantic Large Whale Take Reduction Plan. A little bit more background; the fishing year currently is June 1st through May 31st.

That is getting ready to change; and I will tell you about that in the next slide. Because the ACL was met early, the season closed in the years that you see up there. It closed in 2010, 2011, 2012, and in 2013 there was a delay in the opening of the pot season in order to get the endorsement program in place.

There had been, from what I recall, some issues with the transferability action. Anyway, it took a little bit of time to get that in place, and so they delayed the opening by a month. Then Regulatory Amendment 19 came along, which is the one that put in place the adjusted ACL based on SEDAR 25, I believe it was. It had the larger ACL, and then it also put in that November through April closure due to concerns of interactions with the whales. During that season, the ACL was not met so it went the full year.

Here are the current requirements under the Atlantic Large Whale Take Reduction Plan. I hope you don't have too many questions about this, because it is very complicated. There are all these various zones and there is overlap. Anyway, there is a requirement for surface buoy markings, line markings on the various lines; and they have to have different color schemes for each of the zones. They are required to have weak links in all vertical lines and sinking ground lines.

Then there are new regulations that are going to be effective November 1st for the southern restricted area, which is sinking buoy lines and ground lines, weak links, vertical lines, maximum breaking links, only one trap per buoy, no multi-trap trawls, and bringing back pots every day, which is already in place in our region.

This is what is getting ready to change once Regulatory Amendment 14 becomes implemented. The commercial fishing year is going to go back to being the calendar year. The pots are going to open on May 1st, with a 1,000 pound trip limit, and it goes through October 31st, because that is when the current closure is effective.

There is also a hook-and-line trip limit that is going to be the same as the pots from May to December, and then the first of the year it is going to be 300 pounds. From January 1st to April 20, when the pots are closed, the hook and liners can still fish on that 300 pound trip limit. This brings us around to why the council is considering action in this amendment.

As I said, the increase in the ACL for black sea bass has raised concerns about fishing getting into the season where the right whales are calving and the large whales are migrating through the area. That has raised a lot of concerns with Protective Resources. Basically, if you remember when Regulatory Amendment 19 was being put together, the Protective Resources Division knew that this was going to be an issue and basically said to the council, well, that amendment in

addition to increasing the ACL needs to have an action to minimize the risk of interactions; and so that is why they requested this closure.

It was like you either put the closure in place or you can't raise the ACL. Also, a little bit more background; there is currently a biological opinion that was put together for the snapper grouper fishery in 2006. That biological opinion evidently doesn't take into account a lot of the more recent requirements that have been put in place.

The way it works, once the council picks a preferred for this amendment, then that will likely trigger Protective Resources initiating another biological opinion, which is what the council wants, because it is dated and they really want to be given credit, so to speak, for the regulations that they have put in place in order to minimize risk to protected resources.

The intent of the amendment is to review this closure; is it necessary to have a complete closure? Are there any alternatives that would allow the council to constrain it just temporally? Can it be refined to allow some fishing during the winter, because the wintertime is when the fishermen say it is the best fishing?

The fish are bigger and darker and the price is higher and they want their winter fishery back. The council is considering removing the closure, shortening the timeframe, and spatially designating some other boundaries that are going to be a compromise between protections to the whales and allowing the fishermen to fish.

There is only one action in the amendment with eight alternatives. The next series of slides are just going to walk you through the alternatives that you have already seen in your briefing book. Alternative 2 is to basically just remove it and be done with it. Here come the alternatives that get very complicated.

We worked together through the whole IPT process with the Protective Resources staff from the Regional Office to develop these alternatives. They have helped put together the list of coordinates and the maps. This one is the current critical habitat designation that was put in place on June 3, 1994.

The council did state their intent that this boundary would not automatically change if the boundary for that critical habitat were to be altered, because there is some talk about revising the critical habitat for right whales. But they said as far as the council goes, they don't want to see it change every time Protective Resources changes it.

This is what it looks like. It goes from Georgia through north Florida. This is the only map that we have of it. It is not a very good map; you can't really see how far it is from shore. That is Alternative 3. The problem with this, of course, is Protective Resources is concerned that obviously off of the Carolinas there is no protection for whales.

Alternative 4 represents the calving habitat. It is from Cape Fear, North Carolina, southward to approximately Ponce Inlet, Florida. It is based on model outputs from those folks that you see up on the screen. It takes into account sea surface temperatures and bathymetry, and it looks like this. The hashed area is the proposed closure area.

DR. BARBIERI: Myra, just a quick question. If you go back one; this closure, it is a year-round closures, right?

MS. BROUWER: No; the closure would apply annually from November 1st through April 30th for all the alternatives except for the last one, and that one is a little bit different.

DR. BUCKEL: Myra, they didn't make a distinction on this map. The model only goes to Cape Fear, and so Cape Fear north is an extrapolation of that.

MS. BROUWER: Correct. Here is Alternative 5. This one is generally waters 25 meters or shallower from Cape Canaveral to Savannah. Then from the Georgia/South Carolina Border to Cape Hatteras, it is 30 meters or shallower. This alternative was developed by the Protective Resources folks, and it is based on right whale sightings and sightings per unit effort by depth and captures the Carolinas and the Florida/Georgia area. That is what this one looks like.

Alternative 6; this one is based on comments that the council received when we were scoping this document. The Humane Society, I believe it was, submitted a couple of suggestions that the council subsequently included in there to have a wider range of alternatives. That is where this one came from.

It generally represents the 75th percentile of sightings off of Florida and Georgia based on work by Garrison and Keller. I am assuming you've seen this. It is very wordy, and I'm not going to spend time to read all that to you. Here is the map representing Alternative 6. Then Alternative 7 is based on an alternative submitted by the public.

It is the existing southeast seasonal gillnet restricted area that is currently under the Atlantic Large Whale Take Reduction Plan and then an additional area off of North Carolina. It looks like that. Then finally Alternative 8 is one that we're still working on; that is why I don't have a figure for this one.

Basically the council said to staff, I believe it was in June of this year, they would like a combination between what was then Alternative 8, which includes what you see under the subalternatives, which talks about a different time period for the closure. They wanted us to combine that with Alternative 3, which is the one that has the critical habitat.

They said, well, that would be a good compromise because we're capturing the area off of Georgia and Florida with the existing critical habitat designation. It was a council member that actually offered up these subalternatives, and they've been tweaked a little bit since. This one we would have a table with all the coordinates and all that, but basically from north of the Altamaha River to Cape Hatteras.

Then we have subalternatives that would apply annually from November 1st through December 15th and March 15th through April 30th, throughout. Then Subalternative B is the same thing, but it differentiates between the Carolinas and then the rest of the area. For the area north off of North Carolina and South Carolina, it would be November 1st through December 15 , then March 15 through April 30th. Then for the core area of the calving habitat, it would be November 15 through April 15.

That was I think an attempt by this particular council member to compromise and to try to capture temporally when the whales are most likely to be in the area. Here is my attempt at a summary of all these alternatives, like a little cheat sheet. That is the alternatives; and the timing is you guys review the analyses that Nick Farmer is going to present to you on Thursday.

The council is going to see this document at their December meeting and approve it for public hearings if they so choose. If they can, if there is enough information for them to pick preferreds, they will do so at that meeting. We'll hold the public hearings in January. They will get to see those comments in March and approve all actions, make any other modifications.

The AP is going to meet in April. They are going to get to see it since they didn't get to this past meeting and then the council would submit it for submission in June. This is an environmental impact statement, which means that the timing is a bit lengthier and there is more that needs to be included in the documents. What we need from you basically is just a technical review of the analyses that Nick is going to present tomorrow; is the methodology appropriate?

Is it going to be helpful for the council to make the decisions they need to make? More than anything, that is what they need to know is have they put together the right information, the right combination of information that is going to allow the council to pick one of these alternatives? Then give us any recommendations you may have for additional analyses or changing the exiting one. That is all I have, so I will be happy to take questions.

DR. BARBIERI: To me at least having this summary presentation is a big help after we had to plow through all of that stuff and kind of capture the nuts and bolts of what we are trying to think about broad pictures. Any questions or comments for Myra?

DR. SEDBERRY: Myra, we kind of know where the whales are and we sort of know where the pots are. Do we have any data on where those two interact with each other? Have there been any interactions?

MS. BROUWER: There has not been a documented interaction between the whale and a black sea bass pot in the South Atlantic; so, no, we don't. I think that is part of what Nick is going to do on Thursday is show you how he has attempted to superimpose those two, the effort for the fishery and the distribution of the whales.

DR. CROSSON: I have a question for John Boreman. The Mid-Atlantic has a black sea bass fishery, right? Do you know off the top of your head what their season is or basically whether they are open when this closure is going on?

DR. BOREMAN: Do you mean in the winter? There is a small fishery in the winter; it is a party/charterboat fishery that works out of New York and New Jersey. I am not sure if there is any other going on. That is the only Wave 1 recreational fishery. I am not sure about the commercial side, when that is operating, so I can't really answer your question.

DR. DUVAL: Scott, just to answer your question; there is a commercial black sea bass fishery that operates in the winter off North Carolina. The jurisdictional split between the Mid-Atlantic and South Atlantic is Cape Hatteras. In North Carolina we start issuing proclamations to open that fishery usually in the fall, late fall like maybe late November, early December.

It is generally considered a bycatch as part of the summer flounder trawl fishery, so part of the winter trawl fishery. We do in North Carolina save a piece of our state quota share for black sea bass from the Mid-Atlantic for folks operating pots and hook-and-line fisheries north of Hatteras. That is generally running in the summer months, I believe. We don't allow the trawls and then the very small pot/hook-and-line sector to operate at the same time. I would just need to check the recent proclamation to see if that is open right now.

DR. CROSSON: Then during the rest of the – like into the summer months north of Hatteras, is there a fairly active commercial? Is it jointly managed between ASMFC and the Mid-Atlantic Council? I am getting a little lost.

DR. DUVAL: Yes, it is; and so I guess what I was trying to say is that the winter fishery for black sea bass north of Hatteras in North Carolina is a trawl fishery, so December through like March. Then in the summer months we always try to save a piece of that state quota share to allow for folks using hook and line and pots north of Hatteras to operate within that fishery. It is not a lot, but it is a little so they are open at the same time. It is co-managed with ASMFC. Those state-by-state quota shares are kind of organized through the authority of the commission.

DR. CROSSON: There is a substantial Virginia fishery? I am trying to recall, it has been a while.

DR. DUVAL: Yes, Virginia has taken their black sea bass state quota share and established an ITQ fishery.

DR. SEDBERRY: On the pot gear, the weak link; is that at the trap end or at the buoy end?

MS. BROUWER: Sorry, I don't know.

DR. SEDBERRY: I think that might make a difference, but I don't really know enough about it.

DR. ERRIGO: Actually from what I have heard from just conversations with people who used to work in North Carolina; the weak links are actually stronger than the actual buoy line itself.

DR. BUCKEL: Myra, you mentioned there was a biological opinion in 2006, and that was when there were a lot more pots and a lot more effort. What was that biological opinion back then for right whales and black sea bass pots?

MS. BROUWER: I am not recalling the exact wording, but it looks at the entire fishery. I think it is classified as a Category 3; one of the categories that basically says that it doesn't really pose any risk. But there is some wording; I can picture it in my head. There is some wording about the black sea bass pots. I can probably pull it up on my computer here shortly and get back to you on that.

DR. BUCKEL: That would help, because if there was a lot more effort back then and there was a biological opinion and now we know there is less effort, so it couldn't be worse unless there is some other whale.

DR. BARBIERI: That is not necessarily going to be included in Nick's presentation, right, the previous biological opinion?

MR. WAUGH: This is going to be an important point for you to consider; what do you compare it to? The argument the region is putting forward is you have to compare it to the current situation, which is no pots in the water. What our council is interested in comparing it to is that Plos the previous time when there was a higher abundance of black sea bass, higher numbers of black sea bass pots. We are a little bit at loggerheads amongst the staffs about doing that comparison. That would be an important point for you to weigh in on.

DR. REICHERT: But that is counteracted by the fact that there may be more right whales now than last time around. I assume there are some numbers on the increase?

MR. WAUGH: That would be another great question for you to ask in addition to the one that George asked about the distribution of whales and pot gear.

DR. SCHUELLER: I guess when I was looking over this I was wondering is there a definition for what is an acceptable – right, they call it a take even though it might not be a take, it is an interaction or something. Is there a definition for that? We're sort of given these thresholds of numbers, but where does that –

DR. ERRIGO: We don't know what it is, but I don't think they know. No one has told us what it is. I actually asked, but we haven't gotten anything about that yet. It is kind of hard to calculate what the take is if you've never had an interaction. You can calculate the probable risk of an interaction.

Even if a right whale encounters a black sea bass buoy line; that doesn't mean that it will become entangled, one; and two, that it will result in mortality. It is very, very, very difficult to calculate what the actual risk is. I am not exactly sure they can even calculate what that is. All they know is that one or two individuals dying in a year is a big impact to the population.

DR. SCHUELLER: Right. My understanding is they are calculating that based on lobster pots, which isn't really however comparable you want to think that is. My other question was what is the legality in this stuff? If it is the ESA, how does that play? Does that trump everything? Is there really a choice or not? I don't know the answer to that. That is my question.

DR. BOREMAN: First, this has been a frustration when I was in the northeast with the lobster fishery. The agency takes a very wide view of what a take is, what constitutes a take. Anything that causes a whale to change their mind about something is considered a take. If they have to swim around the lines, then it is called a take; any type of interaction. In terms of the legality, it is always one of my favorite questions on a defense is set up the student and then say, no, sorry; ESA trumps everything. I think that is still the case is endangered species trump everything. But we add the Marine Mammal Protection Act in this, too.

DR. CROSSON: We're being asked to look at Nick's analysis and figure out whether the different scenarios that are being proposed here constitute the probability of a right whale interacting with a sea bass line, right? Yes, the ESA and the Marine Mammal Protection Act

trump everything, but we're the ones that are going to try and review this and see if these different scenarios are going to increase that risk enough that it will happen.

DR. BARBIERI: Just to Scott's point right; Myra, I appreciate that she put that there very explicitly in terms of what our role is, is that technical review of Nick's analysis. I would hope that Nick would present some introduction that would give us some – because there are some of these other issues that are associated with this discussion that would be good for us to be exposed to.

DR. ERRIGO: If Protective Resources who will decide if the changes that the council actions are proposing will pose a significant increase in risk to right whales, the task the SSC is being asked to perform is simply to review the methodologies here to make sure that the analyses being done will be useful for the council to use in making the decision on which alternative to choose and that they were performed in the appropriate way; a basic review. It actually doesn't matter if the SSC comes to a consensus that they don't think the action will impact right whales, because it is actually Protective Resources who makes that determination.

MR. WAUGH: Coming back to the weak link again and Mike pointing out that the weak link perhaps is even stronger than the black sea bass pot line; this may be again due to the fact that the weak link was designed for the lobster fishery where you've got much bigger pots fished in their trawl gear. In that case the weak link is the weak link. But you apply that throughout the range to all fisheries and you end up with a situation where the black sea bass pot being so much smaller, lighter; that is not the weak link.

DR. BARBIERI: Yes; very interesting. I hope that Nick will give a little bit of an introduction before he gets into the more technical description of the analysis that he did. I sure appreciate, Myra, you giving this presentation today, because it contextualizes this discussion much more for us in terms of that technical presentation on Thursday morning.

It explicitly identifies, as Mike said, what questions are being asked of us and what our role in this whole process is, so very interesting. Well, we are like five minutes away from four o'clock. Having looked at the agenda and having some awareness of who is around to give the potential presentations that we could have this afternoon, I think that we're going to recess for the day and return at nine o'clock tomorrow morning to have the hogfish assessment presentation.

Mike has already sent a draft of the notes from our ABC. Marcel, John, and I already have the draft notes that Mike put together with the main points that we're going to be addressing in our ABC Control Rule Workshop Report. I can resume to the comfort of my accommodations and kind of start getting into that preparation of that report. Then, again, we reconvene at nine o'clock tomorrow morning. Thank you.

(Whereupon, the meeting was recessed 3:55 o'clock p.m., October 28, 2014.)

The Scientific and Statistical Committee of the South Atlantic Fishery Management Council reconvened in the Crowne Plaza Hotel, North Charleston, South Carolina, Wednesday morning, October 29, 2014, and was called to order at 9:00 o'clock a.m. by Chairman Luiz Barbieri.

DR. BARBIERI: Good morning and welcome back to the SSC meeting. This is day two of our meeting; and as we discussed yesterday we had basically earmarked today to be going over a number of stock assessments that we need to review. We are going to start this morning with Mike Murphy from FWRI presenting the hogfish assessment that was conducted under the SEDAR review – and Mike may go into more detail about that – process; but the analytical lead was taken by FWRI. Mike, whenever you are ready.

MR. MURPHY: Just to prepare you for this, this is going to be a report on the FWC benchmark assessment for hogfish for U.S. waters. We're using available data through 2012. As Luiz said, the assessment progressed through an abbreviated SEDAR review. We had two data scoping workshops along the way in which data availability and stakeholder input was made available to the assessment team.

The assessment team consisted of Wade Cooper, Angela Collins, Joe O'Hop and Dustin Addis; all of the Fish and Wildlife Conservation Commission. The reason I'm giving the report today is as of late August Wade Cooper took employment elsewhere, so this was dropped on me just before we received the CIE reviews in September.

The CIE reviews just for accuracy and acknowledgement were done by Chris Francis, Paul Medley, and Jeff Tingley. I don't know if you've had a chance to look at those, but in the reviews themselves a number of issues were brought up about the assessment. I am going to talk about the three stocks that we did in the assessment and give more emphasis to the ones that they had a little more confidence in.

The upshot, if you haven't had a chance to look at that, was Chris Francis was critical of all three stock assessments; really did not think that they were qualified for use as advice for management. Paul Medley did think there was valuable information and was available for management.

Jeff Tingley found that there was some uncertainty in the assessment that wasn't accounted for and was sort of borderline about whether it was available for management. But you should look at those, they were very good reviews and helpful in terms of guiding work in the future. Now just as a framework for this, I am going to briefly go over some of the data used in the assessment; the time and spatial frame and some of the important life history characteristics that were important in determining the population dynamics of the stock.

We'll go into the first spatial aspect. Seifu Seyoum of the Fish and Wildlife Conservation Commission did a recent genetic study and found in samples taken from; I think it was 24 micro satellite loci of 720 fish or so, all sampled between the Big Bend and North Carolina. There were three discreet genetic clusters.

In the assessment we subdivided the information in order to do assessments on three different stocks; stocks from the eastern Gulf of Mexico that I am not going to talk about in this talk being that they are part of the Gulf of Mexico Fisheries Management Council's territory; the Florida Keys southeast Florida stock, which includes a group of fish on the north side of the Keys in Monroe County – the stock delineation in the assessment is the county line between Collier County and Monroe County; Monroe County being the county in the Keys – and eastward up until the Florida/Georgia state line.

You can see there is some uncertainty as to where the southeast Florida stock and the North Carolina through Georgia stock is separated; but for the purposes of this assessment, we used Georgia through North Carolina as the second South Atlantic stock. This is going to be a little bit rambling probably, so please feel free to ask questions along the way. It will probably be easier that way.

I don't know your standard procedure, but that would be a good way to handle it. The temporal timeframe; in the end the assessment used information from 1986 through 2012. This is an important issue brought up by some of the reviewers in that as you will see much of the depletion of the stock occurs before 1986.

You've got fairly small changes in this timeframe that are used to project the condition of the stock, which is certainly not the ideal situation. You can see that the Florida east coast, Florida Keys, FLK and Florida Atlantic is the dominant fishery in terms of landings in this region. It has declined throughout the timeframe, well, since the '86 period, and that the Georgia to North Carolina stock is much smaller compared to that.

Some of the reasons for starting in 1986 were certainly the very weak recreational data. The recreational fishery is a large component of these stocks, especially before '86 and some years you would be talking about five to ten intercepts of hogfish. That is why you see a lot of these peaks and spikes.

The smooth – the historic landings there are just an artifact of a modeling technique where the team used license information and catch per unit of effort for the most recent, reliable time period to approximate what is believed to be the historic catch of hogfish. An important aspect also for hogfish between these three stocks is the assumption in the assessment that hogfish in the Florida Keys, east Florida and Georgia/North Carolina stock grow similar to what is seen in the west Florida stock.

As you can see here, if you look at the red dots, the Florida Keys and the east coast, early studies have shown that they have much younger maximum age, an apparent lower asymptotic length they grow to at a higher rate, so a higher K associated with that than in the other regions. The interpretation here is that is associated with a juvenescence of the stock with fishing pressure.

Evidence for that was put forward in that there are studies in Cuba that find growth patterns similar to the west Florida stock. The Georgia and North Carolina fish, while they are all generally larger, seem to fall in line with the west Florida stock. This is an important aspect, although you'll see in the presentation that sensitivities are run to determine the impact of that on status determination.

DR. BUCKEL: Were all these aged by the same lab or were there different studies that could have interpreted annuli differently?

MR. MURPHY: No, these were all aged by the Fish and Wildlife Conservation Commission. The various different programs are used to collect them, either through specialized life history studies or sampling of the commercial catch and so on and so forth. Also, sort of in line with that difference in the maximum age seen in the Keys; it was assumed that the maximum age represented in the west Florida stock was an indicator of the natural mortality rate, and we used

an age-specific Lorenzen analysis with a maximum age of 23 years found in west Florida. Of course, hogfish are protogynous hermaphrodites, so the oldest aged females are about age 10.

Males can be found from age 1 through the maximum age. Everything after age 10 is a male. The maximum age found in the Florida Keys was 16. As I said, sensitivity runs were developed to sort of bracket this base natural mortality rate using different maximum ages. Another important characteristic as I mentioned, the protogynous hermaphroditism.

The graph in the upper left shows the size at maturity for female hogfish. This translates into maturity at age 1; essentially right away. In the upper right is the transition from female to male, and you can see that there is some transition even at age 1 after female maturation. This is all post-maturational transition up through age 10.

Their biology with the haremic behavior and the long number of age classes in the transition phase suggest that male sperm production in terms of spawning could be important. You will see that in the base case the status determinations were configured to use total spawning stock biomass; that is females and males, which is I think the typical thing to do if you've got some uncertainty about the sperm limitation issue.

Again, sensitivities are run about those assumptions, and I'll mention those later on in the talk. If we get into the commercial landings; here I show the extractions from the population from using the commercial fishery from 1950 through 2012. You can see that the Florida Keys southeast Florida stock was the dominant landings for a number of years.

These other fisheries have developed commercially while that stock has declined. While this looks impressive in terms of the Georgia/North Carolina stock, the recreational fisheries are much, much larger than the commercial fishery. In the end I am going to concentrate much of the talk on the Florida Keys, southeast Florida fisheries.

I guess I should say right now that the Georgia/North Carolina stock, given the weakness in the assessment, still the status was determined throughout all of these sensitivities that they were not overfished. Those fish are generally much larger in the harvest than in the Florida Keys stock. I just wanted to put that aside if you think I am giving short shrift to the Georgia/North Carolina stock. I'm going to concentrate on the Florida Keys/southeast Florida stock.

You can see in the lower graph that has been made up mostly of spearfishing since the mid-eighties, early nineties. In all of those gears, the three main gears, spear, hook and line, and pots and traps, you have seen a consistent decline since reaching a peak of over 40 metric tons in the early 1990s.

Now, just to give you a flavor of some of the data that went into the assessment, we had lengths and ages from the commercial fishery. You can see that this gives you an idea of the length distribution that was sampled from each of the three main fisheries. They were moderate to poor, but you can see especially at the age composition samples from these fisheries were very sparse and not really informative in terms of trying to estimate growth separately for this stock from the other stock.

As I said, the majority of the landings are in the Florida Keys, so I will talk a little bit more about that here. This is the recreational landings, and you can see the total landings commercially were six metric tons in the most recent years. These are about 160 metric tons, so this is a far dominant fishery. The spear fishery is the dominant fishery at the current time.

The landings have apparently declined since the 1986 era, but have been quite variable; and if you believe the historical landings information, much lower than the early data. One thing I should mention here, of course, these came from the MRFSS/MRIP data. There was a lot of effort in this assessment.

First of all, you had to post stratify the MRFSS data to include Monroe County on the Gulf or Atlantic States specific estimates from the original MRFSS data; then there is the calibration to the new MRIP data. This is all caused, even with the already high proportional standard errors causes very high standard errors in excess of 50 percent in many of the years.

In a base case, a median standard error for the 2004 through 2012 period was used for those years to sort of stabilize the analysis. I will show sensitivities on the effect of that assumption. One of the reviewers was quite critical of using any proportional standard error for catch and really made probably a decent case for looking at catch more in terms of how we traditionally look at natural mortality where you would have a high series or a best estimate series and a low series, all with tight standard errors, with the idea that these assessments do not perform well if at least the catch is not known fairly well.

This is just a quick look at the sampling from that fishery. Again, you can see very poor length sampling from the two recreational fisheries and nonexistent really age sampling. One of the things that is also critical in this assessment is somewhat qualitative limitations put on the types of selectivity functions that are available to each of the fisheries.

The information on the sizes typically caught in the fishery, the geographic location of the fishery relative to the distribution of the stock were used to make the assumption that asymptotic selectivities were part of the exploitation pattern for the commercial trap and the commercial hook-and-line fishery. That is an assessment again that is evaluated in sensitivities.

All the other fisheries except for the commercial spear were assumed to be dome-shaped that ended in a far right-hand level at zero at some point. The commercial spear fishery was actually estimated within the model; so that could have chosen selectivity that would have been asymptotic or dome-shaped. Here is sort of in between where it is dome-shaped selectivity, but the terminal selectivity with size is about 50 percent; and on the right are the derived age-based selectivities for the fleets.

I should mention here the selectivities for some of the surveys are here also. We had a remote, what was it called, a survey in the Keys of diver counting hogfish survey that looked at fish in less than 30 meters of water; so that was considered to be a dome-shaped selectivity, because that doesn't include the deeper waters where larger hogfish occur.

The Tortugas survey, where there are much larger hogfish, was assumed to be asymptotic, because larger hogfish were available in that area. Again, these assumptions will be challenged in the sensitivity runs. This is the fit to the landings by fleet. It is a busy slide, but you can see

the fits were all fairly tight because of the basic assumption of restricting the proportional standard errors especially to the recreational fishery to median levels found from the MRIP data from 2004 through 2012.

Of course, the commercial landings were all fit very closely with much lower standard errors. Just to go over very quickly some of the indices of abundance; nine indices were examined and six were included here in the assessment. The MRFSS/MRIP catch rate data for spear gear and for hook and line were looked at separately.

Both show declining trends early in the period until the mid-nineties. Those were fit fairly well by the model, but there were variable fits after 2000. The top is the observed data or the points, the nominal data. The line is a standardization of that index. Then the bottom shows those standardized points with their errors associated with them; and then the line is the model fit to those indices.

Some biomass indices were taken from the Florida trip ticket catch rates and the commercial logbook data operated out of the Southeast Fisheries Science Center. These fit fairly well through the time period, both showing that decline in the mid-nineties through early 2000, but then a much more level period after a rebound after 2000.

DR. REICHERT: A quick question; remind me, Mike; those blue vertical lines, what do they indicate again?

MR. MURPHY: I'm sorry; that was me lining them up as sort of a visual tool to put the appropriate years on top of each other. In other words, 2010 on the top graph is directly above 2010 on the bottom graph. Those are just –

DR. REICHERT: Visual aids.

MR. MURPHY: I guess I could have erased them, but it helps me to count over; okay, that is 2010. Sorry, I should have mentioned that. Finally the fishery-independent index available for the assessment was in the Florida Keys. This was the roving visual census survey done by the Southeast Fisheries Science Center and the University of Miami cooperatively since the late nineties.

What you see in the Keys portion of this; that would be the dark blotches or the dark points in the upper right is a big change in 1999. In the interpretation of this and in talking to the researchers on that survey, there were improved habitat maps, a change in the stratification, design changes that the assessment team interpreted as a significant change in the catchability in the survey. If you look at the model fit in the lower left-hand graph, you will see a direct line between 1999 and 2000.

That is just a shift in catchability estimated within the model, essentially breaking this into two surveys over these two periods. These data show that the abundance of hogfish in the Keys and in the Tortugas was estimated to be pretty stable since 2000 with some fluctuations. Of course, the Keys are generally younger fish represented here; and the Tortugas would be older, larger fish. But in terms of trends, there didn't appear to be any strong up or down trend.

There were a lot of length data available from these surveys as they estimate the lengths of fish caught. The Keys surveys you can see are a lot of predominantly Age 1 hogfish in that survey. It was used as a young-of-the-year survey for a class that they identified as pre-exploited phase in the survey data.

Not to bore you with a lot of fits to length data, but the overall length composition data for each of the fisheries and surveys aggregated across all the years fit pretty well. As you would suspect with the sparsity of the data, the year-to-year fit was often much worse. An interesting thing that came up here that will be mentioned a little bit in the sensitivities I'll go into is that steepness was estimated fairly well for the Florida Keys stock.

This is a profile of the different components of the likelihood against the estimate of steepness. You can see that the black line is the total likelihood, and you can see that it looks like steepness is estimated pretty well there. Most of the influence on steepness comes from the length data. The length data mirror that. That is the green line that also shows the same kind of resolution to that level of steepness.

But the red, the age data seem to want a lower estimate of steepness. This conflict was investigated quite a bit by the team in terms of using only age data, using length data, weighting. I bring it up because one of the main reviewer criticisms was that – and this was Chris Francis, who has a lot of literature on this – felt that the length data were too heavily weighted, too influential in the assessment results. I just bring that as a point that you can think about.

Now we'll go into the results of the assessment. That was an attempt to try to show you all the pieces and the warts and the good parts. This is the overfishing status determined from this information, but first go into the two graphs on the right. The estimate of fishing mortality rate, you can see that fishing mortality was estimated to be higher early in the period, dipped down in 2000, but then to increase and to remain quite variable through the end of the assessment period, 2012. No super huge trend there, although it appeared that fishing was a little bit higher in the early period.

The lower graph on the right also shows the trajectories for the population, seeing that the population abundance was higher prior to '94, then became reduced by half, and fairly stable, maybe a little upward trend through 2012; whereas, the catches have, as we've seen, declined through that period.

This has all resulted in the graph on the left, where this is the – it is cut off, but the SPR, the F-30 percent, F-35 percent and F-40 percent are represented by the three horizontal red bars. You can see that the spawning potential ratio has been below most of those through about 2000. In 2000 there was a depression in fishing mortality rate you can see up on the upper right-hand side, which allowed an apparent recovery for a little while up over the F-35 level, and then back down.

You see some variation, but all generally below the SPR level associated with each of those three benchmarks. To throw in just the rough estimates from the Georgia/North Carolina stock, the F to Fmsy ratios for that stock were all generally less than one. But you see some dramatic peaks, and these are associated with enormous changes in catches that were considered to be unrealistic based on the MRFSS estimates.

I don't know if I've mentioned, but the numbers of intercepts in that region that contain hogfish are in the tens, if not less, for most of the period. It is quite variable and not really reliable. The F to Fmsy ratio for the southeast Florida Keys, all generally above one with those occasional dips as we saw in the SPR; and the ratio to the minimum stock size threshold at MSY for the spawning stock biomass was at about a half during the whole period.

DR. ERRIGO: I just wanted to ask for clarity to understand the graph. The dark thick line; is that the estimate from the base model; and then the scatter around, is it an MCB analysis or MCMC runs or bootstrapping?

MR. MURPHY: Those are bootstraps around; and that would be the deterministic, yes. Thanks, that was important. That gives you an idea of the certainty of those estimates there. Here looking at it a little bit differently, just for the most recent year makes it clear that the upper graphs in both of these columns; one is the spawning stock biomass in 2012, the other is the average F for 2010, 2012.

You can see that the spawning stock biomass is below the minimum stock size threshold, and the Fmsy. The Fs are above the Fmsy for the most part, and then there is the distribution of the ratios, just to get a feel for the variability in the most recent estimates. Now I was going to quickly go over the sensitivities.

This isn't going to take a lot of time, but I wanted to show you at the very least the ones that were significant in status determination changes. Certainly, it would have been better in an assessment to have the historic data used in the model. An attempt was made for the southeast Florida Keys stock to use back through 1950. That model did not converge, so the earliest data that could be used was 1981. We changed the initial condition from 1986 to 1981.

I'll go into it, but just as a prelude that significantly changed the level of depletion in the first year. It implied that the starting year can have a pretty strong effect on the status, and we'll look at that. But here is a list of them; I am going to talk about them individually. This is just to sort of set it up.

Here we have – this is going to be the standard for all of these is the F over F-30 percent and the spawning stock biomass over the minimum stock size threshold at F-30 percent, the red line being one. You can see that the change to 1981 had very little impact on the fishing mortality ratio, the overfishing state, the overfished state. The change to 1981 actually pushed it into a lower ratio, so a more overfished state using 1981.

I think because of the productivity of the stock was estimated to be potentially higher. The assumed error for the recreational time catch that I've mentioned a little bit about using the median MRIP PSEs; here the calibrated PSEs were year-specifically used, and the original MRFSS PSEs were used also.

You can see there is very little effect on these in terms of status determination. The steepness prior – as I mentioned steepness and showed the profile likelihood – the steepness was well estimated by this model. Actually a prior was used for some of the other stocks, the Shertzer-Kahn prior; and when that was not used, it still estimated a very similar steepness value. There was no change in status because of this assumption.

Now removal of the asymptotic selectivity for those that had asymptotic selectivity prescribed to them did have a significant effect as you would expect. The asymptotic selectivity gives an assumption and certainly implies a more precautionary conservative worst case scenario, if it is not indeed true.

In these cases the length data were allowed to inform the model of the terminal selectivity. All the models used the double normal potentially asymptotic, potentially dome-shaped selectivity; all resolved to dome-shaped selectivities. You can see the overfishing status sort of bounces around below the one ratio, so not in an overfishing status; and the overfished status changes completely to not being overfished based on that assumption. That is an important feature there.

The reproductive characteristics; there were several different studies that have been done on the biology and life history of hogfish. You see a lot of interesting things in terms of differences in maturation by area, by depth; and in this case a lot of those were incorporated into the model. I guess the most significant here you would want to look at is the idea that using female spawning stock biomass all by itself and a lot of these female-based characteristics in a model that in this run wouldn't use the male and female spawning stock biomass does significantly affect the status determination.

You can see in the top one the base case shows overfishing consistently through the time period. That is the upper line with the Plosses; and then all of these other sensitivities seem to go below the overfishing status. Yes, that makes sense in terms of the younger not having to have that old male biomass.

The status determination in terms of minimum stock size threshold also changes to a not overfished status. Age data; I mentioned there was this conflict apparently in the steepness profile. Work was done to use no age data to estimate growth separately, to use the length conditional on age data approach. These had some effect.

Specifically the growth estimate seemed to move the overfishing status to a less severe case. This allowed the Florida Keys, east Florida information; in other words, that difference in growth we saw early on in the presentation to come through and be used in the analysis. In this case, this would make the assumption that difference in growth was actually real, and for some reason genetically the different stocks had significantly different growth between, especially the Florida Keys and the eastern Gulf and the Georgia/North Carolina stocks.

This was a look at the various life history differences, the natural mortality rate changes, alternate maturity functions, and alternate growth functions. You can see that the range of natural mortality rates used really didn't change the status in the sensitivities. The alternate maturity function, which would be associated with just data from the Florida Keys, didn't change the status.

Actually it is this one. I misspoke when I talked about the growth in the last slide. This is the growth estimated for the Florida Keys separate from the other stocks. This had a significant impact in terms of changing the status. If that assumption about the growth being the same as the eastern Gulf of Mexico stock is incorrect, this shows the effect of that assumption. Those are all the sensitivity analyses I wanted to go over.

Just quickly some other uncertainty that was looked at in the assessment was retrospective analysis; and there was really none in the Florida Keys, east Florida one. You saw some in the Georgia/North Carolina weaker assessment. Of course, that is fishing mortality rate so it is sort of in a positive way, the type of adding more data would imply that past F rates were overestimated and not underestimated.

This is I think my last slide. I debated whether to put this in here, because this was done outside of the CIE review. The idea was that this panel would, if indeed felt that this assessment did provide information, would need some idea on rebuilding to the various different benchmarks within the ten-year timeframe.

This just gives you a rough idea of the catch levels in terms of metric tons that would be needed to meet an overfishing limit that would allow you to rebuild to these four prescribed benchmarks; Fmsy, F-30 percent, F-35 percent, and F-40 percent. I am not going to go into any details, so you can ask questions if you feel it is necessary.

But just to give you an idea, in 2012 the total harvest in this area was 155, 156 metric tons. Preliminary estimates for 2013 appear to be over 100 metric tons, so these are all significant reductions in 2014 and '15. That is a summary of the assessment. There is a ton of information here; and again I advise if you wanted to really get a good quick summary, the CIE reviews, especially their summaries went over some of the strong and weak points and I think had a lot of good points that should be kept in mind when deliberating about the assessment. Thanks.

DR. BARBIERI: Before we open up for questions and discussion, I just want to draw your attention to the action items that we have outlined in our overview document. Keep those in mind, so at the end of our discussion those are the things we are going to be reaching towards:

Review the assessment and consider whether it represents the best scientific information available; identify and discuss assessment uncertainties; and then provide fishing level recommendations. We have a table that we usually complete with those outputs and estimated quantities, deterministic and probabilistic results of the assessment for summarization of presentation to the council. With that, I will open for questions.

DR. GRIMES: Mike, tell me again what the justification was for that growth was constant over the whole latitudinal range that you examined. Isn't it pretty typical that species that occur over a wide range of latitudes like that; the ones at higher latitudes attain larger sizes, and mature later and that sort of thing?

MR. MURPHY: Yes; that certainly could be thought of that way. The justification was in some early work by Rich McBride and others. Their review of the literature found that growth in Cuba – and a lot of this is based on determining age with other parts like the urohyal bone or something like that – was much more similar to the Eastern Gulf and North Carolina than to the Florida Keys.

It was known that certainly there is an intense fishery in the Florida Keys, so the assumption based on those observations were that with the truncated age structure maybe we would expect to see smaller parent sizes at age based on the fishing rate rather than a natural difference in growth.

A point to your question, though, this came up over and over in the reviewers as something that was quite critical, and you saw that in the sensitivities. One of the things that was really mentioned was that growth is used in two ways in an assessment. One is to take the model predicted or model formula calculated ages and then turn them into biomass – or lengths and turn them into biomass through ages.

Another is to match up the length frequencies from the fishery. I think what we saw in terms of the steepness having this conflict between length and age was that those two functions don't always work very well in the Keys, because the lengths at age are so much different than any other areas.

DR. SCHUELLER: Since there was this conflict between the age and the length data, I was wondering if there was any work not included in here related to weighting. My assumption is everything was weighted at a one. Did you consider reweighting such that the SDNRs standard deviation of the normalized residuals was one? Then, also, maybe it is in here and I just didn't see it; is there information about effective sample sizes that came out of the model for these data components then?

MR. MURPHY: Yes; the effective sample sizes were calculated in the SS3 runs, but were not necessarily used directly in revising the sample sizes to the data inputs. That was a criticism by the CIEs. In terms of weighting; that is a huge issue also by the CIE reviewers in that they felt there needed to be a lot more work trying to understand the effect of different weights.

I did not see in my review of all of the files and information, in talking with Wade, we never talked about the use of the natural standardized residuals for looking at trying to estimate appropriate weights for these different factors. The impression from CIE reviewers were that the length data were way overweighted. They should have been down-weighted much more significantly than they were.

I think in general rules of square root, or nothing can be greater than 200; did down-weight the data from its observed levels, but maybe not significantly enough. I think some literature was cited in the reviews that some of these should be down-weighted. You may have 200 fish in a year measured, but the effective sample size should be three because they are not distributed across time and space.

DR. BUCKEL: The model was really sensitive to the selectivities, and I was curious about some of the reasons for these dome-shaped versus – I think a sphere had that in the larger animals it was like 50 percent. Was it behavioral where the larger fish are leaving the area that the spear fishers are going to or is it the depth that those bigger fish are in deeper waters that they can't access. I am just curious about what are the reasons for those.

MR. MURPHY: That is the rationalization is that the fishing effort in particular fleets have characteristics that don't always overlap the entire distribution of the stock. For instance, hook and line they felt comfortable as being asymptotic, because hogfish are generally caught as a bycatch; and a lot of these hook-and-line fisheries occur all the way across the shelf, well outside of the depth range of hogfish so that in theory the largest hogfish should be available to them; whereas divers, of course, are restricted by how far they can reasonably dive; and you may get some real expert divers that are able to go almost to the edge of hogfish depth range, but

certainly not just your common diver. That was the justification for the initial selectivity form choices.

DR. BUCKEL: Just to follow up on that; if I remember correctly, the recreational hook and line was dome-shaped but the commercial hook and line was asymptotic, so is that again based on the observations that the recreational fishers just aren't fishing in the deeper water?

MR. MURPHY: Well, as I mentioned, there was the observation of that but also the observation of the size structure of the sampled catch. The recreational catches generally did not have the largest hogfish in abundance as did the commercial hook and line.

DR. SCHUELLER: I'll follow up on the selectivities. These were I am guessing, since it is SS3-6 parameters; double normal –

MR. MURPHY: Correct, yes.

DR. SCHUELLER: Some of the parameters were fixed, right?

MR. MURPHY: Yes; the ones that were assumed to be dome-shaped, where the terminal selectivity was zero, the terminal selectivity was fixed.

DR. SCHUELLER: The terminal selectivity was fixed at zero?

MR. MURPHY: Yes.

DR. SCHUELLER: So there are sensitivities in here with zero and forced logistic on some of them, right? No; logistic was allowed to be domed, right? The base had two logistics, the rest domed.

MR. MURPHY: With one exception being the commercial spear, which was allowed to estimate all parameters.

DR. SCHUELLER: Okay, so my question is those that were fixed to go to zero selectivity at the terminal size or age or size in this case, it looks like, why zero? I guess I think there is a lot of information out there potentially to lead to some suggestion of doming given the length compositions that you have. But my concern is the extent of the doming and with those values fixed at zero; so I guess I am asking why are they fixed at zero over other values considered?

MR. MURPHY: I would really have to go back into the details. Through the readings of the report and discussions, my feeling is that in those fisheries you would never see in a length structure of the samples fish that were large. It was considered that this fishery either occupied an area where they never encountered the largest fish, so it had no access to them; and the evidence would be at the size structure that they sampled. I can look and try to get back to the committee and find out from Wade as to exactly the rationale for that and make sure I didn't misspeak, but they all did go to zero. I believe they were set that way.

DR. BARBIERI: Any other questions?

DR. SCHUELLER: I have a lot of questions.

DR. BARBIERI: We like questions.

DR. SCHUELLER: If we're following off the selectivity piece of it, when Mike asked about the figures on there, the dark line was the base and the other lines were bootstraps, correct?

MR. MURPHY: Right.

DR. SCHUELLER: So in those bootstraps did you include any uncertainty in selectivity parameters? For instance, these were fixed. Was there any sort of distribution or anything assigned to that?

MR. MURPHY: I think in all the parameters that were estimated, yes. The uncertainty was still allowed, but you still had the restriction that forced that particular form of a double normal, too.

DR. BARBIERI: To that point, Amy, you can see when you look at some of those outcomes of the different bootstraps, you can have a fair degree of variability of the final outcome of the trajectories would be given those different combinations of parameters.

DR. SCHUELLER: You can; I'm just thinking about it in a context of the amount of uncertainty that is characterized in that distribution and what that includes and doesn't include.

DR. BARBIERI: That was my point actually. For us to have an idea, since things seem to have deviated from that at times as you look at some of those trajectories and you see those clouds of bootstrap runs deviate; how much is that actually capturing or not?

MR. MURPHY: Yes; you can be assured that it doesn't capture all the uncertainty because of these assumptions. Again, I think that was a theme throughout the CIE – all three reports were that there were levels of uncertainty they didn't feel were captured; but I don't think that is uncommon in most assessments.

DR. BARBIERI: Do you have one more, Amy?

DR. SCHUELLER: I have a couple more, but I don't want to monopolize the time. Okay, so here is my next set. I am going to move to the indices, I guess. Looking at the indices, they seem to me to sort of be all over the place. My question is was there any sort of analyses done looking at correlations amongst the indices and how much they share sort of common information or don't?

Then given that; at least they appear to my naked eye scrolling through this that they don't share common information. Were the indices prioritized in any way or was there any consideration given to excluding some?

We know indices are meant to reflect abundance over time; and if they all have, at least to my eye, different trajectories, which one are we supposed to believe, because the model is going to just split the difference? This is one of those decisions that need to be made apriori.

MR. MURPHY: And I'll say in terms of the Eastern Gulf stock; that was done in the sensitivities. Different indices were removed mainly because in that stock there were a large number of fairly short timeframe fishery-independent surveys that were giving quite different results, different trends.

In this one there is some commonality in that early nineties dip in all of them. I have to get into the head of the analyst, but there is no indication that there was an other than logical thought process about the aerial coverage of the survey relative to the stock, the temporal coverage, the data availability, about whether the index would be included or not.

There was not the second step of looking at all of them and excluding those that may be conflicted. In fact, the second CIE reviewer from the UK, Paul Medley, was highly of the opinion that there needed to be a lot of work external to the assessment to pare down those two – the logical indices that were measuring same population. That might not be a satisfying answer, but that is all I could think of in terms of that.

MR. CARMICHAEL: I was thinking more philosophically when you showed the status of the Keys, when you used the Key specific growth, and it showed it a lot better; then you made the case early on that the different growth observed and whether or not that is just a function of the exploitation.

It just seems like in this case you have the luxury of looking at other stocks, so we can potentially avoid making an overly optimistic decision on this stock. In thinking bigger picture in other stocks, I wonder maybe if you had the ability to use more of that older data and have shown the stock at a higher exploitation and shown it getting fished down; I wonder if that would make less chance of making that type of error and assuming status?

It just seems like there is a potential risk there. We have a lot of stocks that are heavily overexploited. When we assess them, they are, and it just sort of raises a little bit of a red flag I guess in how we deal with it. I'm wondering if there are other pieces of information and if you had them, would they reduce that risk.

MR. MURPHY: That is a good philosophical question. Certainly, the idea that if the growth was not as productive as implied by the Eastern Gulf of Mexico stock, you have as you saw quite different determination of the stock status. I guess it comes down to even if growth is changing over time; is that now locked into a different growth pattern because of the overfishing or is there some genetic component of that growth that is actually now incorporated into that stock or will it rebound if fishing is relaxed? Will it rebound to another growth pattern that was sort of similar to the Eastern Gulf; I don't know. Assumptions have to be made in those cases.

DR. SMITH: Mike, I hate to beat a dead horse, but that is what I'm going to do. I've got some issues with the growth model. In my view that was the biggest issue in this assessment and may be fatal. I was looking at the observed versus predicted growth, and there seems to be a lot of variability in that plot. But then I look at the control file and I noted that the CVs that are assigned to the growth model parameters are pretty low; they are 0.05. I am pretty sure those aren't the actual estimates of CV from that growth model, so I was wondering what went into that decision to place all that certainty on the growth model.

MR. MURPHY: I wish I could tell you because that jumps out at you right away. If you look at just the blast of points and looked at the CV at a large size and at a small size, they were larger than are in the input data. I don't see anything and haven't discussed with Wade about it anything in the report indicating why they did that.

I can find out; and just be truthful maybe it was picked out of the air as narrow enough. Maybe there were some fitting issues. I know if you have that CV too high sometimes these models don't converge very well. I did not see enough discussion in the text to be able to answer that.

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DR. SMITH: That might be something that could be explored to sort of address this issue with the growth model. If you bumped up those CVs; in some people's view that might account for some of that.

MR. MURPHY: The difference between the areas?

DR. SMITH: Yes.

DR. SCHUELLER: Was there a likelihood profile done on the R zero estimate and do you know if that was done and then do you have any idea?

MR. MURPHY: It wasn't done. In fact, I tried to do it last night, but it just didn't work. It is a significant issue in terms of – as I introduced the talk, the original state of depletion at the start of the Florida Keys analysis – I think it was pointed out by Chris Francis in his review – is at about 9 percent of the virgin level.

By the end of the analysis in 2012, it might be down to 8 or 6 percent. You are seeing this little tiny wiggle in a population and trying to infer its productivity and status, which is a very dangerous thing to do. As I mentioned throughout the talk, because of the weakness in some of the early data, those were the decisions that were made.

DR. SCHUELLER: Can I just ask you why do you think it didn't work? The model wasn't converging?

MR. MURPHY: No, it was my ineptitude with some of our programming that I needed to run.

DR. SCHUELLER: I was just curious if it was a model convergence issue.

MR. MURPHY: It might have been a combination of Kansas City having a big inning, too.

DR. SCHUELLER: My only other comment on that is sort of all these pieces are intertwined with the weighting question. Chris Francis; we all know he is going to bring up weighting. The weighting affects the indices, and the indices that you choose are important and then those all relate to this R zero estimate, too, and your steepness. It is sort of all intertwined.

This is my first SSC meeting. We have some action items; you've already stated that. One reviewer said, oh, one said yes and one said middle. How are we as an SSC supposed to – typically you would have an in-person review and there would be like an overall report from all

of them together, so that can guide you more. With this, with it being sort of one of each; how are we supposed to deal with that, I guess?

DR. BARBIERI: This is really more a procedural SSC. Before we go to that, Amy, which is a very good point and we are going to have to cross that bridge; but I just want to make sure do we have any more questions for Mike on the technical issues associated with the assessment?

DR. BELCHER: Just one quick question, because I know this is kind of something that we've been building in with some of our other assessments. Was there anything like a surPlos production or something of a lower model that was run as well?

MR. MURPHY: There were in the terms of reference requirements to run yield per recruit, and that was done. Early on there was work on some of the simpler depletion-based data-poor models. At this time I don't have enough information in my head to be able to tell you where those went, but they are not in the report.

DR. BUCKEL: Mike, correct me if I'm wrong; but one of Rich McBride's papers – and maybe you were on that – you guys did a catch curve and estimated a Z; is that right? That is for the Eastern Florida/Keys stock?

MR. MURPHY: Right.

DR. BUCKEL: There are all these different sensitivity runs that have shown things all over the place. Did you compare that Z to some of these different outcomes to see, well, maybe this is the one that is giving us the best indication of F, F Plos N.

MR. MURPHY: No, I didn't do that. My recollection though is that the Fs out of – well, I shouldn't say anything about that. That analysis I think was a GCFI look at yield per recruit and using a catch curve to kind of position yourself on the yield-per-recruit graph. It suggested that a change in the size limit would actually increase the yield.

Of course, with no implications for what does that do with spawning stock and all that. I can't recall if that is even close to the fishing mortality rates estimated in this model. I have a feeling it is, but I would just be guessing right now. We can look; it is a good point.

DR. SMITH: This is more of a comment than a question, but just to sort of touch on what Carolyn was saying; when I read this, I felt like it was sort of screaming data-limited assessment, given all the issues that we've been talking about. I was kind of surprised that there wasn't some comparison to a production model or something like that.

MR. MURPHY: I think the logic behind the approach was, well, first of all, to see if it is possible. That is an assessment kind of scientist thing to do. The complexities of the biology of this animal in terms of being protogynous, haremic, and the complexities of the fisheries and the changes over time, I think it might have been thought that some of these simple data-poor models wouldn't be able to capture those dynamics in any kind of a reasonable fashion.

I know in discussions with Wade just in the hallway, we've talked about data-poor models for a lot of these. Especially the issue of having to sort of pull out of the air where is your starting

depletion level; and it wasn't really felt that we could do that for hogfish. This may be sort of a step to inform us that maybe it is in this range, and maybe we could go back with estimates from this. But the beginning depletion level is usually critical in those data-poor models. That is sort of a comment back. I don't have any true answer to that.

DR. VAUGHAN: This is more of a comment than anything else. It just strikes me that there may not be enough contrast of what is going on to really determine status of stock or whatever, especially with all the noise thrown in on top of it, the issues with growth and selectivity and everything else.

MR. MURPHY: Yes, there were reviewers that agreed with you.

DR. BARBIERI: I think we have explored the assessment and addressed a number of questions. I think the thing is crossing that bridge of how we are going to address our action items here. Before we go there, to Amy's question or point about this, this is something that SSCs have to deal with from time to time, especially in the situation where we have a SEDAR assessment process but not a typical SEDAR process.

We made a deliberate – basically Florida made a deliberate choice and tried to work with the SEDAR program in terms of setting something up that we felt was reasonable instead of having the three typical workshops, including a review workshop that would have SSC participation and all of that.

This is one of those things is there is a cost-benefit analysis about how much can we always use the Cadillac process versus something that is more abbreviated. In this case we made the judgment call to go with an abbreviated process. There are advantages and disadvantages to having a desk review; and one of those is we get three separate reports.

We don't have a cohesive review workshop report that sort of tries to go beyond just the three CIE reviews. Usually we get a three CIE review reports, anyway, but we also get that cohesive review workshop report. We kind of have to function as that SSC Review Panel and kind of make those judgment calls ourselves here in terms of what we believe is the way to go in terms of accepting or not accepting. There is no other way to do this other than to say, well, that is the way we are going to have to go and make our own value judgment about how to make a recommendation.

MR. CARMICHAEL: About all I would add is to say the review reports are recommendations to you, and the buck stops with you. You determine if it is best scientific information and you make recommendations from it. Use them as advice, but you are not bound by what comes out of those CIE reviewer reports, and you never have been.

DR. BARBIERI: Because the nature of the questions and actually the nature of the situation and the fact that Wade, the lead analyst for this, has left and couldn't be here, all of this creates an additional complication. I just want to put it out there and explore.

I haven't discussed this with Mike specifically, but if there is – this is just my own personal opinion; but if there is some general feeling from the committee that perhaps sending this back to the shop, the committee can present and has already presented some valid comments and

suggestions. We could see handling some of those and bringing it back to the committee. We already have the three CIE review reports and those are going to have to stand on their own; but it doesn't mean we cannot take this back, try some of those things, and try to refine a little bit more of the analysis and bring it back in April or bring it back in October.

MR. CARMICHAEL: Yes; you have always had the ability to ask for additional sensitivity type runs. In the standard situation where you have a review panel and they put forth a consensus or at least everyone can more or less live with base runs; then going back and changing that does create issues. I think here it is a little bit more of a gray area given the desk review, given that the reviewers were not unanimous. I think if the folks that did the model are willing to look at it some more and this group can provide some guidance, I don't see why we wouldn't do that.

DR. BOREMAN: I guess I will express some frustration with that; because we don't do many of these here. In essence in some ways this is like kicking the can down the road. The alternative approach is just to increase our interpretation of the uncertainty in the assessment and add an additional buffer because of the conflicting parts of the assessment and additional work needs to be done. Delaying it until April doesn't help the council much. Then we will have other assessments to deal with in April.

DR. BARBIERI: No doubt; I think that is a valid point. Given the particular situation here, I thought that putting this as an option was something that would increase the committee's ability to think about all possible issues.

DR. REICHERT: I agree with John; I think we have another tool is to request an update in two years, three years, which could potentially address these issues. I think we have two tools. One is in our ABC Control Rule. The other one is acknowledging the uncertainty, acknowledging the issues that we discussed; and then say given that situation, maybe we should request an update sooner than we would normally do if we have less uncertainty. That is another tool that we can use as an SSC.

DR. GRIMES: I was just going to agree with John, too. I think given the point Doug raised about the lack of contrast and the indices and so on; if you send it back; what is there is what is there and they are not going to be able to reach much stronger conclusions that would support a decision any better than making one now. I guess we could let the fishermen go out and tell them to go out and hammer it real hard. Then you get some contrast; and you might be able to tell what they could stand. Anyway, that is my two cents.

MR. CARMICHAEL: I think those are very good points. I would think if you felt something were done wrong and needed to be corrected, then that is a really good reason to send it back. Otherwise, if it is just tweaking and there is no new information to bring into it, I think he is exactly right.

You are unlikely to get anything more out of it in dealing with the uncertainties and accounting for them here. Giving good recommendations on the next assessment would probably be more effective and certainly much more efficient in the long run. Better to get an update in a couple years than go spend six months trying to polish this a little bit more and then still need an update.

DR. SCHUELLER: I am agreeing with everybody. Right now this is considered, what, best available science. That doesn't mean we can't make recommendations. As Marcel, an update, I don't know that I would say two years.

My only major concern is Chris Francis' concern, and it is the weighting, only because I know from experience when you do that iterative reweighting, sort of put those data pieces on sort of an even playing field; it can have a big impact. But I don't' know if that will be the case or not with this. People have made the point that there isn't contrast in these data, anyway. I don't know, it seems like something we might just have to live with for now and make a recommendation for a future update.

DR. BELCHER: I guess to me, if that is one of the statements that we're making about lack of contrast, I still would like to see what the lesser of models – I say lesser in the sense that lesser requirement model would have. It is helpful in certain instances to see that.

DR. REICHERT: That is what I meant in terms of an update that could include investigating different assessment models, which would address the point that Carolyn and Chris made. I do realize if you do an update in two years, you may not add enough information to come up with a different result; but that provides some time to investigate potential other approaches.

DR. BARBIERI: Okay, folks, good discussion. We're going to have to make a decision here in terms of going through our action items. The first one is consider whether this actually represents the best scientific information available and it can be used for management advice or if the committee feels that there are enough problems with the assessment, perhaps, in terms of just the vast amount of uncertainty.

We know for a fact that the hogfish fisheries provide a phenomenal amount of uncertainty, and there are some particularities there. That is the nature of the beast. The first attempt to – I think this was SEDAR 3 or 4 or something like that when there was an attempt to conduct –

MR. CARMICHAEL: Yes.

DR. BARBIERI: SEDAR 6, yes, to conduct a benchmark assessment of hogfish failed because there were so many issues in trying to conduct a quantitative assessment of a fishery like hogfish; that things just couldn't be put together in a way that the review panel felt was credible.

MR. CARMICHAEL: That was a length-based approach I think, wasn't it?

DR. BARBIERI: That was length based.

MR. CARMICHAEL: I think that is a little information about the performance of some of the data-limited techniques. That was a data-limited technique length-based approach, and it didn't pass reviewer scrutiny. Maybe that lends a little credence to Wade's decision to say let's try this model at least.

DR. BARBIERI: With that, folks, is anybody willing to stick their neck out there or do we need additional discussion? I think we have really discussed and explored the discussion. Right now, to summarize and help you kind of think through this; from the committee there are some

comments that basically say, well, I don't think this is fully cooked yet. If this goes back to the shop and we try this or that; that would be more credible. I don't think there is enough here for us to give this the thumbs up. Others feel, well, I'm not sure sending this to the shop would provide anything really different. Since we have a way to really consider those uncertainties in our catch advice, perhaps that will be the way to go.

DR. BELCHER: There is some further information I think that would kind of help me is what is the current performance of the fishery relative to the ACLs and the establishment of ACL has currently been done how; or at least our estimate to how we are recommending ABC, not so much the ACL. But as far as what is the number based on now, what is the performance of the ACL relative to catches?

DR. BARBIERI: Well, right now we have an average landings. I don't think that this even really qualified for ORCS, or did it? I cannot remember one way or the other.

MR. CARMICHAEL: Yes, it did; I think it was ORCS.

DR. BARBIERI: It did. We have an ORCS-based average catch as catch level recommendation over that time period that was chosen by the ORCS Workshop Panel. In terms of performance of meeting the ACL, I guess JC is pulling this up right now.

DR. REICHERT: I think that is a good point, because I think one way to approach this is what we are having now an improvement over what we did with hogfish when we set the ACL. I don't know; maybe it helps us looking at this in terms of where to go and how to go forward.

DR. BELCHER: I was thinking about that relative to the time window. I know the idea of kicking it down the line isn't exactly what we want to do. Currently we're not hitting the ACL to where we're feeling that the landings are too constrictive. Are we going to hurt anything to wait another three or four months?

DR. ERRIGO: Okay, what you see there is the blue line on the top is the total landings and the black line is the ACL. We're not hitting that ACL. The total landings, I think the total ACL for the South Atlantic.

MR. CARMICHAEL: That is coastwide, right?

DR. ERRIGO: That is coastwide.

MR. CARMICHAEL: It could change considerably if you went with separate stocks, and you have one stock which apparently is in a lot worse shape than maybe what the coastwide suggests. I think there is a risk there, perhaps. I think that we're a little premature on this discussion. I think we should decide whether or not we think this is best scientific information and then decide how to go forward, because you are going to have to make ABC recommendations somehow and I think answer the first term of reference.

DR. SCHUELLER: Just a point of clarification; the ACL that is being used now is coastwide, there isn't individual – okay, so my opinion on that is if these are different stocks and there is evidence for that, then this is probably an improvement.

DR. BARBIERI: Mike, where is the ACL?

DR. ERRIGO: We have not had an ACL in place until 2012.

DR. SCHUELLER: I'm just wondering if; okay, the last time this was done on a one population ORCS method. There is evidence to suggest that there is more than one population, and the quality of the data I know is different, populations are different. Can we choose to put some in an ORCS-based context and others use the model. I guess that gets to data quality question. It is completely a data quality question for each of the individual three stocks.

MR. CARMICHAEL: And somewhat procedural I think, because the first part is procedural; can you separate them out? I would say you can. If you agree that they are separate stocks and you want to specify ABCs for the separate stocks, then you could face the question of how you specify the ABCs for the two stocks.

They are separate assessments, so there is no reason why you couldn't specify ABCs for one component off of the assessment if you have confidence in that and off of another method for the other stock if you don't have confidence in the assessment for it.

DR. BARBIERI: By the way, just to clarify, when I talked to Wade about engaging him to conducting the hogfish assessment, the idea was to conduct a single stock that was actually Gulf and South Atlantic, because at the time there was really no evidence of anything breaking up in terms of populations.

Because, by coincidence, really, the genetic work was just being completed; it really sort of generated a different issue; because with that evidence – and there was a paper that was published, and it is a fairly solid technique; and I think that we should have included that in actually the briefing book. Seifu's paper should be there.

This is really a fairly robust technique that at the time suggested in that publication that the separation and the lack of mixing of those stocks has been going on for quite a while and that the separation is much more clearly detectable than one would expect. Then Wade felt like, well, we discussed all of this; and I guess if we have three populations, we are going to have to conduct three assessments, because those things are three different units.

That is some background on the quality of that genetic work and the status of how we arrived at these three assessments. Amy, just to get back to your point, one point of discussion here is whether we can – and I guess John just clarified that procedurally we can provide catch level recommendations differently for those two stocks since they are two biological stocks.

The outcome of this assessment review would be primarily, if not exclusively, based on the Southeast Florida/Florida Keys stock, because my recollection of the reviewers' comments is that the quantitative assessment of the Georgia/North Carolina stock did not pass muster at all. In that case we would have to produce a catch level recommendation that is average landings based or ORCS based, whatever, for the Georgia/North Carolina portion; and then results of the assessment will be informing the Southeast Florida/Florida Keys.

DR. REICHERT: That is a good point, and I think it would be good to see if the SSC as a whole agrees with that judgment of that reviewer. That may provide a path forward. I agree with that assessment.

DR. SEDBERRY: Just to comment on the genetic work; I just want to note that it is not published and that there is a lack of sampling from Jupiter and South Carolina, and there is no out-groups from the broader Caribbean or someplace else to compare this to. What do those clusters really mean? I don't think we know yet.

DR. BARBIERI: The paper was actually accepted for publication. There is the paper that had been submitted. It is one of those things we have to get Seifu or Mike Tringali to kind of give us the – I don't know enough about molecular genetics to be able to –

DR. SEDBERRY: Neither do I, but I'm just looking at it and I'm thinking without an out-group from a distant areas, South America or the southern Caribbean or some place; that there is not real meaning attached to what these clusters mean. Then there is that lack of sampling right in the middle of the east coast range.

Maybe there is a clime there with constant mixing, but it is slower than normal because they have a shorter larval stage; but I don't think that is even true. I am not sure that we have a clear definition of separate stocks here yet; but I am not a geneticist.

DR. BARBIERI: Those are very good points, George. I don't disagree one bit; those are very good points. I think the only way to get to that would be to bring Mike Tringali in, because this is something that talking to him, he was very, very conclusive on the results of the genetic analysis.

DR. GRIMES: I'm not a geneticist either, but that won't keep me from making comments. I'm not sure I agree with that. Generally speaking, I think that when you find genetic differences, it is sort of a one-way test; so if you find differences, it really means that they are fairly different. It requires a very small amount of mixing to make them homogeneous. The fact that he did find some difference may be valid to conclude that they're different groups. As George says, I'm not a geneticist.

DR. SEDBERRY: The same genetic clusters have been found for white grunt, too, with very similar growth differences between the Florida Keys and the Carolinas. Maybe it is real, but maybe it is not. Again, I'm not a geneticist.

DR. BARBIERI: That is a very relevant point. We are now deciding whether we agree to make those catch level recommendations separately for those two South Atlantic portions of the stock or the two separate stocks in the South Atlantic; believing the degree, knowing Mike Tringali.

He sent me an e-mail, which I can try to find here during the break, George, that really explained in much more technical detail why these very unexpected differences were so strong that they suggest that the separation happened a long, long time ago. As Church pointed out, the minimum amount of mixing would have brought some kind of inconclusive results, and the fact that this didn't happen.

DR. SEDBERRY: Another thing I keep thinking about is I've seen these fish doing courtship displays at the shelf edge reefs out there in 55 meters, 180 feet or so right at the Gulf Stream. I just have a hard time thinking that there couldn't be mixing from that shelf-edge reef off of Florida up into the Carolinas. If this difference is real, there is something weird going on there.

DR. SCHUELLER: Is the paper in our briefing book? My comment is 24 micro satellites is a decent number of micro satellites. I guess I am with Church; it takes a very little migration for those micro satellites to show no difference; so if you are seeing a difference, to me that is decent evidence, especially given there are 24 micro satellites; assuming that none of them were sort of null or didn't have any issues. I didn't see the paper so I can't say for certain.

DR. BARBIERI: Okay, folks, since we haven't even hit a consensus on how we are going to break this up; why don't we give ourselves a little break and take a brief break to collect our thoughts and get back in about ten minutes.

In terms of going forward here, I would like to get back to our action items. George's point, which I think is a valid discussion point, we are not going to be able to get that resolved today. Based on what we know about molecular genetics and knowing the way that Mike Tringali handles these types of issues and his level of experience; for him to say I am very confident and sent me that e-mail that basically documents the fact that there are three separate stocks genetically; I think we can confidently go with that.

I can bring additional or send to the committee additional – I will consult with Mike Tringali when I get back to the lab and send some additional information later on; but I would say that we are at a point where we can consider that we have three different stocks. For our jurisdictional area, we are really talking about the Georgia/North Carolina cluster versus the Southeast Florida/Florida Keys.

We have two points here on the table. One is if we decide – and we had some suggestion that we would provide catch advice separately for those two components, meaning the Georgia/North Carolina cluster; that portion of the stock would be managed using the ORCS approach or some other average landings-based approach.

That stock is considered to be unassessed and a quantitative assessment did not pass review while the Southeast Florida and Florida Keys stocks would be considered that we have a quantitative assessment and that could be used for catch advice. With that in mind, for the purposes of this assessment review and development of information to address our action items; can we consider this to represent the best scientific information available and suitable to provide management advice to the council?

If I make this in the form of a statement that the SSC considers the benchmark hogfish assessment for the Southeast Florida/Florida Keys stock to be the best scientific information available and suitable for management advice; would anybody disagree with this statement? This is a way to get our discussion started.

DR. CROSSON: I was just going to point out that the terms of reference for hogfish require us to give fishing level recommendations regardless, so it is either use this or we just have to rely on catch recommendations. We are being asked to give an ABC recommendation for this stock, no?

DR. BARBIERI: Well, based on this assessment, the outcome of this assessment, the fishing level recommendations will be associated with this assessment. We have standing catch level recommendations that are based on ORCS that are now in place.

If we do not accept this assessment as the best scientific information available and suitable for management advice, the ORCS-based catch level recommendations remain in place. No, they wouldn't, Michelle?

DR. DUVAL: We didn't do it through ORCS; we took hogfish out when we did the ORCS workshop because of this impending assessment. What is in place right now is through the Comprehensive ACL Amendment.

DR. BARBIERI: What we have right now in place is through the Comprehensive ACL Amendment. That was based on I think the –

DR. ERRIGO: I think that was the decision tree; so perhaps third highest landings I think was used for hogfish to determine the ABC value within the range of years.

DR. BARBIERI: Okay, thank you for that clarification, Michelle, because that is important for us to know. That is how procedurally, Scott, this would go. We have a statement under SSC recommendation. I would like to hear some committee discussion points, agreement or disagreement so we can move forward.

DR. GRIMES: To make sure I understand this; is the option to run it through ORCS or just leave it at a decision tree analysis conclusion of what the ABC ought to be?

DR. BARBIERI: Let me clarify just this point. Just like any other stock assessment that we review; we review the assessment and our action items are we have to consider if this assessment is accepted by the committee as best scientific information available and suitable for management advice.

If that is the case, we are going to develop ABC recommendations and we are going to apply a control rule and develop ABC projections like we usually do. If we don't accept this, the existing ACL that is in place right now remains in place until a valid assessment is approved.

DR. VAUGHAN: Does that preclude if we don't accept this assessment that our only choice is going with what is currently in place or that we suggest that two stock are there and that they be reconsidered based on, say, an ORCS basis?

DR. BARBIERI: Yes, we can do that.

DR. VAUGHAN: Because we already have expressed enough concern about the Georgia/North Carolina portion of the stock; that if we accept that as a separate stock, we are going to have to do something, anyways. Then to me the real question is with respect to the Keys/East Coast of Florida stock; whether we accept the assessment as best scientific information or whether we have enough concerns about that assessment.

DR. BARBIERI: That is exactly the question that we have.

DR. VAUGHAN: We've got our own decision tree, first of all, dealing with the stock assessment and then dealing with the two stock hypotheses and how to deal with that.

DR. BARBIERI: Not really. We have already crossed that bridge and we have accepted that we have the two stock hypotheses for the South Atlantic area. Catch advice for the Georgia/North Carolina is going to be based on average landings, whatever the committee recommends. Now here we need to deal with the quantitative assessment for the South Florida/Florida Keys. This is that assessment.

DR. BERKSON: I am probably asking what everyone else has asked ten times, but the current ACL is based on that entire region, North Carolina to the Florida Keys. We're talking about splitting it and the choice is what do we do with the Florida region; do we accept this or not? For the North Carolina to Georgia area, we are going to have to recalculate the average just for that area and do the third highest just for that area, or ORCS, or what do we do for that area? That is what I am confused about. We have to deal with those areas.

DR. BARBIERI: We will; I just want to chunk them. Right now I want to deal with the assessment part. After we complete that part, we'll deal with Georgia. The decision was we're going to consider whether we have two separate stocks, and we decided, yes, we do.

DR. SCHUELLER: Why don't we have three; I am not on board with two yet.

DR. BARBIERI: The Gulf one was already –

DR. SCHUELLER: So we have two for us; got it. I'm sorry, I'm looking at the paper.

DR. BARBIERI: Yes, two weeks ago the Gulf SSC already reviewed the West Florida Shelf/Gulf, and they provided their own catch level recommendation for that portion of the stock. Jim, we will get to that Georgia/North Carolina portion. I just want to make it simple, because the two now are – the discussion, nothing is simple, yes. Let's deal with the assessment part first and then we'll deal with the Georgia/North Carolina cluster.

This is for the assessment part; not the Georgia/North Carolina portion. Do we have any disagreement or any comments that for the quantitative assessment; the SS3 statistical catch-at-age model, this benchmark hogfish assessment for the Southeast Florida/Florida Keys stock is the best available scientific information and suitable for management advice and catch level recommendations.

DR. VAUGHAN: Yes; I have sufficient concerns about it with the lack of pattern in the uncertainty and various other aspects of the assessment. I have a problem with that.

DR. BARBIERI: Okay, so that really I guess brings the issue back for discussion. Does anybody else have enough concerns about the assessment to not accept that statement and agree with Doug? In that case, we would reject this quantitative assessment and consider that this stock is unassessed and we continue providing catch level recommendations to the council based on the decision tree.

DR. GRIMES: But the point I think has been made already that sending it back is not going to get a better result probably. I guess our only alternative is to go with some catch-based decision about what the ABC ought to be. The question is, is that better than making a decision based on the current assessment? I am curious what Doug thinks.

DR. SCHUELLER: I'm curious what Doug thinks, too. Okay, we're sort of sitting around the table here saying, yes, this is best available science; and then, Doug, you are saying, but you don't think it is management worthy. Can you elaborate on that; what specific list of things are you pointing to that make it not management worthy?

DR. VAUGHAN: I am primarily concerned with the lack of contrast in the data along with other issues that were raised by the CIE reviewers; the combination of issues. That causes me to question whether it is useful for management.

DR. BARBIERI: Since we are a consensus body, we don't get in this situation too often where we don't actually have consensus. I think that we are going to have a little bit more discussion here. Do you have something, Amy?

DR. SCHUELLER: Yes, I was going to say I don't know where I sit on the fence. I don't feel like we've had enough discussion to say whether or not it is management worthy or not. We're hearing from Doug, but what do others think?

DR. GRIMES: I think somebody has already said this, too, but does it help to look at where the fishery is in terms of what our current ABC set with the old rule was relative to what this assessment suggests that ABC ought to be or status ought to be? That would suggest that this assessment is not really giving bad advice; wouldn't it?

DR. SCHUELLER: I'm not sure how looking at how it is doing relative to what we have helps us make a decision about the worthiness of its use in management. I think it needs to be based on the data and the scientific robustness of the model and whether or not it is going to provide what we need or not. That is why I am on the fence.

DR. BARBIERI: Just to put something out there for discussion; we could look at this a bit like the way that we looked at wreckfish where we had a lot of concerns about the wreckfish assessment, because there were some fundamental questions that were very difficult given the geographic scope of the stock and lack of a lot of fisheries-independent information.

There were things that were very challenging with that; but we felt at the time that the quantitative assessment that we reviewed trumped and really provided an improvement in terms of the catch advice that was provided for the council compared to the DC/AC analysis. Here right now we have the council receiving catch advice based on our decision tree at our lowest level.

Even that catch advice is encompassing the entire geographic range of the council's jurisdiction and not really recognizing geographic differences in population structure of hogfish. To me, I wonder if we could look at this as a progressive improvement process for this catch advice where we started with that decision-tree-based average landing; now we have an assessment that is full

of imperfections; but as JB pointed out we have ways in our control rule to deal with those uncertainties.

If we don't, could perhaps it provide – you know, step out even just the limits of our control rule and expand our scope of uncertainty for this buffer between OFL and ABC; and then having had a CIE review and having had a very good SSC review that has provided a number of constructive suggestions, come back in a few years with an assessment that would have those things taken care of. I am going to put this out there for some feedback. I would like to hear more from other committee members.

DR. BOREMAN: This is obviously a bit problematic. I was just thinking how we would approach this in the Mid-Atlantic. We've gotten assessments that have cleared CIE review and passed muster with them that we rejected. The principal reason is that we look at it as do we get a robust estimate of OFL from this assessment?

Unless you can get a robust estimate of OFL, you can't do the P-star approach. I guess that is the question here is the estimate of OFL coming out of the assessment robust? Whether the PEF associated with it is or not is a different issue, but is the OFL estimate robust based on the assessment; and if not, then I don't think we can accept the assessment as appropriate for management advice.

DR. VAUGHAN: I think John expresses it very well. I think that is what I am struggling with.

DR. CROSSON: Just getting back to the question then what we would do if we did not consider that the OFL level that is coming out of this is acceptable for management; I am looking at this and I see provide fishing level recommendations. I believe the SSC is being asked to come up with a new ABC recommendation whether or not we're using this.

You are telling me that it just would default to the Comprehensive ACL numbers unless we change it. I guess this is getting back to the control rule again and what is the initiation for the council asking for an ABC recommendation. What is the process where they formally ask it? To my mind if they put it in the terms of reference for a meeting that they are asking for a fishing level recommendation, then they are asking us to do that; whether or not – okay.

DR. BARBIERI: Just to clarify – that is a good point – all of these actions that are specified here in Item 7.4 of our overview document are relative to this assessment and that is why they are listed in that order; review the assessment and consider whether it represents the best scientific information available.

If we decide that we're going to go there and say, okay, this is it, then it is suitable for management advice, and in that case we move on to identify and discuss assessment uncertainties, because then we go and apply a control rule. Then based on that control rule, we provide fishing level recommendations.

Whatever the outcome of our P-star analysis, we make a recommendation for projections to be developed. We define whatever time verizon that we want those projections. These fishing level recommendations here are only if we accept this assessment. If we don't, the council doesn't have any – we deem the information brought by this assessment to be unsuitable to provide

management advice, therefore we don't provide any. We don't have anything to go on to provide that catch level recommendation.

DR. CROSSON: Including the things that are in the P-star evaluation and uncertainty characterizations and everything else that we normally -- well, I'll just back off and let John.

DR. BOREMAN: I don't agree with you. I think if we reject the assessment, I think we still have the option of providing the council with our best recommendation for ABC. The recommendation may be to return to the previous ABC recommendation, but I think it is incumbent upon us in good conscience to sit here and not just say we'll throw up our hands and walk out and everybody go home; we should at least think about is the ABC that the council is operating under now, is that adequate? Is that doing the job or can we come up with a better one for the council to work with somewhere in between that one and this fancy-dancy approach here?

DR. BARBIERI: Since this was written by John Carmichael, I will let him explain.

MR. CARMICHAEL: The terms of reference that you see – and if you recall they are the same you see for every assessment – these are pretty standard. Luiz is right, the "provide fishing level recommendations" there is relative to that above. However, John is also right; you are not just bound by what is there.

If you believe that information has come to you that make you question the basis of your existing ABC recommendations, the council expects that you will let them know about that. As we talked about the other day and Amy mentioned about when these are reviewed; and it is, well, when new information becomes available, you look at it and you review it.

I think this falls in that situation. Why we didn't foresee to say, oh, first of all, we want you to provide a new ABC for hogfish and then review this assessment and give us fishing level recommendations from that; that is somewhat implied, and you always have that. If new information comes available to you, you, as the scientific advisors to the council, can always recommend to the council we think we need to change our ABC.

It is probably better to do it now then let this go through to the council and them say, oh, but you think there are two stocks so we probably need two ABCs, so let's come back in April and have you give us an ABC based on two stocks. Let's cut out that middle man step; and take that fishing level recommendation TOR very broadly, even though it originally is relative to doing the assessment.

DR. BARBIERI: My point is that we don't have any additional analysis. We don't have the numbers; we do? Okay, Jim.

DR. BERKSON: Two things, first of all, we are going to I think end up with different ABCs, because we are going to do it geographically. I would think that is the direction we're going to go. Regardless of the order of that; that is the first thing. The second thing is, if I heard you correctly, we didn't come up with an ORCS value for this, because we said we're going to put it through an assessment and we're going to use the value coming through the assessment. If we don't accept the assessment, then I think we can say as an SSC do we want to try putting it

through ORCS and would that be better than going with third highest? I think we certainly have the ability to do that now.

DR. BARBIERI: All of this is a bit philosophical.

DR. BERKSON: No, it isn't; it is extremely practical. It is practical right now unless I'm missing something.

DR. BARBIERI: You are missing something. Do you have the numbers in front of you now? Will this committee have the numbers now to provide that catch level recommendation to the council; just a simple yes or no question? How many metric tons or millions of pounds are we recommending today, right now, to the council as the new OFL and ABC? Do we have that in front of us?

DR. BERKSON: I'm guessing we don't or you wouldn't have asked that; but we certainly could have e-mail distribution in six weeks and have a webinar conference. There are different ways to do this without waiting until April if the committee felt like that was the best way to go.

DR. BARBIERI: Right; and this is why I said in terms of philosophical is that because we are not going to come out of this meeting – we are going to have to recommend an additional analysis or an additional pathway. But until we have that analysis done, we don't have the numbers in front of us. That is what I said. I

In terms of providing the fishing level recommendations, John is right, we can recommend that we are going to step outside of the assessment and recommend ORCS or recommend some other data-poor method be conducted. But until somebody, either from the Center or from FWC, actually conducts that analysis, we're not going to have the numbers in front of us. That analysis may or may not be ready for our report, which has a deadline to be in the council's briefing book. That is what I wanted to clarify.

DR. BELCHER: But in the past we've had situations where we have had to go with projections, and we haven't seen – those have never been actual hard-core numbers we've had in our possession either, because we went through and we applied the P-star. Then we went back to the Science Center and said we need you to run it at this P-star level; but we never said anything about the actual number that came out of P-star. We just recommended that value.

There was a whole suite of numbers that came out afterwards. I think similarly I kind of feel the same way that Jim does is that we may not necessarily have the numbers. We've been that way with fish that we can't see data for. We've not seen ABCs for confidential data points either, and we just recommend the framework by which it is run, and then as long as the framework is appropriate.

DR. BARBIERI: To that point; I hear you. I am going to acquiesce and let's go in terms of finishing our action items so we can move on to the other.

DR. VAUGHAN: Okay, we've already committed to doing this for the North Carolina to Georgia stock; we've said there are two stocks. We are already going to have to do something for the North Carolina/Georgia regardless of whether we do it for the Florida stock. We are

already committed to doing an ORCS type thing. It is not that this is going to be something new or different.

DR. ERRIGO: Okay, I just to throw this out there. The report which we have and the assessments were done for each stock individually, so all the landings' data actually should be broken down by stock. It should be in the report. We can use that or at least the trends to look at that.

One thing about ORCS is that the last time we did the ORCS method we actually had AP members and fishermen involved in the fishery to help us decide how to rank the stocks. If you feel that you need that for hogfish, then that might delay assigning an ABC value at this particular meeting. But in terms of the landings' data and actual numbers for hogfish, it is either in the assessment reports or I can probably get them for you when that needs to happen quickly.

MR. CARMICHAEL: I believe you classified hogfish through ORCS when all the AP people were there. It just didn't move into actually the council taking that recommendation and using it because of the impending assessment. You have the information that you need probably to apply ORCS if you so choose to go that route.

DR. BARBIERI: Okay, so in that case, Mike, I would say that statement does not consider the benchmark hogfish assessment for the Southeast/Florida Keys stock to be best available science suitable for management advice and catch level recommendations is not true. We are going to have to change that.

MR. CARMICHAEL: I guess I'll raise a point of issues when you are working only by consensus. If there is someone who believes just as strongly that it is best available science; you change the statement to this and now then I'm back on the other hand and I have someone saying, oh, I believe it is.

I think it would be helpful to figure out does the group truly agree with that or is it more there are a few members of the group who have reservations? I think it is important to make sure. If we say there is consensus that it is not best scientific information, let's make sure that you have consensus across the board on that.

DR. BARBIERI: There were so many discussions about alternatives that led me to believe that – you know, coming from so many different members; that led me to believe that is the way that this thing was going.

DR. BERKSON: It is likely we won't have consensus on one or the other where everyone thinks it is best or not best, in which case we should make a list of the Plosses and minuses of the assessment that we can agree on. We don't have to judge it; we can just say we do not come to a consensus that we should use it for management and here are the reasons why. That can be used in the next assessment to try to fix what we consider to be the problems.

MR. CARMICHAEL: Yes; that is what I think is that perhaps the statement is the SSC could not reach consensus on whether or not this represents best scientific information. Then you've got to list your reasons.

DR. SMITH: Maybe I'm getting a little ahead. I had reservations about the assessment, but I do think there are things we can look at that might give us a little confidence in it. For example, the sort of truncated growth that they are seeing in the Keys indicates that there is perhaps some sort gross overfishing sort of thing going on, which is what the assessment showed.

If you look at the length comps, there is not a lot of evidence of expansion in the Keys, but there is some evidence for contraction of the length comps, which also points to overfishing. I do think there is some evidence in there that even if this isn't the gold standard assessment; I do think some things are lining up that would indicate that this is going in the right direction, anyway.

MR. CARMICHAEL: That is exactly the way we put the issue to SEDAR review panels; that you don't get off the hook by just a simple pass/fail. You are still under the obligation, and you guys are under the obligation to say what you can about status of these two stock groups using the information before you and to give catch level recommendations on that. Yes, I think that is good advice, Will, and hopefully we can tease something out of it.

DR. SCHUELLER: I guess it seems to me that I want to make sure we're not getting the two pieces intertwined. To me, my sense of what is going on is that this is best available science for both; however, we're still debating whether or not they are useful for management. I also agree with Will that just because it isn't the gold standard, it doesn't mean it can't be useful. We have to be careful, we can't just say only Lexus' are useful. Honda's are great too, right? We have to step back and really think about are there data as will suggested that push us in one direction for our status determination or not?

MR. CARMICHAEL: The term of reference is whether or not it is best scientific information. I guess under that would we say there is a higher standard to be met to be useful for management than best scientific information? Useful for management was something the councils SSCs used to kind of say when they weren't as involved in the determination of best science; and then with the changes in the Act; you are expected to say what is best science for the council. If you are saying it is best science, then you are kind of telling the council we can use this.

DR. SCHUELLER: I guess I don't see those as the same. They are not mutually exclusive, but they are different. Something can be better than what you had, but it can still not be good enough to actually be used for management if there are some flaws in it. That is my view of it. If that is not appropriate given what the SSC's charge is –

DR. VAUGHAN: That is my mental concept.

MR. CARMICHAEL: I think if you are saying it is better than what you have, but you are not agreeing it is best available because you think with the information that exists for this stock there is something that could be done that is better and it is available. It is not like research has to be conducted or methods have to be developed. You are thinking there is a way to modify what you have here to make it best available.

DR. BARBIERI: One thing it might be useful for us to review; I mean, this has to do with the NS2 and the actual guidelines. How we interpret best scientific information available versus

general counsel, the agency and how the councils are instructed to really interpret that term are not necessarily the same.

DR. BERKSON: I already regret bringing this up, and I haven't said it yet, but it depends on what you are talking about in terms of best scientific information. There are lots of different kinds of best scientific information. Some of them relate to management, some of them relate to population dynamics. Some of them talk about system dynamics. You can have great science that doesn't necessarily inform about management. We need to clarify that and that is all buried in National Standard 2.

DR. BARBIERI: I am just wondering, to tell the truth, if we have general counsel listening.

MR. CARMICHAEL: No.

DR. BARBIERI: Oftentimes we do. I do hear from general counsel about their requirements to apply some of those things from a legal perspective instead of simply because we have to abide by those national standards. It is a little bit of different information, and I wonder what Shep and some of my friends there in legal counsel –

DR. GRIMES: Maybe we need to give Shep a call.

DR. BARBIERI: That we will say this is the best scientific information available, but we're not going to use it for management.

DR. VAUGHAN: I agree with that. This recent conversation sort of raised where my concerns are coming from, because I do agree that this is the best you can do with what you've got; that it is the best scientific information. The question is that is rumbling around in the back of my brain is whether management would be better served by basing ACL on this model run versus whether it would be better served from an ORCS approach applied to the appropriate stock. That is what I'm struggling with.

DR. ERRIGO: I think you need to consider in terms of best available science in your role as the SSC; if you deem this best available science, that means that according to NS2 the council needs to use this information to try to come up with their ACL and ABCs. Do you think that by using this in conjunction with your ABC Control Rule that there is a higher chance that when you set the ABC; that there is a higher than 50 percent chance that you might be overfishing because of the amount of uncertainty in there, even after you apply your control rule, simply because there is just way too much uncertainty or there is a potential for bias or something like that.

If you think that you can use this information in conjunction with your control rule to set an ABC; that still keeps you less than a 50 percent chance of overfishing; and then you can use it for management to set an ABC and probably be legally safe.

DR. BOREMAN: I'm not even going to touch that because I got lost right in the beginning of what you were saying. I think it is very simple. It is the best available but it is not good enough. It is not good enough because it is not giving us an acceptable OFL. It does contain a lot of information related to life history characteristics.

The stock characteristics, the updated landings, updated indices of abundance or other selectivity or whatever has a lot of good, important information that we haven't had in front of us in the past. It is the best; but it is not giving us an acceptable OFL estimate so we have to still resort to a different level approach than the P-star.

MR. CARMICHAEL: I believe we've had assessments where – and I think it is very clear the wording is incredibly important. I was concerned about saying it was not adequate for management because that is not really a meaningful term. But John presented it very different, and I think that is critical, because we've had assessments where we said, yes, this is the best information that is available, but it is really not adequate for us to get an estimate of OFL or an estimate of stock status.

We've had assessments done, a number of ones in the SEDAR; in the Caribbean in particular we are like you did the best you could with what you have; however, the information is just not adequate. It doesn't have the contrast. It is not robust enough for us to evaluate stock status with this here. I think that is where we are. I think if we couch it in those terms, you might be in a more defensible position than bringing in this not useful for management kind of vague term.

DR. BARBIERI: To me it is the best and it is all capitalized here, the BSIA. I wonder if we are not going to change the catch advice and we get in a situation where the council is now being asked why it didn't use the best scientific information available.

MR. CARMICHAEL: No, I don't think so.

DR. BOREMAN: We are using the best available, but we're not going all the way to the OFL to use that, because that part of it we don't agree with, but there is a lot of other stuff in there. It is still the best; it is just not good enough.

MR. CARMICHAEL: It is a sequential evaluation. If you decide it is best, that means there are not fatal flaws, it is done as good as it could be at the time; and then you go down the list. Can you get an overfished evaluation; can you get an overfishing evaluation; can you recommend an MFMT; can you recommend an MSST? Can you recommend an MSY?

It is very much like that. We've had assessments where we said – I think Spanish mackerel was an example where you had an assessment, you accepted it. You felt the overfishing references were viable. You didn't trust the biomass estimates, and you said we can't give you recommendations on overfished, but we can give you recommendations on overfishing. I think if you go through it sequentially and decide which pieces of this you have confidence in; then you can build a record and show where you think the information stops being robust.

DR. BARBIERI: I'm sorry for my confusion basically, because the Gulf Council and the Gulf Council SSC actually interprets this a bit differently. We've had input from General Counsel on this issue specifically, and General Counsel came to our meeting to give a presentation and explained that sometimes us as scientists, the way that we interpret this is not necessarily in line with the legal issues that are associated with National Standard Guidelines 2. Because of that, I was confused, but it is clear to me now.

DR. BERKSON: I think J-squared have come up with a really good plausible explanation and justification for what we've been talking about. I am good with this.

DR. BARBIERI: Have we captured that up on the board?

MR. CARMICHAEL: Not yet; he's got to clean it up.

DR. ERRIGO: I think we need some discussion of what you can and cannot get from it, but I think what I got is you feel that the assessment is best available science, but there are things that you cannot get from it due to the amount of uncertainty in some of the data and parameters, such as the estimate of OFL you feel it is not robust enough to base ABC values off of and things like that.

However, some of the information is useful in perhaps getting status, but I'm not sure. There was a suggestion that perhaps looking at some of the length comps and some of the other information may be able to infer status of overfishing. More discussion there would be helpful.

MR. CARMICHAEL: You've done things like that before, too, where you said we can't give you a quantitative value for overfishing or overfished, but we believe the preponderance of the evidence suggests it is likely that overfishing is occurring. That is where it gets important. The guidance that you give to the council is important. If you don't have confidence in the absolute numbers, that is okay, but what do you feel about the overall gist of it? Can you give qualitative advice to the council that will be useful?

DR. BELCHER: I just wanted to bring up. I know the difficulties in how we're all interpreting different things, but as John just said we've been dealing with the issue of OFL being unknown. Different councils for lower level tiers are setting an OFL. We were told by two different councils two different spins on that; but as long as we were comprehensive in the discussion saying why it was unknown, that was as justified an answer as putting a number in there with no certainty on it.

DR. BARBIERI: Okay, so that resolves our first action item.

DR. SCHUELLER: Our first action item is review and consider best available information. Our statements are the SSC for the Southeast Florida/Florida Keys is – then the question is, is that an accurate representation for the Georgia/North Carolina side of things? I guess I argue that again is best available science.

DR. BARBIERI: But the assessment; I mean, this is not about the existence. There is a scientific paper that described the existence of the different stocks. Here we are reviewing the stock assessment. Do you think that that quantitative stock assessment for Georgia/North Carolina represents the best available science in terms of the assessment?

DR. SCHUELLER: I don't know if I am fueling all this or not, and I have been on the fence, but for me I think they both reflect best available science. I don't know that we've given a good enough justification to say why they wouldn't be useful for management. I think the onus is on us to say what is wrong with it. The contrast in the data issue I think we should talk about some more, because I'm not so sure I totally buy that as a critical flaw, I guess.

DR. BARBIERI: Amy is bringing up the issue that the Georgia/North Carolina portion of the assessment, the assessment of that stock represents the best available science. Would anybody disagree with that statement? If not, then we revise our recommendation for that portion of the stock; so the SSC considers the Georgia/North Carolina assessment to be the best available science.

That defines the best available science for both of the South Atlantic stocks in terms of that action item. The second one; identify and discuss assessment uncertainties, and then we have to provide fishing level recommendations. Considering that we are ten minutes to noon; shall we break right now and return to those discussions at 1300 hours?

(Whereupon, the meeting was recessed at 11:55 o'clock a.m., October 29, 2014.)

The Science and Statistical Committee of the South Atlantic Fishery Management Council reconvened in the Crowne Plaza Hotel, North Charleston, South Carolina, Wednesday afternoon, October 29, 2014, and was called to order at 1:00 o'clock p.m. by Chairman Luiz Barbieri.

DR. BARBIERI: (Recording started here) – some of our discussions of the action items regarding the hogfish benchmark assessment. I want to thank the committee. I think we have had very robust discussion and conversation this morning that has really helped I think document our perspective on some of the difficult issues that we've been dealing with in this situation.

Also for some of us; I've learned something today in terms of different perspectives on issues. It is one of the benefits of having a synergistic panel like this where we can kind of learn from each other. With that, for our action items we have completed number one, and we have our bullet statements that Mike put together on the left for our SSC recommendations.

As always, we are going to be sort of fleshing those out and beefing that up for our report in a more narrative form. That moves us now to identify and discuss assessment uncertainties. Of course, some of those have already been captured by this morning's discussion. I was wondering is there anything else that the committee would like to capture about either identification or discussion of assessment uncertainties.

DR. JOHNSON: I was interested in the growth rates and whether or not the differences are actually existing biological differences or whether those are chicken in the egg with the fish re-driving those, because the impacts were huge on the actual parameters that come out. Assuming if we could look into doing the analysis we could do on the biology of that to figure out whether those are real differences in growth like we think with white grunt and some of these other stocks, or whether they are fishery dependent; even maybe broaden the scope, because that may be the case with other stocks as well.

MR. CARMICHAEL: Another point that I thought should be mentioned is the fact that it is a largely recreational fishery and its reliance on MRIP. I think Mike mentioned there are pretty high CVs on the catch estimates and also pointed out the small number of intercepts. There are not a lot of bio-sampling coming out of that; so I think that is definitely something to note here.

DR. BARBIERI: Right, very good point. Any other additional points? Of course, this morning Amy, Will, JB, Doug and others have brought up other points that had already been captured

about those uncertainties; so we don't need to go over the same ones. I am just wondering if there is any additional input to document our perception of the uncertainties in this assessment.

DR. BUCKEL: This would be future research type of thing, but certainly addresses what Eric brought up. This is something that Ben Hartig was telling me about. In a closed area off the Keys, the hogfish are much larger, which would suggest that it is not a genetic thing going on; it is fisheries based. Their research recommendation would be to compare size distributions from the fished areas to closed areas. There are multiple closed areas in the Keys, and maybe those data would provide some evidence to Eric's point.

DR. BARBIERI: Yes; very good point to kind of test the hypotheses. Anything else?

MR CARMICHAEL: I'm not sure it is in there, but I had a note you discussed the difficulty in separating between the Keys stock and the Georgia/North Carolina stock in sort of that open area there. You should probably note that. It may be down there with the genetic stuff.

DR. BARBIERI: Good point; and if we capture that now, at the very least we have a little redundancy there, and we can clean it up during our report-writing phase. If no additional recommendations from the committee or observations or comments regarding our second action item, then we can move on to our third one in terms of providing fishing level recommendations.

DR. REICHERT: I assume you want to do that by region? Where do you want to start?

DR. BARBIERI: I don't think it makes any difference. I think in our report on our recommendation comments, we have already explicitly expressed that we are going to be treating those two genetic stocks separately. It is just a matter of making recommendations.

DR. REICHERT: I would like to start with Florida East. The report lists that all fish are currently overfished and experiencing overfishing.

DR. BARBIERI: I think this now goes back to JB's earlier point in terms of OFL. I don't know if we have a resolution yet on whether we find that the estimate of OFL in this assessment – the discussions were leading me to feel like the group felt, no, the estimate of OFL was not –

DR. SMITH: I guess nobody has actually said it, but I'm okay with the assessment. I think it has got a lot of warts, as Mike put it, and a lot of uncertainties; but I think that we can address those problems in the way that John mentioned earlier in our ABC estimate.

DR. VAUGHAN: I did talk with Mike at lunch today. I think some of my concerns have been allayed a little bit; I will say that.

DR. SCHUELLER: I will agree with Will's statement, too; and he did earlier point out some growth information that leads you in the same conclusion as what is coming out of the model, so things sort of all point the same direction.

DR. BARBIERI: I would imagine in that case for the East Florida/Florida Keys assessment, the committee is accepting the base run estimates of reference points and other management quantities. In terms of the reference points, as we start thinking about filling in that table, one of

the bridges we haven't crossed yet – and we usually go there – is whether we accept the estimated steepness value and whether we can have a direct MSY estimate versus going with a proxy reference point.

Obviously, we are going to have to have that determination for our application of our control rule and for the quantities that we report on that table. Discussion and input from the members? Well, the steepness value is listed up there as 0.83 and I don't know if we have Mike's presentation still available.

He could put the likelihood profile there for steepness up on the board. The fact that they actually started – refresh my mind here, but I think that they started with a completely free steepness estimation process and then went with the use of the Shertzer-Kahn prior; and this is it.

DR. ERRIGO: They also did a sensitivity analysis where they took the prior off. It showed no difference in status determination or estimation of the benchmark parameters.

DR. BARBIERI: It is not completely clean. Some of the CIE reviewers have some fairly strong comments about the relative weights of different data sources and conflicts perhaps between age and length.

DR. REICHERT: I was just looking at Medley's review. Stock recruitment the relationship is flat, estimation of steepness are poorly determined from the data, but the values obtained remain reasonable and appropriate.

DR. BARBIERI: Obviously, this is not an easy decision; and that is why I guess people are thinking about it and perhaps a little bit on the fence. The value is reasonable, but the likelihood profile is not as clean as we would like to see it.

DR. SCHUELLER: If you just look at the total likelihood and profile and not necessarily the component pieces, it is about the best steepness profile I have seen in a while. It is based on the data quality, right.

This all comes back to data quality and what those data can tell us and whether or not we believe length data can give us information about steepness, because that is where all the information is coming from. But given the model we have and that we are not going to go back and redo or anything; this is what we have, I guess. If we don't use it, what are we going to use?

DR. BARBIERI: We usually just recommend an SPR proxy. Doug, talking about lack of contrast, right.

MR. CARMICHAEL: Compared to the other one, at least this shows you did get somewhat lower recruitment at lower SSB versus the other one. Your lowest SSB had the full range of recruitment. Maybe this is why this did a little better.

DR. BARBIERI: Right. We have a steepness, which means that we fill our management quantities there and reference point estimates and MSY-related units. I'm not sure that we need to do this here live, do we?

MR. CARMICHAEL: We can look at what the numbers are just to know what you are getting into.

DR. BARBIERI: Perhaps in the interest of time, while folks go through, maybe Mike can start looking at those values since we are going to apply a ABC control rule and list the P-star there.

MR. CARMICHAEL: This is the table of status parameters and such for Florida Keys; it is PDF Page 260 in the assessment report.

DR. BARBIERI: I think the next step, since we are looking at providing fishing level recommendations, it will be for us to apply a control rule. I am pulling that up myself and read through some like we usually do. Right there for assessment information, I think this would fall under Tier 2, reliable measures of exploitation of biomass. No, this would be 1; quantitative assessment provides estimates of exploitation and biomass, includes MSY-derived benchmarks. Okay, no penalty.

MR. CARMICHAEL: The best assessment it could be.

DR. BARBIERI: Uncertainty characterization, what tier are we there, high or medium?

DR. REICHERT: I would lead towards medium.

DR. SMITH: Just so I understand the process; this is where we would account for the problems that we have with the growth model and lack of contrast and stuff like that, right? Okay, thank you.

MR. CARMICHAEL: You don't have any criteria like that, which is going back to the whole discussion of yesterday of whether or not these categories gave you enough to really address assessments.

DR. BARBIERI: This is the interesting part of the discussions. If we had gone through this beforehand, before having that workshop, people would have a different perspective on some of the issues that could be added on, changed, removed whatever to create better resolution basically and taking in account some of these things that right now are not necessarily being captured by our uncertainty characterization.

DR. SCHUELLER: I was just going to say I guess I am falling in lines with Marcel; what he put on the table for medium, because of the statement full uncertainty not carried forward in projections. We've discussed some of that this morning related to selectivity and growth and some of those other major points. It doesn't really allow us to go any lower than that given what we've already determined.

DR. REICHERT: That was my point, we do have distributions, but this may give us some leeway in terms of including some other thoughts about the uncertainty characterization that may not be specifically listed in this list. I personally would still be comfortable with a 3 here, but I just wanted to mention that. I think that is also what you were trying to get at, John, there may be thoughts that we can consider that are not specifically in here.

DR. BARBIERI: Okay, Dimension 3; stock status. That is an easy one; it is overfished and undergoing overfishing. If you scroll down there Tier 4; and then for the risk, the PSA the productivity and susceptibility risk analysis.

MR. CARMICHAEL: The susceptibility score, according to MRAG, is high.

DR. BARBIERI: It is high according to the MRAG so that would be 10 percent.

DR. REICHERT: I agree with high. In previous assessments we kind of looked and checked to see whether we collectively still felt that that was correct, but I have no problem with a high risk given the species population characteristics.

DR. BARBIERI: In this case, I think one of the questions would be – and John Carmichael brought this up yesterday – in this case, because the stock is overfished, there will be a rebuilding plan. We could present the council with a trajectory of yield at F rebuild that will have to be put together.

MR. CARMICHAEL: Yes.

DR. BARBIERI: But as our control rule states there, the probability of rebuild in an F rebuild type of rebuilding scenario is 50 percent probability of rebuilding is the default value. We could actually present the council with a recommendation – in line of the uncertainties of the assessment and the P-star value that we determined, that we would be recommending a probability of rebuilding that would be equal to 72.5 percent and develop a rebuilding trajectory for the stock that is at a higher probability of rebuilding then the 50 percent.

MR. CARMICHAEL: That is your recommendation.

DR. BARBIERI: Yes. I don't want to put words in anybody's mouth; I mean, this is how our control rule right now in terms of rebuilding plans is explicit about; but we can always recommend something different if we feel that there is justification to do so. Would anybody disagree with that recommendation of rebuilding at the 72.5 percent? Okay, hearing none; I think we are there.

We are waiting, of course, for this process to take place before we could develop a trajectory of rebuilding yields, because we didn't know what the probability of rebuilding that the SSC would be recommending. I will get back with Mike. He has all the necessary information to develop that trajectory and provide us with those quantities.

MR. CARMICHAEL: Preliminarily at least. When it is in a rebuilding, the council needs to choose the rebuilding timeline, so they will need to know how long it takes to rebuild at F0. They will need to know if you can rebuild in ten years. They will need to see how long it takes to rebuild at like to 27.5 percent. Then if you can or cannot do it in ten years, that will determine the endpoint.

DR. BARBIERI: We could provide two or three different rebuilding scenarios and have those as options for the council to decide which ones to use.

DR. SCHUELLER: Are you looking for input on something right now; where are we? At this point what are we contributing?

DR. BARBIERI: Well, we are going through Action Item Number 3; provide fishing level recommendations. Since the stock is overfished, there will be a rebuilding plan that is going to be developed. There will be a yield stream at F rebuild or if some other probability of rebuilding that is going to be generated; so rebuilding at F rebuild has really a probability of 50 percent of achieving that.

The SSC can recommend to the council, in our opinion, based on the scientific evaluation of the stock and the assessment and all of that, whether we recommend that rebuilding at F rebuild is okay or if we recommend rebuilding at a higher probability of rebuilding as determined by the algorithm in our control rule, which is 1 minus P-star probability of rebuilding or at some other one other than those two.

We went through the basic algorithm that we usually do, the plain vanilla one-size-fits-all; but we're asking for your input as is this okay? Would you recommend something different? The council, because rebuilding schedules, as JC explained, have to do with management and risk. The council will pick which value it chooses to use.

MR. CARMICHAEL: You will recommend they do a rebuilding plan with the 72.5 percent chance. Then we'll find out if it can happen in ten; and if not, what is the time it can happen in. Then within that constraint of getting to 72.5 by that time period, they may choose fixed F, they may choose F rebuild, they may choose various things.

They will come back to you with alternatives for the rebuilding plan. I expect they will look at 75 percent of FMSY, because that has worked out in a lot of stocks recently. This just sort of starts the ball rolling. Now we can move onto the next stock component, right?

DR. REICHERT: In previous stock assessment reviews, we usually also recommend a timeframe for a potential next assessment. Given the uncertainties and given the discussions we had this morning; it may be good for us to say we would like an assessment or an update in this and this timeframe, potentially addressing some of the issues that we brought up and that were brought up by the reviewers.

I think it would be good to do that. That may give the council some guidance in terms of setting priorities or requesting Florida to take another look at this. To get the ball rolling, I would say in maybe three years would be good. That would provide some opportunity to potentially collect some additional information and possibly address some of the issues that were brought up.

DR. SCHUELLER: What is the max age on this again; it is 23, right; 25, okay. I guess I am just wondering why three years. I guess my gut instinct is like five years, because then we are going to get sort of a long time series on those indices. I don't know how quickly a rebuilding plan and some of these things will go into place; but doing it too soon doesn't really provide anything other than trying to address some of our concerns, I guess. The question is are those concerns large enough to warrant a quick turnaround. I guess I'm not so sure given the fact that we have a lot of other unassessed species.

DR. REICHERT: I'm comfortable with that. I just wanted to throw that out there based on the discussions we had this morning. This is a longer-lived species and maybe five years is a more appropriate choice. There may be some other data streams that become available, especially for the North Carolina/Georgia population in terms of the video index. I would be comfortable with that. too.

DR. BARBIERI: We have actually, Mike, five years. Are we there yet, guys? To me, this completes the fishing level recommendations for the Florida Keys/East Florida stock. We still have the Georgia/North Carolina stock now to develop those. One point here to start the ball rolling; my recollection of reading the CIE reviewers' report is that the quantitative assessment of this stock did not pass muster with all three reviewers.

The SSC has the flexibility or discretion to depart from that recommendation from the CIE review panel; but just to get our juices flowing; I am putting this out there. Any disagreement with the statement that this quantitative assessment did not past review and that catch level recommendations for the Georgia/North Carolina stock be developed based on some data-poor methodology or some other known quantitative assessment methodology or using a different level of our list of methodologies there.

MR. CARMICHAEL: That brings sort of a question for me. They rejected it. I guess just really didn't feel the data were sufficient to support the model, which makes me think that is a real question of saying is this best science? If the model is not appropriate for the data, to me that is not best science.

DR. BARBIERI: Right. Just to give you a little background – unfortunately, Mike Murphy couldn't stay for the afternoon; but I remember when this assessment process started and talking to Wade then and going through this whole initial looking at the data streams, all the inputs for the assessment; it wasn't until the last minute that Wade decided, well, there might be enough here, and he was really going out on a limb to put all of this into SS3 and try to run an assessment using a statistical catch at age approach. Really, it seemed that the information was very incomplete and that there were uncertainties that were so large that in my opinion it would deem this stock not suitable to be quantitatively assessed at this time.

DR. SMITH: Just a reply to John's comment; it is hard to evaluate whether it is the best possible science, because that implies that you are ranking it against something else and we have nothing to compare it to. At this time this probably is the best available.

MR. CARMICHAEL: You don't really have to rank it against something else; you have to rank it against established criteria. It seems like the reviewers didn't feel that it gave robust, believable results, because the model required information that it didn't have. Maybe in collecting some new data, then it would support an age-based model.

If you just had catch and indices and you applied it to an age-based model and it didn't work, you say, well, I need age data. That is so an age-based model in that case would not be the best available science, something less data intensive would be. One of the criterions in National Standards is about whether or not you know something in there; and it is not always real clear unfortunately in that language, but about being like the right tool.

I guess that is where I sort of had this thought on this one. I don't know that this is the right tool; and it seemed like the discussion around here was more like a more data-limited technique may be a better approach for this stock given the data that you have up to this point in time. We could do a better model down the road; the standards kind of address that, too. You shouldn't hold up for that but you shouldn't expect something to happen that is really not pretty realistic to happen.

DR. GRIMES: I really would say the same thing John was. It might be the only data that is available, the best available data; but if the data doesn't support using a certain analytical approach, it is not the best available science.

DR. BARBIERI: After that discussion, I think it would be good to have some bullet points that really clearly capture what some of those critical uncertainties or data problems are.

DR. REICHERT: Some of them are very clearly outlined in the report itself.

DR. BARBIERI: The thing is we can do this if we have a very detailed report that has some of those points clearly articulated. In the interest of time, we can do this later as we build our report, but it would be good for us to have some of that in our report that incorporates some of this concern and some of these issues.

Okay, having that resolved, then the next step is for us to make a recommendation on fishing level quantities. I think this assessment wasn't able to inform catch level recommendations for that stock; but what else could it do instead that could be provided? ORCS?

DR. REICHERT: I think if we look at our current ABC Control Rule, I think that is where it would end up.

DR. BARBIERI: One of the issues with ORCS – and Mike Errigo pointed it out earlier – is that usually we, as recommended by the ORCS procedural report that process involves – I'm sorry, it has already?

DR. BERKSON: Yes, because we went through the tables during the actual ORCS process or workshop.

MR. CARMICHAEL: You may want to consider, if you would change any decisions based on , just the northern stock, but otherwise.

DR. ERRIGO: We put the hogfish through as if it were one stock in the South Atlantic and came up with a risk of overexploitation category for it during ORCS. If you feel that is not correct, you can change it. If you want to use information from the assessment and justify that, you could, but there is currently through the original ORCS process a risk category associated with hogfish.

DR. BARBIERI: Excellent, because then we are ahead of the game. It is just a matter of getting now a landings' time series for that.

DR. ERRIGO: I can probably provide that given just a bit of time. I am actually trying to pull it out of the assessment, the input data. I would have had it already, except that older recreational

landings are in numbers with average weights, so I was trying to pull all that. I just need to do the multiplication and addition to get it, but I can have it with a little time.

DR.BARBIERI: If the committee is comfortable with that moderate high risk of overexploitation that came out of the workshop, then it is just a matter of us making that explicit recommendation to the council that we are going to use probably the ORCS approach and then have the quantities provided at a later time like we do for projections.

MR. CARMICHAEL: Tomorrow.

DR. BARBIERI: Or tomorrow.

DR. SCHUELLER: My only question is do we anticipate anything used for the ORCS approach to change given that we're only using a subsection of what was used in the past? Do we anticipate any changes of any of those?

DR. ERRIGO: I don't' know much about the North Carolina hogfish fishery, but I can give a rough look to see if anything glaring looks like it might change based on the criteria ORCS sets out for how you assign the category for risk of overexploitation. Then I can say you may want to consider looking at this aspect of the hogfish fishery to make sure that it is still the same given that it has been cut up into just the North Carolina/Georgia piece.

MR. CARMICHAEL: Yes; that is what I was thinking, why don't we try to get that done over this evening and go through with the ORCS and refresh what the scorings were. I know that one thing that was looked at was the trends; so if you can get the trends in catch, you can consider like the fishery characterizations. Obviously, the fishery in the Keys is very different from the fishery in North Carolina, so we may want to go through that perhaps tomorrow when we can get the information put together for that stock component alone.

DR. BERKSON: I can't remember the exact number of criteria we used on Table 4. We used some, we cut out others, and we added a few; but I think it is like 10 or 11. Most of them would be absolutely automatic for us to decide the value. The trend would be a little bit more complicated. It ought to be a really straightforward thing to do. That is easy for me to say.

DR. BARBIERI: My feeling, considering where we are now in our schedule and all and we still have two or perhaps just one stock assessment to review, but it is a fairly large presentation, I would say we defer this to be handled later.

MR. CARMICHAEL: Yes, and move into mackerel?

DR. BARBIERI: Yes. Our agenda includes the mutton snapper assessment update review. We discussed yesterday that we're not going to be completing that review at this meeting given the fact that the analysis hasn't really been completed, and we don't have the final report in place.

Joe O'Hop actually came over to present to the committee at some point, if time is available, where we are in terms of that assessment and get some input feedback from the committee on some of the points that could be adjusted or refined for that assessment that we are going to

finally review at our April meeting. In the interest of time, I would say we start with the mackerel presentation, which is Agenda Item Number 9.

We have John Walter. A little anecdote about this assessment that I thought was very interesting; we finished the review, and John Walter was the lead analyst for the South Atlantic component, so we finished that on a Thursday and he went home and his wife had a baby.

DR. GRIMES: And the other analyst got sick, he got the flu and came one day and didn't come the next. I figured he got the king mackerel assessment flu.

DR. WALTER: All right, thanks for the invitation to come talk about this assessment. I will say that the timeframe of SEDAR – and that is one thing that people have started to get concerned about how long it takes for these things, but the timeframe of a stock assessment from start to finish – from the start of the data workshop to the end of the review workshop is exactly the human gestation time.

I will say that I am the spokesperson for this, but it was the work of a whole lot of people involved to get to this point. I am at the mercy and generosity of a lot of people who have contributed to this assessment. I will start with the end and the summary, because I think that is helpful sometimes.

The CIE reviews – this went through a full CIE review. They were generally favorable to the model for advice given the data and the working constraints. That is a "thumbs up" in general for the assessment model for advice. Two, the stock status is that it is not overfished and not undergoing overfishing in the base and all the sensitivity runs. We have positive outcomes here.

Also, the Gulf is not overfished and not undergoing overfishing. As this stock mixes in a fairly substantial manner with the Gulf population, the linkage between those two stocks is something we need to consider in the Atlantic. That gets to the greatest source of uncertainty in the previous assessments, which was the winter mixing zone where all of the fish in the Atlantic and the Gulf go in the wintertime, because it is apparently warmer; like a lot of people go to south Florida.

This was a major source of uncertainty because it was a mixed stock fishery. Some substantial work was done at the data workshop to reevaluate that and to shrink the winter mixing zone so that the magnitude of fish that are not assigned to stock is much smaller than in the past. I will go over that in a little bit more.

Unfortunately, there is no strong evidence of a stock-recruitment relationship, so we will be forced to entertain proxies for stock-recruitment situations and for future recruitment. Then one other negative is it looks like the last five years of recruitment is estimated to be very low. Recent landings are also well below the current ACL.

That is something that we'll need to consider in our projection advice. Those are probably the take-home messages to consider throughout the rest of the more detailed information. Moving on; the outline, I will go over the basic life history of fishery details; the stock structure; then the model setup; the model results; stock status and projections. Fortunately, I think that there was

already a stock synthesis presentation done for hogfish; so I am assuming that everyone is 100 percent familiar with stock synthesis at this point.

I know it is not often that it is the most routine model applied to the South Atlantic situations. I will try to minimize some of the stock synthesis lingo and try to explain things as best I can; but given the timeframe and the complexity of synthesis, I won't be able to do justice to it. If there are questions about what is going on, please ask.

The basic life history is that the fish mix in the winter mixing zone. They go to two different areas for spawning and for the summertime, so there are assumed to be two separate stocks. There may be greater stock structure, but right now we don't have strong evidence of that; at least not evidence that defines the stocks any differently than an Atlantic and a Gulf.

They have sexually dimorphic growth with females attaining much larger sizes. The fish obtained the fairly large maximum size as it is. It is a coastal pelagic fish and forms fairly large schools at certain times of the year and spawns in batches from May to October. The fishery is mostly a handline fishery with some gillnet fishery.

It is mostly landed by trolling handlines. This is a picture of the fishery and one of the vessels in the fishery. The recreational fishery has a fairly large or a very important constituent base in tournament fishing where these tournaments are targeting the largest fish out there and have a fairly intensive tournament fishing operations.

There is also a fairly substantial recreational fishery, which is just private recreational and headboats for the species as well. In the history of assessments, it received about every two-year assessment from the pre-SEDAR mackerel stock assessment panel, and that was largely an ADAPT VPA.

Then in 2004 it went through its first SEDAR and used the VPA Tool Box, which was an adaptation of the ADAPT VPA. That used a time-varying stock definition, which was fairly complicated to implement and a fairly complicated definition. Then in SEDAR 16, a VPA was used as well; and the winter mixing zone was partitioned simply 50/50.

I'll go over what the winter mixing zone was in the previous SEDAR 16 in a minute. Then reference points were based on MSY proxies and the stock was deemed to be not overfished and maybe slightly undergoing overfishing. I think it was right at 1.0 on the F metric. Then in 2014 we've applied synthesis and then also a VPA for continuity purposes.

The new information shrinks the winter mixing zone. In the past, in SEDAR 16 the winter mixing zone went from Volusia County all the way to the Collier County Border, so the large green area; and from November through April all of the fish that were landed in that area were assigned simply 50/50 to one stock or the other.

That represented a substantial amount of the total removal of the stock, which meant that much of the removals we really didn't know what stock they were coming from. Now we have a new, smaller winter mixing zone that goes from south of the Keys to the Miami/Dade Border. How did this come about? What it wound up doing is meaning that only about 7 percent of the total landings are unaccounted for.

This was determined by some careful analysis of the landings by county and by state. These are the landings going from Texas, on the left, all the way to North Carolina. These are by month starting in November, December, January, February, and March going down on the column. And then we also looked at this for every year.

You could actually – if you look at every year, unfortunately at that level of detail it was confidential information – you could actually trace the migrations of the Atlantic stock of fish that would have been summering in the Atlantic down into the mixing zone. Usually what they wound up doing was stopping in the green area from the Atlantic, so that would be the Indian River/Brevard/Volusia area, and then also from the Gulf side coming down and stopping at around Collier/Monroe, which said that it didn't look like a lot of the landings were actually moving into this mixing zone; so it wouldn't be completely mixed 50/50.

The otolith microchemistry also supported that; that there was higher probabilities of a fish being one side or the other. The further you got is the closer you were to that. We could reasonably minimize the winter mixing zone by moving the bars, essentially from the red lines to the black lines, with Monroe landings being one of the major outstanding source of unknown.

The decision was made that all landings south of the Keys went to the Atlantic or were in the winter mixing zone and that the remainder of the winter mixing zone would be then partitioned 50/50. This reduced the uncertainty by a lot and is likely a better restructuring of the assessment.

That reallocation wound up meaning on average 6 percent of the landings have gone into the Atlantic, and increased the Atlantic by about 6 percent and then a decrease of 7 percent in the Gulf. That will hopefully have had a better resolution to the stock assessments and have been a better partitioning of the fish to the actual stock of origin. The assessment models that were applied were primarily stock synthesis, which was initially configured.

DR. SMITH: Just a point of clarification, before we move past the stock structure stuff, the assessment that we were given is a South Atlantic assessment versus the Gulf of Mexico assessment, is that correct? Okay, thank you.

DR. WALTER: Yes; I wouldn't want you to have to go through both of them, but this is key to the South Atlantic.

DR. SMITH: Right; so all the Gulf of Mexico data, the Mexico data and stuff that is in this assessment doesn't necessarily pertain to the South Atlantic I guess was what I was getting at.

DR. WALTER: The data workshop is one and then the assessments were separate.

DR. SMITH: Okay, I understand, thank you.

DR. WALTER: To try to minimize to some extent the confusion, the assessments were completely separate stand-alone documents. To go over the assessment model; initially synthesis was configured to exactly replicate the VPA so that we could get continuity of platform. If you make the same assumptions in the VPA and configure synthesis to essentially be a VPA, you can get almost the same answer, which is what we did.

Then we configured the synthesis model to the best practices of a fully integrated stock assessment model using length and age data. Synthesis I won't go over too much, but it is widely applied particularly in the west coast, and it has become more widely applied in the Gulf of Mexico. We had as our secondary a continuity model, the VPA Tool Box.

I won't go into much of the results from the VPA other than those results are documented in a separate document; it was a review workshop document. That model is primarily used for support and to confirm some of the results. The model structure uses the fishing year in the South Atlantic; so that is the timeframe. The winter mixing zone is partitioned 50/50, but this is a key distinction that only the stock-specific age and length composition was used.

The winter mixing zone total removals used accounted for each side; but the composition data, because that composition data is not known, is not used. We didn't take 50 percent of the winter mixing zone length comp and just put it to the Gulf or the Atlantic, because it would have given a mixed signal on cohorts.

But those removals were assigned toward the total removals under the assumption that the cohort structure that is obtained in the Atlantic-specific side applies to the Atlantic fish in the winter mixing zone. Clear as mud? It is easier than before, trust me, when the moveable winter mixing zone; it depends on what time of the year.

The model structure; there were six fleets that we modeled; handline, gillnet, shrimp bycatch fleet, headboat, charter/private and then a tournament fleet. One of the important pieces of information that we had was that the information coming from a project that evaluated the tournament fishery specifically, a project called the Fish Smart Project that characterized the tournament fishery both their selectivity, the magnitude of their removal, and how that operates distinct from the private recreational fleet; and what that allowed us to do is to specifically model a fleet that targets only the largest fish out there.

The nice thing about that is that we could assume that was likely to be a flat-top selectivity, because they were trying to get the biggest fish, they use different gear than the recreational fishery, and that gave us a flat-top; something we could probably assume was flat-topped, which winds up being a convenient situation when you are trying to model the selectivities.

We had four surveys. Unfortunately, three of these surveys are fishery dependent. We had a commercial trolling only from the handline fishery – it excluded non-trolling catch rates – a headboat index, the SEAMAP trawl survey index, which is largely an Age zero survey, and then an index of shrimp effort.

That is not specifically a survey, and it doesn't get treated quite the same as a survey in the model; but it is used to estimate the total amount of discards. I will go into how that is done in a minute, because it is fairly important and somewhat of a nuance. Then the tournament fleet selectivity, as I said, was modeled as a logistic function, so it is assumed to be flat-topped.

We had sex-specific selectivity that we modeled. We modeled females as an offset for males. When we set up the models, we tried to have the Gulf and South Atlantic be as similar as possible and diverge when necessary, so that we would have one comparable result in terms we could compare one or the other.

Also, when you are going through a joint review workshop, it is somewhat easier to have things be consistent than to diverge just simply based on maybe model or preference. One issue that winds up being probably not necessary, and the CIE review felt that there was probably over-parameterization of the selectivities that we may not have needed to model females and offset were males, but it was necessary, we felt, for the Gulf of Mexico that didn't have a tournament fishery that could be assumed to be flat-topped.

The assumption in the Gulf was that male selectivity might be flat-topped and then females would be dome-shaped as an offset to that. It is a relic from a decision for the Gulf that was retained. We did a sensitivity looking at when you take that offset off you get much of a difference in the results, and you don't. It is just a slightly extra parameter than needed.

Then all other selectivities are modeled with a double normal function, which can allow it to be either flat-topped or domed-shaped, depending on what the data indicates. Natural mortality was modeled as a Lorenzen function of growth, so declining with age; a base natural mortality of 0.16 based on the maximum age of 29; and then sex-specific Von Bertalanffy growth was estimated within synthesis using externally derived growth estimates as starting value.

Length comp was put in by sex and fleet, and age composition was modeled as conditional length-at-age keys by both fleet and sex. Some further things about the model start-up; we were able to track landings back to when the fishery was assumed to be virgin in 1900, which then we don't have to estimate starting conditions.

We can assume that it was virgin then because most of the landings were negligible prior to that. We had a fairly high CV of 0.2 for the landings for the private charter and headboat based on CVs that come out of the estimates of those landings in MRIP or MRFSS, as it were, of whenever those removals were actually calculated; so back in time, MRFSS now MRIP.

Then recruitment deviations were estimated for the 1981 to 2012 time period when we actually should have recruitment signal in the data, both in the length and age comp or in any of the indices. Then we have a time series of effort used for the shrimp fishery to estimate discards. I will go over that in a figure later on.

The fishing mortality proxy was the exploitation rate in number for all ages. In '80 the narrow vector was assumed with a constant CV of 0.1 at age. Initially, prior to the review workshop, the stock-recruitment relationship was estimated freely. The variability in recruitment was fixed at a value of 0.6.

Subsequent review of the model at the workshop recommended that steepness be fixed at a value of 0.99. In looking at the stock-recruitment fit, you will see that there is very little evidence of stock-recruitment relationship; and simply having to be estimated in the model may not necessarily be evidence that it is there was the decision that the review workshop felt.

That was fixed at 0.99. That will lead us to some further discussions about what to do for future recruitment that I will go into in terms of what decisions coming out of the review workshop were for that. But just to lead into that; then for projections an average recruitment over a certain time period was assumed. It is under the assumption that there is no definable stock-recruitment relationship, but recruitment and benchmarks are likely to be derived from recruitments that are

something like what we're seeing in the recent time periods; so essentially predicting with variables but recruitment at about the level that we're seeing recently.

That is something that often winds up being done when there is no stock-recruitment relationship. Then SSB was modeled using millions of female eggs, using updated estimates of batch fecundity; so SSB is just female eggs. Moving on; the shrimp bycatch, if you are familiar at all with what is done in the Gulf of Mexico; and I think in general shrimp bycatch has been a big issue in the Gulf of Mexico; it has not been considered as large of an issue in the South Atlantic.

Recently there was a shrimp workshop that I think was primarily a shrimp bycatch workshop in the South Atlantic; I think we're beginning to understand that we do need to incorporate bycatch into our models in the South Atlantic. One way to do that; within the synthesis modeling framework there is a way to not use the estimates of discards, but use the estimated vector of effort and then have the model calculate the discards that it would be predicted to do.

If you don't really believe your total discard estimate; so if you try to estimate the bycatch from either observer data or other scant pieces of information, but you generally believe the effort because we have some good idea of how many shrimps are probably taken, then that might be a better way to estimate the discards.

In this case, that was the approach that we applied. We had a vector of shrimp effort that we back-calculated to the start of the shrimp fishery, so the figure on the left shows the buildup of shrimp effort; the dashed line is the buildup over time. You can see that the line that is shooting kind of straight up is the pace of boat building in one of the main shipyards that built shrimp vessels. That was largely used as the slope of the increase in effort.

Then once we get to the solid line; that is where we actually have the traditional or the routinely collected shrimp effort data. On the left is estimates of shrimp discards that came from a generalized linear model of bycatch per unit effort that was developed to try to predict what the shrimp discard would be in every year. This comes from combined observer data and the SEAMAP trawl survey.

The SEAMAP trawl survey largely provides the inter-annual variability in that estimate of discards; because it is the only time series of the actual ups and downs of young-of-the-year fish. The observer data provides the scaling to the commercial fishery. We were able to estimate within the GLM the inter-annual variability and then the level that would be predicted from the actual commercial fishery, which was actually substantially lower levels than what was observed in the SEAMAP survey.

Let me just reiterate; the SEAMAP survey was used to estimate the inter-annual variability, but the absolute magnitude of those discards comes from the magnitude that would be observed on commercial fishing vessels. Then we generally didn't want to have to believe that as the truth; and so the way it is used within synthesis is it takes just that median level and then fits to that median level and uses the vector of effort to predict what those discards should have been.

It is a rather complicated effort to incorporate the discards. One of the benefits of doing this approach is that we're not telling the model two pieces of recruitment information; because if we

put in the red time series of recruits or of discards and we also put in the SEAMAP index, we're telling it recruitment is recruitment, and recruitment is recruitment twice from the same piece of data. In this case it avoids that double-recruitment signal.

The other points I needed to make on this complicated slide was that the bycatch reduction devises that were implemented in I think 1998 were also taken into account and retroactively applied to the discard estimates to say what would the discards have been had they not used BRDs, because all of the observed information in the recent time period used BRDs; so we had to then increase the discards back in time.

Then I think probably the take-home message here from its results – and this is a lot of work, but the discards are not that dramatic of an impact on the stock of estimate year, so the magnitude is not that high. This fishery is not being driven by shrimp fishery discards. We probably won't have quite as many issues as say in the Gulf at least with this particular species.

Moving on to the data treatments; to be able to incorporate the tournament information, the tournaments have about 25 percent of the total age composition comes from tournaments. We have all these really large, really old fish that if we gave it to the model just as 25 percent of the composition data, it would be severely biased.

What we needed to do is actually down-weight the tournament information according to the magnitude of removal for tournaments. The Fish Smart Project gave us the estimates that about 3 percent of the private recreational landings come from tournaments, and so that allowed us to incorporate the tournament fleet as 3 percent of the private recreational fleet beginning in 1980 when tournaments started and then ramped up to 1991 and continuous at 3 percent through the current time period.

The second data treatment; there were a fair number of outlier fish that were below the size limits in a lot of the composition data. It is quite possible that these fish were of a different mackerel species. There is both cero and Spanish mackerel that could have been caught at 12 inches and not identified.

What it does, it has very little impact on any of the quantitative results, but it means that you can't diagnose anything because the fits look so poor, so those were removed. That is kind of a convenience thing that otherwise you won't be able to diagnose any of the other problems and you try to model 12 inch misidentified fish.

I talked about the shrimp fishery discards. We used knife-edge retention above the size limits, with those retention limits changing in the years that the size limits changed; and then a time block in the tournament selectivity in 1997 to allow selectivity to vary, as it looked like that tournament fishery started to focus even more on it being a one-fish kind of a tournament.

As opposed to tournaments being bring in a bunch of fish; it was focused a lot more on really bringing in a single largest fish; and that would have been a change in selectivity. Then these are the time blocks for the size limits. This is the data type by year. It is essentially showing where we have catch data back to 1900 for the handline; and then when each fleet starts in, when the abundance indices come in, the length comp, the age comp, and then the discards, and then the model results.

The first thing we generally do with any of the models is to try to determine how well any of the key parameters are estimated and what conflicts there might be in the data. This is the plot of the log likelihood or the change in log likelihood by data component and overall. Overall is the black line; and where that meets the minimum value, then would be the maximum posterior density estimate for the virgin recruitment or not.

It is one of the key parameters defining the total magnitude of the population. You can see that it is fairly well estimated in terms of having a fairly steep drop-off at about a little bit higher than 9. That is the total change in log likelihood. However, there is a conflict between primarily the length comp data, which is in yellow, which would suggest a smaller value; and then the age data, which is in green, which would suggest a much larger value. We've got two pieces of information that tend to be pulling against each other.

Unfortunately, that is a fairly common situation with these integrated models where you are incorporating a lot of different pieces of information. What we like to see is that any of these parameters are estimated and then try to deal with the conflicts and resolve them as we can later on.

I won't show the likelihood profile for steepness, because that was not the model that we're using for advice, because we fixed steepness. I do have it as one of the extra slides in the back end of this presentation. I can show it and show the model-estimated steepness, but it was not felt that was a good estimate of steepness.

I've also got the likelihood profile for the other key parameter, which is sigmaR of the variability in recruitment, but I won't show that here right now, because I don't think they are critical to this. In the landings by fleet, in the left by each fleet and then the fishing mortality; I think the important thing to show or to point out is that the major forces of fishing mortality in this fishery are the charter private fleet, which is in yellow, which is the highest value and then the handline fleet.

But in recent years that has flip-flopped; so if you look at the plot on the right, the handline fishery is higher than the charter private, which has declined by a lot. In almost every situation the fishing mortality rate has declined substantially in the most recent years. That is a result that will be seen in the overall F metric.

One of the challenges in these models is determining what fishing mortality metric to use for the overall estimate of whether the stock is overfished or not, because any one of these fleets may have a different selectivity; so you need something that functions as a summary F, and this case we use exploitation rate in number.

I did look at exploitation rate in biomass or you can also use the sum of all the fishing mortality rates to then use to estimate the benchmarks. The results are robust to any one of those particular metrics. It wasn't a big concern to me of which one to use; we just needed one that we could use as an overall summary.

Moving on, discards; so on the left are the model predicted discards in thousands of fish; so total being about 200.000 fish. Then in the right are the fits to the discard series for each fleet indicating that the primary source of discards is the charter private fishery. The discards in the

shrimp fishery; you can see that the model is fitting just the median value of the time series and not to each inter-annual variability.

The total number I think is somewhere also around 200,000, so in age zero fish; so it is not a major source of mortality. Discard mortalities are shown on the left for each one of the fleets, too. In general this is not a discard-driven fishery, which in some ways makes it a little bit simpler to model.

Fits to the indices, fits to the handline index shown up on the upper left, the headboat, the SEAMAP survey, which would be age zero fish, and then the shrimp effort – the shrimp effort fits almost perfectly, and it doesn't really function as a survey. There is really no other piece of information for it to conflict with, so it says the effort is exactly what we gave it. It is a little misleading to call it a survey; it just happens to be how it is interpreted in the model.

But in terms of what is giving us an abundance signal, it is these three pieces of information; and we do a jackknife analysis where we remove one of these surveys at a time and determine which one is giving us the different signal, and I will show that a little bit later. The fits to the length comps – and these are aggregated over all years – I am not showing every fit to every sex and year and fleet. We would not want to be looking at that.

They are in the reports, so these are aggregated over years. Some of the lack of fit is due to a time block that is not showing here; so in particular the headboat has a time block in retention, and the tournament has a time block in the selectivity. The lack of fit is a little bit worse than reality in the model; but the one fleet that fits the worst is the tournament fishery.

In some ways the model is saying the predicted is the red and the observed is the gray. It is not fitting the tournament data perfectly. The tournament data is challenging to model in particular because I think there is a second set of selective process; so there is the selectivity that catches the fish and then there is the process of the fish being actually retained or weighed in at the tournament.

If it is not a tournament-winning fish, it may not actually wind up being weighed in even though it was selected and caught by the fishery. There may be an added process of selectivity that we're not able to model there. Then these plots cause a lot of consternation. This is a common output from the synthesis models; and I will take a little bit of time to explain them.

On the upper top is the scale bar. You see bubbles in blue that are positive. That means there is more fish than is expected at length. In white there is less fish than is expected in length. On the Y axis is the length going from 40 centimeters to 140 centimeters; and then it is for the sexes combined and then males and females for each of the fleets.

What we're looking for is some sort of systematic lack of fit. What I have shown next is a bad situation; because looking at these one might say, wow, this is terrible when in fact this is the best we could do. They were a lot worse to start off with; and the amount of systematic lack of fit is greatly reduced by a lot of the modeling decisions that were made.

I think that what you want is random patterns and outliers, and not severe outliers; but you don't want systematic patterns. You will see that there is a systematic pattern in the tournament where

there is more fish than predicted by the model, particularly for males. The fact that it is not fitting males here is a systematic lack of fit; but in general for the other fleets there are not these systematic patterns.

Now, really, these are best compared when you see what happens when it is bad. When we're looking at these and we see something that is a really poor fit; that is when something needs to be done and you might question the reliability of the model if something isn't. In one of the initial parameterizations in the model, the male selectivity was fixed to be flat-topped and thoughts that females would then be an offset to that.

What happened was that there was this systematic lack of fit where there were no large males, but the model was expecting that there should be large males. There are these big holes and that was a pretty systematic lack of fit. Then we allowed the selectivity to be dome-shaped, which substantially cleared up that pattern. This is a case of when things are bad and how one might try to correct for that.

Moving on, the selectivities on the left are the length-based selectivities. All the selectivities were estimated as functions of length with the exception of the selectivity for the shrimp fishery that was age-based only on age zeros. The selectivities show strong doming for everything except for the tournament fishery and slight divergence between the male and female estimates of selectivity.

These are the derived age-based selectivities from the length-based selectivity showing doming. There was a lot of discussion about the reality of dome-shaped selectivity; and as it winds up being a very critical thing in almost every assessment, it became a big consideration and some of the arguments that were brought to the table as to how this might actually operate is that there is some market preference for an intermediate sized fish.

In the commercial fishery there is a market preference for that and in the recreational fishery there is – because of the tournament information saying to get really large king mackerel you have to do something different than the average recreational fisherman; that said that, well, okay, the average recreational fisherman is likely to have a different selectivity and a dome-shaped selectivity than the tournament fisherman.

In particular a headboat fishery, which is a fairly non-specific type of fishing for most species; that seemed to say that these dome-shaped selectivities probably have merit for a fish like this. Because this selectivity is not simply a function of the contact selection of the gear and the fish, it is also mimicking availability; so these selectivities imply that there is a lot of biomass that is cryptic to the fishery.

There is one at the review workshop that was specifically requested that we calculate the amount of cryptic biomass. It is about 40 percent of the biomass is cryptic to the fisheries here; cryptic meaning they aren't selecting for it. But in terms of the explanation of where they happen to be, it is not as straightforward as they are just in deeper water as it might be for a fish like a grouper.

DR. SCHUELLER: I wondered if you could just speak to if anything is fixed in these estimations and selectivities and what is fixed, if it is.

DR. WALTER: In most cases the initial selectivity is fixed at zero, but in some cases the descending limbs are estimated to allow a smooth increase and a smooth decrease. There is an option to allow it to just smoothly increase and smoothly decrease. None of the selectivities at the tail end were estimated or were fixed to be zero. I will note that the parameters defining this doming were not well determined. There is not a lot of fish out at that size.

I think that is always the case that it is not well determined; the dominess or the strength of the doming; but in terms of other things fixed, that was the primary things that were fixed, yes; but obviously the tournament was fixed at asymptotic..

The stock-recruitment relationship; this is the estimate coming out of the model; estimated stock and recruits showing very little evidence of any kind of a stock-recruitment relationship. The goal was for benchmarks and for projections to not impose a stock-recruitment relationship, but then to just assume some average level of improvement and entertain an SPR proxy.

The estimate of steepness coming from the previous model entering the review workshop was an estimate of 0.5, which some felt was a low estimate for this species. I think the primary reason why I would argue that it was not a well-determined estimate in addition to this scatter plot was the fact that when we looked retrospectively by peeling off one year of data; that estimate of steepness changed from 0.7 to 0.5 within four years of peeling back a piece of data.

What it was saying is either we're getting a lot of recent information that steepness is very different, which doesn't seem like any recent information should have improved the scatter plots that substantially, or the estimate of steepness is rather flimsy and adding one year of data can tilt the balance between 0.5 and 0.7, which would have a very big impact on any kind of the benchmarks for probably a not well-determined result.

Here are the recruitment deviations. There were estimated to some to zero. SigmaR was estimated to be 0.83. They show some evidence of a pattern with somewhat higher recruitments during this time period; and then in the last five years all negative recruitment deviations. It is this pattern that is worrisome for our projections, because these would be the fish that our fishery would then be operating on in the future.

Here is the overall exploitation rate. I will say when we look at this on a relative scale, that it is lower than the benchmark. It is hard to interpret whether this is high or low, but we see that the most exploitation appear to be occurring in the nineties, and it is a lot lower in recent years. Then this is a time series of spawning stock biomass in red and black showing in the recent years an increase and then a slight decrease in the last two years in SSB.

That slight decrease in SSB is largely a function of the declines in estimated recruits as those fish that were not there are not maturing. The increase right here is the maturation of and the filling out of fecundity of these large recruitments. One of the conundrums of actually modeling and trying to attempt to get a stock-recruitment relationship is here we've got increasing SSB, which any stock-recruitment relationship would want to see increasing recruitment under that situation; and we have increasing SSB at the same time we have declining recruitment; so it is confounding the estimation of the stock-recruitment relationship with that piece of information. Sensitivity runs, there were many. Some of them were scoping.

When we build an SS model, we usually build it from the parts up by adding one piece of information and then seeing what happens starting with the indices then adding the length comp, then adding the age comp and seeing whether we get different information and seeing what changes. Those are largely scoping kind of runs.

Then we do a jackknife of the abundance indices, removing one at a time, retrospective analysis, evaluate whether natural mortality could be higher or lower. Then one key run was to remove the tournament data, because I was concerned that putting in tournament information, which is the oldest, largest fish, could artificially inflate our estimate of the stock status as being a lot rosier than it would be, because you would see all these old fish when the rest of the fishery might be declining; so it might buoy up the stock.

As it turns out, it didn't have much of an impact. Then equal index weighting; so the indices were weighted according to their estimated CVs. We then did a sensitivity run of equal index weighting, then lowering the CV on the recreational landings, removing the female offset on selectivity, down-weighting the length comp and age comp, and then comparing the indices and length comp model with the externally estimated growth rate fixed, so not estimating growth within the model.

I won't go into all of these and all of the results, because I think they can be summarized in one plot of what the stocks – any kind of implied changes in stock status. Here are all of the sensitivity runs plotted on a stock status plot of SSB over SSB at SPR 30 percent and F over Fspr 30 percent.

For all of the runs and then the base run – and I will note that these calculations aren't the calculations used to provide advice, because the SSB SPR metric is calculated using a geometric mean of recruitment. This is using the recruitment for the whole time period. But because that recruitment is lower than the recruitment from the whole time period, the SSB the status would be shifted to the right.

In any case. this is giving us a picture of what each of the relative sensitivity runs say; and none of them say that the stock is overfished. The dash line is the MSST; so that would be the definition of overfished; and then all of the runs say that the stock is not undergoing overfishing. I can go into more detail on these; I have some extra slides on any of these in particular.

I think for the sake of getting the larger message across; that tells us what we need to know by doing the sensitivity runs. The jackknife of the indices I will show specifically, because that is important to say this is the Sesame Street, which one is not like the others. Anyone remember that one? I won't repeat the ones that are already in my head, but they are a little bit younger crowd on my home team.

We see this on the top recruitment, spawning stock biomass and fishing mortality rate are key metrics. The one that stands out as different for recruitment is the SEAMAP survey, which is this line right here. If we take the SEAMAP survey out, which is really our primary source of age zero information, we get a different pattern of recruitment particularly in the recent years.

Taking the SEAMAP survey out says that recruitment, while lower, didn't decline as much as what the other pieces of information are saying. Then in terms of biomass, the headboat survey

is the primary outlier in terms of it is saying something different than the other pieces of information. The headboat survey, by taking that out, the SSB is estimated to be lower and the increase not as steep.

In terms of the most sensitive indices, it is the headboat survey for SSB and then the SEAMAP survey for the recent recruitment. Now, here is the plot of stock status relative to Fspr at 30 percent, using the geometric mean recruitment for 1990 to 2012. This time period was chosen, because it is the time period that we have the SEAMAP survey to inform recruitment.

The assumption is that future recruitment and the recruitment under which we calculate the benchmarks should be similar to the geometric mean recruitment during that time period. Our stock status says that we're well above one and that we were never undergoing overfishing. In the bottom, the fishing mortality metric says at no time were we actually undergoing overfishing.

This translates to what we call a snail plot in other arenas; the trajectory of stock status over time, and in this case we are through the entire time period in the zone of not undergoing overfishing and not overfished; the stock status estimated to be 2.5 times the SSB at SPR 30 percent and fishing mortality rate at 0.17 times the SPR 30 percent.

Moving on here is the yield per recruit and spawner-recruit plot. I think the main thing to show here is that our fishing mortality metric comes exactly from this plot, from just taking the spawner per recruit level and saying we want an SPR of 30, and that is the corresponding fishing mortality rate. These don't entertain any possibility of recruitment overfishing since we are just not using any stock-recruitment curve. It is simply from the calculations of yield-per-recruiting spawner per recruit.

These Fs are lower than the F at maximum yield per recruit by a substantial amount. The Fspr 40 is also plotted on here showing the difference between – that is about 0.12 versus 0.16. Again, I will reiterate that these Fs seem extremely low. If you are used to thinking in instantaneous Fs, but this is just a different metric; it is exploitation rate; but don't overinterpret this as being a low F. The fishing mortality rate for this stock is low compared to this.

It is not that we're using a low bar for F. One of the considerations that we're beginning to take into account in all of our stock assessments is the effects of environmental factors on the population, something that I think an emerging. We're developing greater capacity to incorporate these factors and we're developing longer time series of information to actually be able to detect them.

We're at a good convergence of ability with knowledge that we might actually determine that these are undergoing. We looked at an evaluation of catch rates and the effects of temperature on catch rates; and under the hypothesis that changes in sea surface temperature was affecting catchability of the stock, and so that the indices we're using would be biased by not accounting for changes in temperature.

We detected little effect of temperature at the spatial and temporal scales of our CPUE measures. Then when we put three different metrics of temperature, both mean sea surface temperature, degree days, and then an index of upwelling based on the amount of cold water during the summer time into the stock assessment and tested them against the residuals from the indices;

there was no significant effect. What that means is if we put any of those three indices of temperature into the model to try to correct for changes in catchability, it wouldn't have had any effect.

It says either temperature doesn't have an effect on catch rate; it is that within the assessment model it would not have had a beneficial effect to incorporate it. It may be any number of reasons why. Maybe the resolution of the data we've got, the fact that fishermen generally don't fish when the temperature is not right, so we're not getting as many observations across the full scale of where temperature is actually impeding the catch rates of the fish.

It is probably a topic of further research. Well, I'll go into the last part in a minute, but there was also consideration that there might be a spatial shift in the population as the climate may be changing. A fish that is largely a South Atlantic stock fish might enter into alternative management jurisdictions and have to be managed by other outfits.

We looked at data coming from the northeast and in particular their trawl survey and their larval survey to detect whether there is any increase in king mackerel; but it looks like it is unlikely that management jurisdiction for king mackerel will be shifted out of the South Atlantic any time soon based on there were very few larvae caught; and there doesn't look like there was an increase in larvae or in catch rates of small fish.

At least for that species, it doesn't seem like that. Right now it is a concern in terms of is the recruitment we are seeing simply happening somewhere else. Then predator/prey interactions were noted as it is something to be considered, but we had little data to evaluate that. Lastly, this is something that emerged later on after the data workshop, and really after the review workshop, was that there seems to be a significant correlation between the recruitment deviations and the location of the Florida current.

I will show what that means. When the Florida current – this is a picture of the Gulf Stream and the current velocity is the color scale. You see the Gulf Stream is in the left really close to shore right at the shelf edge. When the Gulf Stream is really close to shore, it impedes the formation of any eddies that would peel off the Gulf Stream; and those eddies are hot beds of productivity that pull in nutrient-rich water from deep areas.

When it is close to shore, it largely impedes the formation of these eddies. When it is further offshore, it allows the formation of these eddies, potentially leading to either more productivity, more food, better growth rates, any number of factors which could lead to changes in productivity of the stock.

When we correlated them with the recruitment deviations in the model, there was a significant correlation between – let me say what we correlated with. We correlated the western access of the Gulf Stream, so the longitude that the Gulf Stream was during the wintertime in each year and then correlated them with the recruitment deviations.

The central access of the Florida current at 28 degrees north seemed to have a significant correlation with the recruitment deviation, which suggests that any number of factors that might be related to productivity as a result of this could be affecting the stock; and the recruitment deviations can manifest themselves in the model.

It could be recruitment, it could be increased or decreased growth rate, it could be differences in survival. Recruitment deviations are just the sludge where nothing else fits so the model just puts them as more recruits; but it could be any number of those factors that is actually leading to that.

I'll just offer this up as what appears to be an environmental signal that does appear to be correlated with what the model is estimating recruitment to be. Where this comes into our advice in the future I think is we're going to have to make some decisions about what we think recruitment might be in the future and whether there is some periodicity to recruitment.

I will get into that a little bit more when I show the recruitment plots. Now the projections; the projection specifications, we projected four different F metrics, 30 percent SPR 40, 75 percent of SPR 30, which is almost exactly SPR 40; and then F current. The previous proxy was an SPR of 30 percent, so there is some historical precedent.

There was not a lot of discussion about 30 or 40 based on life history. At the review workshop it was mainly a carryover from 30 percent being used in the past. The selectivity in relative F among the fleets was averaged for 2011 and 2012. This is generally not something that we considered too much other than we did not average for three years because of effects of DWH in 2010 really affecting the relative fishing mortality amongst fleets in the Gulf; and we wanted to be consistent between the Gulf and the Atlantic.

There was, if you would look at the fishing mortality plots, a shift in relative F between the handline and the recreational fleet. What we're doing is we're projecting forward that allocation into the future. That is just something to note. Then the landings for fishing year 2013 and 2014 were not final at the time that we ran the projections, so we carried over '12 and '13, which were both low.

Now we have estimates for 2013, which were actually 18 percent lower than 2012 and '13. The stochastic projections I show here incorporate that lower value. We don't have the '14 fishing year estimates in yet; so those were carried over from '12 and '13 or '13 and '14. In either case it looks like the evidence is suggesting that the landings haven't increased in the recent years and haven't come very close to the ACL.

Then future recruitment, I've gone over this, it winds up being a geometric mean of about 7 million recruits. Here is the plot for recruitment. This is a pretty key thing for us to consider in terms of our future recruitment advice. With the last five years of recruitment being estimated to be low, our standard operating procedure would be to just project forward with what we think that recruitment is; the geometric mean recruitment.

Once we've settled on the years to average over, we would assume recruitment then reverts to that mean level, which would say that we would jump right away to this red line. But if we are stuck in a period of low recruitment, then we may not immediately jump back to that line. We may actually have some future low recruitment.

We entertained three hypotheses that we call a high-three, medium-three, and low-three recruitment. They don't correspond to high, medium, or low recruitment, anyway. Whether the recruitment in the next three years is going to be something like the average of these recruitment

deviations, which would be low; half of that average which would be the medium, which is the red line; or revert back to the long-term average immediately, which is this line; so we did projections with each of these three hypotheses.

When we show later there is a high probability of overfishing if you set the ABC assuming recruitment is high and it turns out to be low; and so that is one of the considerations that I will present in a decision table of what that probability is of overfishing if recruitment is something other than what we expect.

A next consideration for recruitments is since we've been substantially underfishing in the stock throughout the time period; the fishing mortality rate that we project into the future would be a lot higher than we've ever seen, and would be one and a half times the highest value ever seen and a fivefold increase over F current.

It would be a substantial increase in fishing mortality rate fishing at both Fspr and 30 and Fspr 40 percent. It is something to consider about how that fishing mortality rate might affect the stock. But because the stock is not overfished and the stock is much higher above the level – well, actually we want to fish a stock down to our target level; so there is some consideration that when the stock is in that level, we don't always get that situation where we're above the Bmsy proxy by such a large amount that we're actually in that situation; so sometimes it is a little counterintuitive that you would actually want to reduce the population. Lately we've been focused on how we rebuild.

This is more of how we simply manage a stock that is in fairly good shape. The projections at Fspr 30 on the left are again the recruitments; just to keep those in mind. The spawning stock biomass, what it would predict to do to SSB starting at the 2012 time period; it would be predicted to reduce the SSB.

The retained yields in millions of pounds would show us a big jump in the first year of the projections. I will comment on that in a later slide. That jump is something we've seen in some other stocks when there have been substantially low amounts of recent landings leading to a big buildup of biomass as sort of a lot of underages are creating a big surPlos in the most recent years; but these have some problematic results of actually declines in the ABC over time.

Then the red, green, and gray lines show the yields under each of the recruitment hypotheses. Then this is relative to the historic yield showing what the predicted yields would be compared to what has been removed in the past. Then here I show for each of the four F metrics only the high recruitment; because I think we saw before high, medium, and low recruitment just results in reductions in the most recent years.

In terms of comparing across the different F metrics, we see F current would be yields that would be slightly increasing over what is being landed now; but I think that F current is exceptionally low compared to any F we've seen in the past; but that was what was requested; spawning stock biomass under each of the scenarios, and then the entire time series of projected yields.

These are also the deterministic runs, so these aren't the stochastic runs coming out of the bootstrap. The uncertainty was quantified using our parametric bootstrapping approach. Our probability base advice will come off of the distributions from the bootstraps. Here is just the

table of those same values. I won't go over the table but it is useful for people to have this. Then I did a mockup of how we would generate the ABC advice using the control rule.

This is not specifically the P-star method as outlined in Shertzer et al. I call it sort of a P-star light, because it is not done every single year. In theory the P-star method would say, okay, here is your probability of overfishing, get your ABC and fish at that. Then the next year, once you fished at that, you would have a different level. That is very-time consuming.

It would take several days for me to do that iterative process. We'd probably get something similar. Amy, maybe you can comment; do you get something similar? I think Eric had mentioned that it is a really time-consuming thing.

DR. SCHUELLER: Yes; I thought they were working on – I don't do P-star myself. I do it for menhaden, but I thought they were working on something they were calling P-star light, so, yes, to speed up things. My recollection is that it was fairly similar.

DR. WALTER: If given a P-star value or probability of overfishing, then I can return the ABC at each one of the years from the distribution of the OFL fishing at Fspr 30, or Fspr whatever, or something else; but this is just an example using a P-star of 0.4. There is another potential option for ABC advice that is something that has been considered in the Gulf in situations where there is this large spike in initial yields.

This has some undesirable consequences of saying that you're going to have declining yields over time. That is often hard for people, particularly if the stock is in good condition, for that to be well received. That is not a scientific problem. That is more of a problem of councils haven't wanted to choose to necessarily do that.

Partly that is a function of the recent four catches being so low, leading to a surPlos of biomass is probably the large part reason for that. But one of the potential options is that – let me see if I can remember this. When the stock is above the target level that the yield is taken as the equilibrium yield rather than the yield that you would get fishing at the Fspr 30, so you take either the ABC or the equilibrium yield, which the equilibrium yield would be the yield that you would get at whatever Fspr 30 in perpetuity out 100 years or at that mean recruitment level.

What that would do is give a constant level of yield at the Bmsy or your proxy for MSY rather than this sharp increase. That is I think something that was considered similar for red snapper in the Gulf. The next thing is the consideration of what is the probability of overfishing if we assume the incorrect – if we have assumed high recruitment and recruitment is really lower than expected.

Here if we were to have a probability of overfishing that we initially set at 0.4 or any one of the values – I did this from 0.3 to 0.5 – and we assumed high recruitment, then the probability of overfishing; given that it was medium or low recruitment, is then the percentages on the right. If we set it at 0.4, assuming it is high, in 2015 we have a 46 percent probability of overfishing; 51 and 54. If recruitment is low, at 0.4 we have a 56, 70, and 76 percent chance of overfishing.

What happens is if we assume it is high but Mother Nature doesn't give us the recruitment, we have a high probability of overfishing. That is something we might want to take into account. I

will conclude now on I think some of the summary here or the assessment challenges. We had little evidence of a stock-recruitment fit, which means that we don't have simply an MSY estimate, so it is not as straightforward in terms of benchmarks.

I've gone over their concerns about what future recruitment may have in store for us; and then there is some lack of fit to the composition data. Primarily one of the big challenges for the assessment of this stock is we don't have an adult survey. There is no scientific survey that routinely catches adults of king mackerel.

We are relying primarily on the fishery to give us our information. Then it appears that there is environmental forces at play that are may be leading to a lot of the patterns that we're seeing, because these declines don't appear to be due to fishing mortality. It looks like the declines of recruitment are something environmental and not due to decline in F.

The strength of this assessment; the moving to SS from the VPA allowed us to model the male and female growth explicitly and separately. The unaccounted for removals are now a much smaller fraction of the total, so our largest acts of uncertainty is much smaller. We have a long time series of landings beginning at virgin conditions, a high volume of age and length composition information, Plos we have a juvenile trawl survey.

It is a fairly data-rich stock assessment. There is substantial biological information to characterize growth fecundity; and then the stock status is one where we have a fairly positive outcome and it is a matter of coming up with robust advice given that. I will just acknowledge all the people that contributed to this from various state, federal and academic agencies, and all of the panelists who sat through many webinars and through many panels listening probably to me more than they would like to; and then the CIE reviewers provided critical review and advice.

In particular the Fish Smart Project has been very helpful in providing us data and their work; and then scientists in the northeast and Mid-Atlantic who provided information that provided some ancillary evidence. I'll stop there with my presentation; I have extra slides on some things if people want to see them, but I don't think they are critical. They are documented in the report. I will turn it over to the Chair.

DR. BARBIERI: Thank you, John, an excellent presentation; very thorough. I appreciate you going through this much trouble. Now, we've been here for about two hours. I recommend we just take a ten-minute break and reconvene a little after three thirty to start discussion.

All right, folks, back to the grindstone. This is pretty much the same kind of process that we went through this morning that we usually go for all assessments. In this case since we had a regular SEDAR assessment process, including a review workshop and a review panel; usually we ask the Review Panel Chair to kind of present a summary overview if he has anything that he would like to put out there in terms of additional information for the committee and initiate some of the main discussion points. Jim Berkson chaired that review panel; and, Jim, if you can give us some of your thoughts or summary statements.

DR. BERKSON: There is not a lot to say, you all will be surprised to hear since I am giving the report. We had a really excellent review panel. I've worked with a lot of great review panels,

but this might have been the best one over all that I've worked with. Our three CIE folks were Noel Cadigan, Sven Kupschus and Arni Magnusson; three names I could pronounce, fortunately. Along with that, Luiz was on board and Church and Jim Tolan, who is on the Gulf SSC.

John Walter did a really good job in his presentation of talking about what some of the key issues were and how they ended up being the outcome of the discussions. For instance, there was a lot of debate over whether there was a stock-recruit curve or not. Well, we decided that there wasn't one; and therefore we went with a steepness of 0.99.

We also talked about cryptic biomass and its implications for selectivity and dome-shaped versus flat. John really did a good job of covering a lot of these issues that we discussed; so I don't see a lot of need for me to bring them up again. It was an interesting meeting. The analysts did a great job both in doing the work and the presentation.

For me this is always an issue of what do you do when you have no stock-recruit curve and your management is supposed to be based on MSY, especially when you have to do projections into the future? I am curious to see what everyone else thinks about that. Anyway, that is a short report.

DR. BARBIERI: Before we start formally addressing our similar list of action items as we had this morning, I think it would be good to give the committee the opportunity to review some of those outputs and presentation results and ask John some questions and get some clarifications. With that, we start with George.

DR. SEDBERRY: That was a really great presentation. I was particularly interested in the Gulf Stream eddies and the relationship between that and recruitment. They do cause upwelling and they do increase nutrients and that probably enhances larval survival; but those eddies are really important also in cross-shelf transport of the larvae from the spawning grounds at the shelf edge into the coastal nursery area where the SEAMAP trawl survey picks those young of the year up. It would be really cool to look at that in additional detail sometime, look at the satellite images of Gulf Stream eddies versus the abundance of those young of the year in SEAMAP.

DR. CROSSON: I also thank John. I was part of the workshop assessment panel, so I observed it then. It was the first time working with some of the Miami guys, and so I thought that was really great. I guess one of the questions I have for you -- I didn't pay as much attention to the Gulf analysis as I did to the South Atlantic one due to lack of time -- did they have this issue with the low recruitment in recent years with the Gulf assessment as well or is that something particular to the South Atlantic? I know you concentrated on South Atlantic, but I know Mike and you were working closely on a lot of these things.

DR. WALTER: The recruit deviations look very, very similar; except that I think the last year the Gulf showed an increase. It doesn't look as extreme but when you plot the recruitment deviations, they look very similar. They are lower in the last five years in the Gulf, but are not as low as the Atlantic.

DR. BUCKEL: John, there is one of the slides I was trying to understand the difference between – you showed I think it was a shrimp effort; and it was like in the eighties and nineties there was this increase in effort; but then the bycatch wasn't there. Is that because the recruitment wasn't –

or the bycatch didn't follow that pattern. I was expecting to see, okay, if shrimping effort goes up, you are going to see an increase in bycatch. I guess if the age numbers aren't there, then you wouldn't see that. Is that what was going on?

DR. WALTER: In this?

DR. BUCKEL: Yes, or am I missing something? On the right, bycatch and number, so that spike in effort in '90 to somewhere around '95 – no, on the left, sorry.

DR. WALTER: On the left.

DR. BUCKEL: Yes, so the spike in trip effort, but then that doesn't translate into –

DR. WALTER: A spike in discard?

DR. BUCKEL: Yes, on the right.

DR. WALTER: Yes; the effort and the discards didn't really correlate as you would expect them to. I think that is really because the only main information we have for the discards is coming from the SEAMAP survey, so it is just tracking recruitment. I agree that is an odd situation.

DR. BUCKEL: Along the SEAMAP, look for the SEAMAP estimates; what do we know about these age zeros? I guess I am concerned that it's a pelagic SEAMAP bottom trawl; so there may be times when those fish are up in the water column and not available to the trawl. The question more specifically is what do we know about catchability of age zero king mackerel and trawl gear as a function of some environmental variables like daytime or different factors that might change during a SEAMAP trawl survey and have to be standardized for?

DR. WALTER: The index was standardized with a generalized linear model. It accounted for a couple of the variables that may have affected the catchability. One of them was the temperature. Temperature I believe was in the standardization model for the survey along with a few other variables.

It is documented in one of the assessment workshop papers what the – the actual model I think used months, area, and maybe depth. In terms of other factors affecting catchability, you probably don't know. But I think the larger question is the reliability of that index as an indicator of stock status, and given the life history of the animal being more of a pelagic.

We've used it in the Gulf a lot. They are definitely caught, and they are caught in fairly high rates; a lot higher rates than what the shrimp fishery catches. For whatever reason, how the gear that is used in SEAMAP, the areas it fishes or how it fishes, catches higher catch rates. That was one of the reasons we couldn't use the catch rate specifically from the survey and apply it to the fishery effort.

I think the largest question about the reliability of the survey that was brought up in the workshop was the spatial coverage; that it doesn't extend as far down into Florida as a lot of the population. The main concern was in the southern extent of the survey and maybe not capturing the full range of the stock.

DR. SCHUELLER: My question is related to the SEAMAP index, too. The quality of the data, if there are any other data that suggest recruitment is reduced in the more recent time period, for instance, maybe some composition data. Granted they are limited, but the problem with it is it is at the end of the time series, right. Is there any other data available to support the notion that recruitment is reduced in the most recent time period?

DR. WALTER: Well, the composition data would be informing the recruits going back several years. They would recruit to the fishery at age 2. One year olds are caught in the fishery; so the comp data does have fairly recent recruitment signal. But in terms of the last year, it would only be the SEAMAP survey; probably only the SEAMAP survey.

But the comp data, those recruitments going back five years are definitely informed by comp data. Other information in terms of from other things not used in the model, in that case I think we do have some documentation from some of the fishermen about comp data in recent years that seems to suggest that there were recruitments that wouldn't have been seen by the model, because the comp data from 2013 and 2014 wasn't put in but we've got it.

It is something that would be useful to look at in terms of if you put that in, what would the model say; or, also is that important information for what the future might hold. We're always looking backwards a couple years. We're not looking back as far in this fishery, because they recruit pretty young to the fishery.

DR. REICHERT: John, I know this assessment was completed before the SEDAR 41 was halted. Have you guys thought about how potentially influential the headboat issues were for this assessment?

DR. WALTER: Let me just ask a question; you said SEDAR 41 was halted. I am assuming there was an issue with the headboat being used as an index, which was one of the concerns that were going on in SEDAR 41. The question is do the same concerns apply to our use of the headboat index?

DR. REICHERT: It is not just the index; it is the data associated with the headboats, so it is landings and index.

DR. WALTER: Okay, so the whole headboat information from landings, discard, and index is being called into question.

DR. REICHERT: The earlier years, not the most recent years; '92 and prior I believe.

DR. WALTER: Well, I just by random chance happened to land on this slide which is the jackknife of removing the headboat. Those issues weren't brought to the table in time, whatever they were. I can't comment on them other than the influence of the headboat largely is something that has occurred from the late nineties onward in terms of the jackknife.

The headboat creating any kind of divergence looks like it is about late nineties. If it is early headboat poor information; and not to say that the information in the early time period could have changed everything; I don't know what the issues are, but they weren't brought to the table.

DR. SMITH: I've got a related question regarding how uncertainty in the landings was treated. I think a value of 0.02 was used for commercial CV and 0.2 for a recreational CV. I was wondering how you arrived at those and if you explored any alternate configurations on that.

DR. WALTER: Yes; the 0.2 was derived from looking at the CVs on the early MRFSS data. I think the CVs are now lower in recent time periods;, but back in time they were rather high. That is where the 0.2 came from for the recreational. We did explore a run where we reduced that CV, and that was one of the sensitivity runs we showed.

The CV for the commercial; that was a decision that we thought the commercial landings were likely to be fairly well known. Usually we have to make some assumptions about the most important thing we put in the model. We like to try to at least put footholds down on the things that we hopefully can trust.

In that case, for this fishery we felt that the commercial landings were quite well documented. There wasn't much in the way of species misidentification and misreporting, so we felt they are pretty well known.

DR. SCHUELLER: In the report there is a retrospective analysis, but that is with steepness estimated, right? Maybe I'm missing it; is there a retrospective analysis after steepness was fixed?

DR. WALTER: You are missing it, because it didn't make it in the report. I had thought that the retrospective pattern would be improved; because one of the things that I thought was causing the retrospective bias, which we see a retrospective bias where the recruitment is always estimated to be higher than it subsequently winds up being; the SSB is subsequently lower than estimated retrospectively; so what you want to see is a random pattern around the lines and not a systematic change in their value as you peel off a year of data.

In the steepness-estimated model, we had a retrospective pattern that I thought was largely driven by the fact that as you added a year of data steepness changed pretty substantially, which actually changed everything else about the model; and so going from 0.7 to 0.5 in over four years of peeling of a year of data should have created a retrospective pattern. In this case steepness is fixed, so it says that something else is going on in addition to that. This is the retrospective.

DR. SCHUELLER: Is R-zero changing then with this? Do you know if it is changing as you're peeling years of data off or is pretty consistent among these runs? Another thought is just those low recruitments. I don't know if you've played with this at all; but if you push them up to sort of a mean value, does it take away some of that? I don't know, I've played with retrospectives a bit and it seems like it can, as you said, get at what some of the things are that are causing some of the issues.

DR. WALTER: Yes, I can check the R- naught changing. I can tell you right now what it is doing, because this is R-naught right here. It is changing as a sextet. What is changing is that these really high recruitments are getting dampened. The concern about these is you are not modeling the passes well; so when you project this forward on your terminal year, then are you projecting forward with a model that isn't just as bad as any one of those other years?

Perhaps in this case what our caution would be is it looks like we systematically overestimate the recruitment. This must have all kind of different – this is high sensitivity. Your second question is did I play around with trying to solve the retrospective patterns? I did extensively.

I went back with the steepness-estimated model, going back and looking at steepness 20 years in the past and looking at the estimates of steepness and found that it really just showed sort of a cyclical pattern, which was one of the reasons that I felt it was not well estimated.

But if there was some estimate of steepness, it was likely to be something that was a distribution around the estimates over the retrospective pattern; so it is taking that distribution of estimates retrospectively. That estimate of steepness I thought was probably a better course of action than taking any one terminal year.

For sure, the terminal year estimate was estimated to be really precise, so that was greatly underestimating our uncertainty about what it was, because the precision at which it was estimated was artificially low, as a lot of things are with these models when you simply use the Hessian-based estimate of the variance. I didn't work further with the steepness fixed model.

DR. BARBIERI: Are there any other questions for John; comments or points of clarification?

DR. BUCKEL: The spawning stock biomass is high in recent years, but yet landings are down. I guess talking with fishermen what is the explanation for that? It seems to be things aren't squaring up there.

DR. WALTER: Yes; and it goes to the selectivities. They are not selecting for the fish that are out there according to the model. They are catching primarily twos and threes, and those fish that have moved into the SSB are out of their window. If you look at the numbers at age and the mean age; that kind of illustrates what is going on that the mean age in the model has gotten a lot higher.

Part of that is the increase in the growth of the older fish, but also an apparent lack of younger fish coming back in; and so not having those younger fish is allowing the mean age to increase. This is an issue if you used like mean age or mean size as your only proxy for the population; that is predicated on recruitment being constant.

Here you could say, oh, everything is great because the mean age and the mean size is increasing. If you plot the mean size, you would see that, too. But it is due to also a lack of small fish that would be the ones the fishery really would be driven by. The fishermen also feel that the fish have moved offshore and away from them and that temperature effects have caused their reduction in catch rates.

That was one of the reasons they wanted that explicit exploration. It didn't show exactly what they say the real phenomenon is. I'm probably not doing justice at all to what any individual fisherman feels is going on. I say that they probably have a much better feeling for whether this jives with what they are seeing or how it might factor into what they are seeing. I don't want to pretend to speak outside of –

DR. BARBIERI: To that point, it so happens that Chairman Hartig is a dedicated king mackerel fisherman with long-term experience; so it would be good to get some of his thoughts and perspectives in addressing Jeff's question.

DR. HARTIG: It is interesting, the impacts, why the Fs are going down when you still have a big biomass of great big fish. Selectivities are certainly one of the issues. As these fish get older, they get much harder to catch. Most of the fishery, both recreational and commercial, is a trolling fishery and they are mostly targeting the smaller animals.

Even the fish as they get older, they get wiser and they get smart. They don't really get caught in the same numbers as when they're younger as when they get older. There are a couple of fisheries that do operate live bait fisheries that catch some of these bigger fish. But even those fisheries, if you don't have the middle fish that creates the competition to get them rolling – and we fish a live bait fishery – if you don't have the middle fish, the big fish don't get riled up and they are much harder to catch.

In the fishery we fish it is mostly bigger fish in the summertime in that spawning group. We'll get fish around our boat and we can actually see them in the water eating all our chum; but if they are not wound up enough to get excited to catch them, they just eat all your chum and they are doing just fine. We feed them a lot.

The other thing is that as that middle class has gone away, the effort in the fishery drops off dramatically. The king fishermen on the east coast are now fishing in the Gulf in the summertime. All that effort that was there in the wintertime to catch the mixing zone is not there in the summer. There is a significant drop in F that way.

Then the other way is when you don't have all those fish, there are a lot of permits that just go dormant. When the fish are hard to catch and you have to go every day and you are not getting the catch rates that you used to get; a lot of people drop out of the fishery. There are a lot of different parts operating on why F is going down, which isn't all bad, because on that spawning stock you probably don't want a lot of F and allow that to stay there to some degree to be able to produce your recruits in the future. That is some of the explanation.

Then the environmental variable, at least where we are, which is the area of action where the fish spawn right around that Jupiter are and where you saw on that water mass chart where you showed that eddy; that eddy probably started right there at Jupiter where the shelf starts to break off. That has been an important part of these animal's spawning characteristics over time. Moving to that area in particular, eggs and larvae get entrained.

Those eddy, and then as George said, then moving those animals to an appropriate place where they can grow up, where the shelf is broader and much more nutrients and much richer environment; but what we've seen in the very near term is that the eddies are forming on almost a weekly to biweekly basis when it was normally every two weeks or three weeks we would get an eddy that would form.

What happens in those cases is we get these upwellings, the fish are much more mobile, they are very flighty, they move through; so we never get any time on the fish anymore. In times past when we would get two weeks to catch these fish, now we see a wave of fish move through in

three days. If you are not there on the first day when the bulk of those fish are moving through, you missed the bite. You get two days of progressively smaller numbers of fish to fish on.

But we have to be there every day to make sure that we can participate when those waves of fish are coming through. It is a complicated situation that we're going through, but the other thing is that we'll tell you a little bit later is the last two years – and Amy your question was about what are we seeing in the last two years to maybe inform that recruitment thing that you are going to be looking at a little bit later in those three levels of recruitment.

We have seen a year class or two possibly that has shown up in the last two years. That was my concern when we went into this assessment. John, we had great discussions about it. Thank you for bearing with me and getting this recruitment scenario so that if we did see something change, we could make that change here at least at the SSC when you see a higher recruitment moving in. The recruitment in the past was real.

There is no doubt about that at least based on my observations. Mine go back 40 years and a number of different fisheries when I interact with small and big king mackerel. Spanish mackerel, I see the little kingfish mixed in that; and we had not seen that for four years. I was alarmed enough to bring my fishermen together a couple years ago to say, hey, let's back off on our trip limits and be a little proactive. That didn't work, because the assessment was so close. But, anyway, it is complicated and I do have some hope now for the future, which I hadn't seen. We see a year class coming up. I'll stop there.

DR. SEDBERRY: Ben mentioned those eddies again and that reminded me that back in the seventies – this is a little bit of trivia, but back in the seventies MARMAP had an ichthyoplankton survey, and that scomberomorus larvae were abundant enough in those surveys to actually say something about them.

Collins and Stender published a paper on that in '83 or 4, somewhere back in then, and found significantly more larvae associated with I believe the Charleston Gyre and the Daytona Upwelling, which are both eddy events. Again, I think these eddies might be really important in early life history of these fish and affecting recruitment.

DR. SCHUELLER: Two points of clarification, I guess; one is related to weighting. Did you guys look at doing any iterative reweighting or what are your effective sample sizes here? Then the second question is for your bootstraps, when you are doing your bootstraps, you are looking at the Hessian-estimated uncertainty around parameters, is that correct? Then are you adding any data uncertainty into those bootstraps?

DR. WALTER: Yes; the first question was did we do any reweighting of the sample size. We did not iteratively reweight the sample sizes. The sample sizes were the input sample sizes up to a max I think of 200 on the length comp and age comp. We did not do an iterative reweighting according to like the model-estimated effective sample size.

I won't go into the pros and cons of it, but we didn't. The second question was did we look at, in terms of the bootstraps, how much uncertainty they actually incorporate; did they incorporate data uncertainty? No, so it takes the distribution of the sample from the expectations. It does not

then resample from like the residuals. It is not going to create as bad of a dataset as what you've got. In that case I think one might say that it doesn't fully represent all of your uncertainty.

DR. BARBIERI: Just on that point; that generated quite a bit of discussion at the review workshop as well in trying to understand the configuration and how the bootstraps were structured. Especially for this SSC application of the control rule and evaluation of that spread around the OFL, how much that would be captured by the uncertainty characterization of the assessment.

DR. WALTER: One comment to that. We haven't seen the difference of the ABC from the OFL, and I think in that case we'll have to come back to that if we see that the ABC is actually quite close to the OFL, which means that the distribution is really tight. I believe it is going to be quite close to that in in which case it is going to be a situation of probably saying that not enough uncertainty was captured in the bootstrapping. It is a common situation that we are just unable to quantify all of the uncertainties we have, particularly with single model and particularly with the means that we have available to us.

DR. BARBIERI: Right; and there are situations where you cannot get as much spread as you would like to. For us it is just informative to have that so we can qualitatively make some decisions here how to handle the situation. Are there any other questions or comments for John?

MR. CARMICHAEL: I think we should probably go ahead and list a couple of the uncertainties that John noted, but make sure they get in the report about inability to estimate a stock-recruitment relationship leading the fix in steepness. There was great uncertainty in even short-term recruitment estimates, because that is certainly going to affect your advice, I expect. Then there was lack of fishery-independent surveys really particularly for adults. Those are some of the things that we should certainly note.

DR. WALTER: Just a comment on that point about the ABC not being very far from the OFL or them being pretty close; the blue line would be the ABC. The OFL would be the value at 50 percent; so taking the red line and looking at 50 percent; and so that would tell us the difference that we're seeing from that.

Ideally we would like to be a buffer that represents enough of the uncertainty we think we have; but if it is only about a 5 percent difference, then it is really not much of a buffer. I think this illustrates the point. Then here I did plot the OFL and the ABC, so that would be the difference between the red line and the blue line for the high three recruitment hypothesis and then the ABC for the low three recruitment hypothesis. Just showing the difference between them is not that great.

DR. BARBIERI: Having captured some of the main points of uncertainty; I know we have a review panel report that perhaps we can kind of dig into later in the report preparation and sort of add some more to our SSC meeting report as well. I think we are at a point where we can go into – unless there are any additional questions or comments, we can go into our action items as we did this morning. Number one being review the assessment and consider whether it represents the best scientific information available.

DR. REICHERT: I would say yes.

DR. BARBIERI: I see a lot of nodding heads around the table. Would anybody disagree that the assessment really captures well the dynamics of the stock and provides reliable estimates of reference points? Okay, so with that, we have already discussed identify and discuss assessment uncertainties.

We have already discussed some of that; but perhaps not in the context of our application of our ABC Control Rule and how we want to deal with the amount of uncertainty that the assessment has been able to capture and characterize. Perhaps we start by going over our control rule. John mentioned explicitly during the presentation that this was proxy reference points.

In our Dimension 1, that would be our Tier 2; reliable measures of exploitation or biomass, no MSY benchmark proxy reference points, Dimension 2; uncertainty characterization. Amy, to that point of evaluating how much the method is used and how the configuration of those bootstraps were able to capture as much as the uncertainty as we would like to see; we do have distributions of Fmsy and MSY, so those are not lacking. That will put us in that medium tier.

MR. CARMICHAEL: Or is it high; what else is included in the uncertainty?

DR. BARBIERI: John, we discussed this at the review workshop, remember, and that was going full circle a little bit about the issue of trying to understand more about how the bootstraps were configured. When we look at our control rule here, we can actually make the best assessment of how much of that was captured.

DR. SCHUELLER: I guess the reason I asked the question is typically the stuff that is coming here has data uncertainty incorporated into it, such that age and length compositions are sort of resampled and then input into the models and the models rerun. Indices usually are the same thing, landings the same thing.

You are getting sort of that data uncertainty as well as uncertainty in parameters. For instance, let's say steepness wasn't fixed but it was estimated so you could put some distribution in there or whatever. There are any number of things that could be incorporated; and so I guess for me I am not sure if this is high or medium, because I am not sure what full uncertainty means. Obviously, we don't have model uncertainty in any of these. I don't know that you guys ever looked at sort of model-averaged assessments or anything like that. Then there is data uncertainty, parameter uncertainty, those types of things; what does full mean, I guess?

DR. BARBIERI: To that point; I think John was trying to tell Amy how much of that uncertainty is being carried forward into projections or how much of that we're being able to capture to have a spread of that PDF of OFL that we believe is capturing more. That is why to me it would be medium given the outcomes that we saw there; your comment about the buffers being relatively small and the distributions being relatively narrow.

DR. BERKSON: I think the definitions are a bit vague given all the different kinds of uncertainties there are; but I think medium is consistent with how we've evaluated other stocks on the sliding scale of vagueness. It is not that big.

DR. BARBIERI: You said medium and I don't see anybody disagreeing with that. We are going with the medium. Then stock status, Dimension 3; it is neither overfished nor undergoing

overfishing. That one is easy. No penalty there. Then for the risk analysis in terms of productivity and susceptibility – Marcel.

DR. REICHERT: Under 3 you said one or two?

DR. BARBIERI: My reading is that the stock is at fairly high. It is not really close to the benchmark values.

DR. REICHERT: You just mentioned that overfished or overfishing and not the second part, so that is Number 1, because we feel that stock is at relatively high biomass relative to the benchmark values. It is not in close proximity to the benchmark values.

DR. SCHUELLER: What are the benchmark values? Are we getting there but I don't know how to – and to Marcel's point there was 30 percent, 40 percent, 75 percent. There are multiple options presented. Since we're not using Fmsy, what is our benchmark? I am not clear.

MR. CARMICHAEL: Currently it is F 30 percent.

DR. BOREMAN: I don't know if this is the time to throw in some caution to the council about this, but we were in a situation with Atlantic mackerel. About ten years ago it looked similar to this. We had what we thought was a stock biomass that was two or three times higher than the Bmsy level. Now we can't find the mackerel anymore on the northeast shelf.

They must be somewhere else. Since we keep getting recruitment, we can't find adults so recruits are coming from somewhere. Maybe it is a species change going on there instead of a sex change. I like John's recommendation to do this F 30 percent equilibrium, kind of, so you don't go all the way to the extreme for the ABC.

What we've been doing is something similar to that; and like on scup we had the same situation. All of a sudden our ABC jumps way up. We go to the council and say here is the ABC but we recommend an incremental approach so the fishery doesn't over-expand and we find out later that we overestimated stock biomass or something, either way. I think there should be some caution there. Don't get too excited about these high ABCs that could come in. I don't know if this is the place to put it, but I think somewhere that should be a note going back to the council.

DR. BARBIERI: Good point, John. As John Walter pointed out, in the Gulf the SSC has kind of considered some of the same approach for that same reason. It was an iterative process in that case with the council and them telling us how much they would like to go to that fishing down the SSB to something that would be approach SSBmsy or keeping that high abundance level, depending on how they want to manage the stock.

DR. BERKSON: Obviously, we can give them the standard ABC and then say here are other options you may want to consider and give them two or three with what we see is the pros and cons of the other options. I think that would make a lot of sense in this situation.

MR. CARMICHAEL: I was going to say that is what you've done in the past is you have applied the control rule since that is developed for you and the council; and then you have given the recommendation such as consider like an equilibrium type yield recommendation. In some

cases the council has gone with that in some cases they haven't. At least it lets them weigh the risk; and you talked to them about the risk of trying to fish down the fishery. In this case I think you could bring up the uncertainty in the recruitment. It gives you an opportunity to make some good scientific advice points as to how they may want to judge the risk of fishing down versus maintaining an equilibrium point.

DR. CROSSON: I remember when that scup assessment came back on the Mid-Atlantic, when we reviewed it on the Mid-Atlantic SSC, and it was something like B over Bmsy was like over 4 or something really high; and there were some fishing representatives in the room that were very excited about that when those numbers first came out. The caution I think was well put.

I think one thing to keep in mind when we do give advice to the council and derive whatever number comes out of this process is that given what we've heard that a lot of the biomass, this excess biomass that are in older age class fish that are not necessarily being selected by either the commercial or the recreational sector, I guess with the exception potentially of the tournament fish, which is a smaller component of the fishery; even if the ABC or the ACL is increased, there is certainly no reason to suspect that that is going to drastically result in an increase in fisheries effort unless there is some big change in regulation. I don't even hear, listening to the chairman, say that there doesn't seem to be an issue with regulations so much as the presence of the fish moving offshore.

DR. BARBIERI: Yes; but to circle back to Amy, because the review panel actually was discussing this at the time, because in trying to anticipate some of the discussions here and going back to the council's established proxy reference point for king mackerel; as John pointed out over time it has been that 30 percent SPR or yield at 30 percent SPR. That is going to be the starting point. We addressed the stock status and we still have to look at the risk analysis and the value that came out of the MRAG is that this is a high risk.

MR. CARMICHAEL: It comes out as high risk by MRAG and that is what was used last time.

DR. BARBIERI: By the way, folks, not to sound like –

MR. CARMICHAEL: Monday?

DR. BARBIERI: Yes; I think we should have structured the ABC Control Rule Workshop to the end.

MR. CARMICHAEL: Where we remembered all these problems that we run into with assessments?

DR. BARBIERI: In every meeting we run into these things that right now explicitly our control rule says that instead of making the assessment of the risk analysis ourselves, we are going to use the scores that are already reported for the South Atlantic waters by the MRAG reports The workshop was like an opportunity to discuss this. One of the terms of reference was like do we keep this, do we modify, do we remove; how do you want to handle this?

The same thing about the point that Will presented this morning; what is in that uncertainty characterization? Can we add a little more resolution there to generate some points that are not

as vague, perhaps more inclusive. Some of these things is because invariably, as we applied this, we end up getting into this situation. Right now unless we have a revised control rule –

DR. BOREMAN: I just have a hard time accepting that this is a high risk for mackerel. Mackerel is a very productive species. In our ABC rule for the Mid-Atlantic, we have what I call a sanity clause. At the end of everything it says, however, here are our rules, but at any point the SSC can vary from these rules as long as we can justify it. Is there a similar statement in the South Atlantic?

DR. BARBIERI: No; and it is interesting that we mentioned Rich Seagraves presentation a couple months ago, and Rich had that there. That caught my attention that clause was there. I can tell you the Gulf SSC has added a clause like that as well that would say as long as we justify – and we have actually double-checked with legal counsel about this. NS1 is explicit that SSCs have the discretion to generate catch advice outside of established and approved ABC Control Rule if it is well justified.

DR. BOREMAN: This would be a case where I think we can well justify it, but as you're saying I guess we're stuck at this point.

DR. REICHERT: But maybe not, because if you look at the PSA score – I think we can go very quickly go through that – its high overall score is 3.2. What is the cutoff, 3.10 or something; so it is just on the lower end of high. I feel there is at least two or three of the productivity and susceptibility scores that I would feel very comfortable adjusting. I agree with John that medium risk is probably more realistic for this stock than a high risk. I think we can justify that by quickly look at that. That is Page 3.

MR. CARMICHAEL: I completely agree; but I think the problem is our rule has said that we will use that. That is why we had that as part of the workshop, because we had several stocks that this came up with when we did the ORCS Workshop; that we thought some of those things don't seem right. That is why we said maybe we should reconsider using this MRAG PSA business at our control rule; and maybe we should apply our own judgment with some of these. We need to fix that.

DR. BOREMAN: Yes; I would like to make a recommendation to our workshop on Monday that we add – not go back and fix this specifically, but add a clause to the control rule that says that it gives us the discretion in a case like this where we can change it as long as we can justify it.

DR. BARBIERI: It is not too late for us to do that, because our report is not even written yet. It is going to be circulated. We can, beside all that discussion, still do all of that. It is just procedurally.

MR. CARMICHAEL: We can't do it for this.

DR. BARBIERI: We cannot do it for this, because we have already approved the ABC Control Rule. If you modify it, it needs to be approved by the council before we can actually officially use it.

MR. CARMICHAEL: All of these things lean toward the medium and high. We saw that in Mike's scoring results that in his presentation so many of them fall in medium high. Fifty-three percent have been medium risk, 46 percent high risk, and there has been no low risk.

DR. BOREMAN: Well, maybe no low risk in this region, but other parts of the country we have definitely species; butterfish we see as a low risk species. It is just the way it works out. It is just the slate of species or the deck of cards you've been dealt with down here I guess you've got to play with.

DR. BARBIERI: I'm trying to reconcile in my mind that this assessment estimates that about 40 percent of the stock's biomass is cryptic and not really susceptible to the gear and vulnerable to the fishery. To me that wouldn't even be moderate vulnerability and moderate susceptibility.

DR. CROSSON: Just looking at these categories; I know we have to follow the control rule that we have right now in place; but in my mind something that is low risk, if it is highly productive, which there are fisheries that are highly productive in the South Atlantic; and even if it was highly productive and moderately vulnerable and moderately susceptible, I still wouldn't feel like it was a particularly risky fishery, because it is going to bounce back. That is the essence of it. This is just a really hard thing to swallow for this fishery. It sounds like it is red snapper, which it is not.

DR. BARBIERI: Yes; that was a mistake and lesson learned about how we structured those workshops; to have them in a way that we can actually through the meeting get a refresher, because we have these things once a year, either the April or the October meeting that those issues pop up.

But then, of course, we have so many other things going, we kind of forget about what the specific issues are. In this case, fortunately we have still enough time to go and make some of those adjustments, to the point that we may not be able to have anything ready for April. We might have to go even later than that.

DR. CROSSON: I know this is certainly a year are away from the actual discussion of the king mackerel ABC recommendation; but one more thing, thinking about this productivity and susceptibility and using the MRAG numbers; it seems like there should be something in this PSA analysis or this category or bin, or whatever we're calling it, that accounts for a history of overexploitation or not. Because if you look at the history of king mackerel and everything we've seen that it has never been overfished and overfishing has never occurred; and it seems like that is something that should be incorporated into any kind of PSA score.

DR. REICHERT: In terms of we cannot change that – and someone can help me, but I think there was at least one stock where we did this exercise and changed the PSA score. I forgot what species it was, so there is a precedent. Off the top of my head, I think it was cobia or something. But I may be mistaken; but to my recollection I think we've done this at least once. But again, I may be mistaken, but I thought we changed that.

DR. BERKSON: Just like ORCS, there are an infinite number of characteristics you could have on here and an infinite number of criteria and then an infinite number of weightings. I don't

know that we want to get into that. I think having the get out of jail free card sounds like it might be a good option.

DR. BARBIERI: Mike, if you can go back we should have our total score. Okay, so we have a P-star of 32.5 percent. Providing fishing level recommendations, while Mike might be working on filling in the table or perhaps he will do it overnight; so a couple of things that we usually do, just to refresh our mind and generate some suggestions from the committee, is we need to, as JB pointed out, discuss supplemental information that goes beyond projections that adjust to that P-star level is one thing.

Another thing would be the length of time that we want to recommend in terms of the projection timeframe. Then we have also usually at this point provided some recommendation to the council on when to conduct another assessment, whether it should be a benchmark or update, if there is something that is not obvious, and then the timeframe for that. Those are usually the issues that we provide recommendations to the council associated with the fishing level recommendation. If we start by looking at the scenarios that we would like to look at for projections.

DR. BELCHER: What was the projection period we were using the last time?

DR. BARBIERI: Last time for king mackerel? I don't remember.

DR. BELCHER: Recommendation on the last one.

MR. CARMICHAEL: For?

DR. BARBIERI: King mackerel.

MR. CARMICHAEL: For your ABC?

DR. BARBIERI: Yes.

MR. CARMICHAEL: The last time was a little complicated. You had a P-star of 27.5. There were runs at like 28 and, I don't know, 30 or something; and there was some interpolation between those two values that was kind of complex.

DR. BARBIERI: Yes, because the last time we had the different states of nature, remember, that could not –

DR. BELCHER: We still had time projections. How many years did we recommend?

MR. CARMICHAEL: I think you went out to 2013. That was like 2010; and you went out like three years.

DR. SCHUELLER: It seems to me given that the statement has been made it is a recruitment-driven fishery and there is a lot of uncertainty in recruitment; I would do short-term projections. I don't know; I think maybe add five or six years on here. That seems reasonable to me. Anything beyond that, you are just sort of guessing, right?

DR. BARBIERI: Amy, I didn't hear you when you said like short-term projections; what numbers were coming?

DR. SCHUELLER: I said five or six years.

DR. BARBIERI: Yes; and by the way, usually five is the maximum number; usually, not necessarily, but usually so we go sometimes with two or three or five; but we usually never go usually, sometimes.

MR. CARMICHAEL: The last time you did three, but realistically is this going to be assessed again in three years in time for you to have action in three years? It is unlikely.

DR. BOREMAN: Well, that is the question is how long do we feel comfortable waiting until we either get an update or a benchmark? This species is not going down the tubes. I think for this species we could probably wait a little longer than we would for others, like goatfish or hogfish, whatever it is.

DR. BERKSON: Are we also talking about how we would come up with a relative F value and turn it into an ABC for the time period? In other words, John had that last slide that looked at continuing catch at current levels, letting it max out, and then somewhere in between to basically say you probably don't want to let it max out, because you are going to have that big drop and that is dangerous; and that is what JB said. How do we turn that into an ABC option to make that same point?

DR. BARBIERI: In that case we would – after we decide the length of the time series and we decide it is going to be five years; we request projections at P-star 32.5 percent for five years, as five-year projections at that P-star.

DR. BERKSON: Yes, values for the P-star.

DR. BARBIERI: Well, no, we do this using reference points that are at the yield. That would be yield at 30 percent SPR as the reference point.

DR. BERKSON: That is a single run, correct?

DR. BARBIERI: Not necessarily if you are doing this – yes; I know what you mean, yes.

DR. BERKSON: That is not the three options that John asked.

DR. BARBIERI: What I'm saying is I'm starting from that plain vanilla and then adding – yes; so the next one we do, we will have short-term projections – five-year projections at P-star at 32.5 for ABC and 50 percent for OFL. This would be yield at F 30 percent SPR. Then we add to that some of the other scenarios – and I don't remember necessarily technically –

MR. CARMICHAEL: You had the question about recruitment. Recruitment is now low. If you just used the average, it pops right up to the average, so we add the sort of stepping up. I think that is something that you guys should weigh. I think that is what Jim was recalling.

DR. BERKSON: I think there are two dimensions to this. One is what is going to happen to recruitment; the low, medium, and high. Then the other is what are you going to do with the harvest; and that is how big of a bump are you going to have versus the bump and then the slide down again. If you want to avoid that steep, steep climb followed by a big slide, you don't harvest as many. I'm not wording this well; it is late in the afternoon. I apologize.

MR. CARMICHAEL: You're saying it right, but that is what we talked about you give advice to the council.

DR. BERKSON: Right.

MR. CARMICHAEL: Here is what you get from the control rule, but you can give a recommendation that they don't attempt to harvest that apparent surPlos because of the questions about recruitment, because of having to fish down the fishery; that they should consider being precautionary.

DR. BERKSON: That is not our standard ABC advice; but those are projections that I am recommending we run; that I think John said the same thing.

DR. BARBIERI: Exactly, and you did say it right. In this case we have the plain vanilla at yield at F 30 percent SPR, and then I guess the one that you're talking about is using yield at equilibrium – I mean, F at equilibrium 30 percent SPR.

DR. WALTER: Yes; would be the long-term MSY proxy; do a projection of that.

DR. BARBIERI: Do a projection at that; so we keep the P-star consistent amongst those, because those have to do with how we apply the control rule.

DR. BERKSON: Can we put up a slide of John's again or is that going to screw it up? John, do you know the slide I'm talking about.

DR. WALTER: Yes, I believe this one.

DR. BERKSON: Yes.

DR. WALTER: How to frame the long-term equilibrium MSY as some sort of a P-star advice within our control rule.

MR. CARMICHAEL: The only thing it really wouldn't be P-star in that context; it would just be general advice on reducing the risk.

DR. WALTER: I've had two thoughts along this if you want me to comment. The risk would be quite low of overfishing under that; and that could be quantified; because as you could see the risk – compared to this is the risk at 50 percent; this is the risk at 40 percent; this will be an even lower probability of overfishing. You could quantify the risk. It would not be using the control rule to choose it; so the wording of how and why it got chosen I think would be important to develop. I don't know how to do that.

DR. BERKSON: It wouldn't be offered as a base run or anything other than alternative advice. I don't think we would get into that territory.

DR. BOREMAN: Yes; and I would be reluctant to put too much scientific value on that. It is just a number, something to think about. This goes back to more of an OY issue for the council is they need to look at it; we have a possibility here for an expanding fishery, how much do we want to expand it? They've got to weigh the market, the demand, recreational demand and so on against raising the ABC too high and losing all that in the future. We can throw out several options to them to think about, but this goes into the social science side, too, I think they would need to think about.

DR. ERRIGO: Just real quickly about redoing the PSA; it was for cobia. The reason why we redid it for cobia was because the original score was done for cobia as an Atlantic-wide stock, and the stock assessment that was done during SEDAR 28 broke it into two separate stocks at the Georgia/Florida Line.

When the SSC looked at the stock, which was Georgia north; they felt that the PSA score was not reflective of the biology of that stock north of the Georgia/Florida Line; and they adjusted the PSA based on size or age of maturity or one of those factors, which changed its PSA score.

DR. BOREMAN: Now that Scott is back in the room, can I repeat my pithy remark about taking into account the social science aspect? It's in the record.

DR. BARBIERI: That did come up and it was a serious comment.

DR. BELCHER: I have a comment; it is not to the mackerel part, but relative to the PSA. To me at some point I think, too, it is the idea of did we accept the method of the MRAG approach or did we actually accept those hard, fast results of the MRAG report? There are two different things there. I think the question is, again, if we need to go back and revisit what went into that to generate the numbers that got high and medium, maybe that is more of the question we should ask ourselves. Is the approach incorrect or is it the final outcomes that we think need to be addressed?

DR. BARBIERI: My recollection is that they actually held a multiday workshop and convened a panel of experts that actually went into all of that.

DR. BELCHER: And I get that; but as we just said for cobia specifically, there were regional differences. Again, if we do something relative to hogfish, hogfish is now two stocks; so maybe in looking at that, productivity is different, vulnerability is different, so those numbers would change.

One may not be high; the other may not be moderate. Again, in thinking about that is it the approach that we bought into or was it the fact that everything was done for us, and now we're just not happy what the current final outcomes were. Maybe we need to put a little bit of our own looking in on what went into their approach to get those derivatives.

DR. BARBIERI: Right; good point and something to add to our ABC Control Rule Workshop Report and some of the recommendations and comments that we want to include there. We have

the fishing level recommendations; and that is done and that encompasses some of the options that we want to put in front of the council. Now in terms of the assessment type and the assessment timing, are there any recommendations that we feel would be appropriate or nothing specific?

DR. BOREMAN: One way to look at it is, again, looking at the suite of indices and measurements that we have in the assessment; is there anything there that if it goes off track would give us a clue that things aren't working? I don't know if it is a certain index or two that we can just track like a quasi-rumble strip approach.

That could be our recommendation; we would like to see an update in three years, but don't have to do a full update and maybe just keep us informed on certain time series of fishery-dependent or fishery-independent information that we can see if this stock is staying on track or not.

DR. BARBIERI: Yes; to my recollection we've done this in the past; we've made some of those recommendations actually like that. I don't remember which stock it was, but there was something like this where every year we are going to look at some information. Now that we have our SAFE report that we review every year, this might be something else that is added to that. It is something to think about that we would be recommending. We don't need to make that decision right now. We are going to have time to look into our report after we can have more time to think about this.

DR. CROSSON: This was a benchmark and the effort that was put into it by John and the group that was doing the Gulf assessment to really separate out that mixing zone confusion I think was wonderful. It is hard to tell at this point, but it seems like whenever it does get assessed again we could probably end up looking at an update and not another benchmark because of just how well this one was completed.

DR. BARBIERI: Yes, king mackerel hasn't had an update. It has been all benchmarks. John, we've completed our action items. It is five o'clock. Unless the committee has a different recommendation, I would say let's recess. We've got to look at what we have on the plate for tomorrow.

MR. CARMICHAEL: Tomorrow we have Regulatory 16 and bag limits; and I guess we should think about mutton snapper. Maybe there is a short status update Joe would like to fill us in on. Maybe if people could stick it out another half hour, we could wrap that up.

DR. BOREMAN: Nine o'clock is late for me; can we start at 8:30 with the mutton snapper and get that out of the way?

MR. CARMICHAEL: Can we start earlier than we advertised, Mr. Chairman? We can go later, but sometimes starting earlier can get us into trouble with the rule-makers. We could do 8:30; I just don't want to be in trouble with anyone who might have planned to come tomorrow and have us start. We have done it at the council before. We could start at 8:30, perhaps.

MR. WAUGH: The comment says here other than the starting and ending times of the meeting. That is what we've got on the agenda. I think if you want to burn some time and get some stuff done, do it now and start at 9:00 like it says here.

MR. CARMICHAEL: Everybody complained about starting so early; so let's just do mutton.

DR. BARBIERI: Let's do mutton snapper. That was a very good point, JB; but what happens is there are some complications. The language that is explicit on the agenda in terms of procedures for council meetings; so it is easier for us to go late a little bit and now see the mutton snapper and leave early tomorrow.

DR. CROSSON: I just wanted to state that when Myra gave her presentation about the right whale interaction stuff that Nick is going to talk to us about tomorrow; we asked her a few questions, and I remember her saying, well, those are good questions for Nick.

If we remember what those were, if we wrote them down, we might tell Nick some of them already before he does his presentation tomorrow. Well, no, it doesn't have to be formally right now. I'm just saying that if we have any questions for you, you probably wouldn't mind knowing them ahead of time.

DR. BARBIERI: John Walter, thank you so much. Julie Neer is also reminding me that we still need to go back and discuss the Georgia/North Carolina hogfish and how we are going to frame our catch advice for that stock.

DR. ERRIGO: The SSC needs to tell John which of the recruitment scenarios they would like him to give the projections at so that he can get us values for the ABC and the OFL and also the F 30 percent SPR. Whether it is the low, the medium, or the high recruitment scenario that he propose, I suppose you can make him do all three.

DR. SCHUELLER: He would probably do better doing all three.

DR. BARBIERI: Yes, I think that doing all three; if this is something that it would be insurmountable –

DR. BELCHER: Because you have identified the risk given each of those levels, it is up to the council to determine which of those they are happy with. Obviously, there was a higher risk if it was based on the conditional of low or high.

DR. BUCKEL: Just to add to that, to provide more information to the council, I think you mentioned that the assessment went through 2012 and now there is the information on '13 and '14 recruitment, not the age-based but the survey. The SEAMAP data would be available, right? That may help them decide which one they want to go with, low, medium, or high.

DR. WALTER: We would need the index updated because we're going to give them the – or the nominal values could be used. If that is going to go to the council as the ancillary information of what recruitment is this year and last year that the model didn't see, then we want it as up to date, if it helps their decision about which recruitment to take; I forgot what the 2013 value was with it. At some point I looked it up, but I forget; if somebody will spearhead getting that information to the council, before the presentation of the council.

DR. BARBIERI: Yes.

DR. BARBIERI: Marcel is going to be giving the presentation of the SSC meeting report, including the report on this assessment to the council, anyway. I am not going to be able to attend the December council meeting. That will give him extra motivation to have all his ducks in a row. Okay, John, thank you so much. I think we have a lot of good stuff here in terms of king mackerel. Joe, if you are ready, let's have that overview of where we are with mutton snapper assessment update.

DR. O'HOP: In keeping with the viewpoint that the stock assessment is sort of an amalgam of art and science and time is slipping away; this assessment is an update of SEDAR 15A, which is an ASAP model; so comparing the ASAP to SS3 is probably like an Audi A8 versus a Fiat 500 minus the fun and utility probably.

These are preliminary assessment findings; sorry, I don't have the report yet, it is coming. I think it will be done in November. Anyway, Bob Muller and Dustin Addis are also associated with this report. The SEDAR 15A Review Panel had a few suggestions for us. They asked us to consider alternative data streams.

They wanted us to take a hard look at the index and make sure we were using indexes that had value for the assessment. We had equally weighted them before, and maybe there is some more way of objectively weighting qualitatively how well the index is related to abundance. They wanted us to consider some alternatives to constructing our headboat and recreational indices.

We were using caught or targeting criteria. They suggested the Stevens and MacCall method might be one way of including some additional trips to look at presence/absence and get a better handle on and maybe construct a better index of abundance. They wanted us to leave out interaction terms in the commercial CPUE indices. That was probably a reasonable thing to do.

They wanted us to not split the headboat CPUE index into two series. We had done this because of changes in the size limits in the fishery. We didn't want to have one index that would cover both periods; but they suggested that we'll let the model handle the selectivity. We've done that in this particular modeling exercise.

The last one about separating age 2 and 3 fish in the FWC Visual Survey; that survey no longer exists by itself. It is now part of the NMFS/University of Miami RVC; so that one doesn't apply any more. We're using ASAP, and it is an age-based model. It requires ag- composition data as inputs. Usually we don't have a lot of age information available, so we have to in some way convert that length data that we have into ages.

They suggested that we take a look at other models that might include building age compositions from the length data internally in the model instead of applying it externally. We are still using ASAP for this update, so we haven't changed that particular method. What we have done is change the method that we actually try to apply to the length data to build those age comps.

Before we were using a bootstrapping technique; we've gone to a stochastic age-length key, which is a combination of the length frequencies that we observed from the various fleets or from the various indexes. We used the natural mortality, which is age-specific Lorenzen type, and the growth curve that we've built. They also wanted us to make sure that when we're

modeling to make sure that we use a more flexible way of specifying selectivity. Before with ASAP 1A we were limited to age-specific criteria for specifying selectivity.

With ASAP 2 and 3, we are able now to specify starting values for logistic or asymptotic selectivity or double logistic type selectivity; so that should take care of some of their concerns. What we did in the SEDAR 15A was run a whole bunch of runs with different aging techniques; age-length keys, a direct aging. We had different specified natural mortality rates; so we had a whole bunch of different scenarios that we ran.

Taking a look at how those played out gave the review panel some idea of how variable the results might be under different assumptions. They selected a run that was somewhat reasonable with the life history of mutton snapper, using an age-length key, specifying a fixed steepness at 0.75 and natural mortality of 0.11 corresponding to the maximum observed age of about 40 years. That is the one in the little square in the lower right-hand side of the plot.

That one was used to characterize and used for reference points. Perhaps things will change. With the current model, we are no longer in that happy place; we are probably to the left a little higher. We'll get to that at the end. There has been a recent study of genetics. There are known spawning areas within the Caribbean and southeastern U.S.

The spawning areas are the black triangles. The stars are where the genetics collections were taken, around Puerto Rico, the Virgin Islands, and in the Florida Keys where the Riley's Hump aggregation is. On the basis of those particular samples – the authors; it is John Gold's group – felt that there was a single demographic stock in U.S. Continental waters.

It is distinct genetically but it is not – there is still enough variation where it is not. It is demographically a distinct unit, because it is separated by distance and it seems to be isolated by the distance that larvae would have to take to travel to other areas within the Caribbean and southeastern U.S.

The U.S. Continental water stock at the moment can be treated as a single unit versus those in Puerto Rico or the Virgin Islands. They seem to be very distinct demographically, even if they are not totally distinct genetically. That doesn't sound quite right, but basically we can treat them separately as separate units.

We still don't have specimens from central and western Caribbean. We don't have other specimens in U.S. Continental waters. Right now we're operating as if the Gulf and the Florida Keys and the Southeast Atlantic are all part of one demographic unit. In planning the assessment, we wanted to update landings and index series. That is usual. We wanted to try and incorporate as many of the review panel's recommendations as we could.

Because more data was available, we wanted to make sure that we had the life history parameters correct or at least the best estimates that we can get. We wanted to take another look at how we did age-composition construction and how we constructed indexes and selectivities and how we linked those data to fleets.

We've used the latest version of ASAP. It has got a lot more flexibility and a lot more functionality, so that is a good thing, but that meant the update is not simply an exercise that

takes the new landings and the new index series and runs it. It is not as simple as that. When we took a look at our growth curve, we didn't find very much difference in what we would estimate from 15A and what we estimated in the update assessment.

There are very, very similar parameters estimated. The growth curve for 15A looked like this. It is kind of messy, a big smear of lengths at age. That is very typical. Then putting the two new growth curve for the update and one from 15A together; you will see 15A as the red dotted line. The black line would be the update assessment; very similar. Weight at age is also very similar.

Because we used the same weight at length data for converting lengths to weights, then those two curves are going to be very, very similar. We refigured age-specific survival; but because the growth curve is so similar, it gave us a very similar Lorenzen age-specific mortality or age specific survival, as I have plotted here; so very similar.

We had new maturity data. There wasn't much available for 15A. The sample sizes are shown over the curve. Up on the upper left is 15A; the update is on the lower right. You can see that we didn't have very many specimens; well, 39 it says that we were using to define the age at 50 percent or actually the length at 50 percent maturity.

We had a few more in the update assessment. It is still kind of low but it kind of works. At least we were in the right ballpark; so we were able to get new parameters there. For the update assessment, we also had more aged individuals available for the maturity analysis; and that provided new parameters.

We feel pretty safe in putting out new length at maturity and age at maturity values for the update assessment. They are not all that different, but they are a little more different on the ages. The majority of landings occur in Florida; and they occur in Southeast Florida and the Florida Keys. That is the area defined between the two orange dotted line arrows; probably somewhere in the order of – well, the majority of landings occur there. There are going to be other landings sprinkled around.

Off North Carolina below Cape Hatteras; that area is representing about 10 to 12 percent of the commercial landings now since about 1999. There is a recognition that there are more landings occurring than just in Florida, but basically about 90 percent of the landings are going to be down in Florida. Recreationally all the action is down in Florida as well.

We've used the demarcation line by that yellow area as a division, very artificial between what we're calling Atlantic and what we're calling Gulf. It doesn't mean a thing in terms of council boundaries or anything. It is just a convenience within the model. We use it to separate fleet landings by those areas. We had some information on discard rates and mortalities; immediate release mortalities that we apply, depending on whether you are on one side of the boundary or the other.

Age-composition data or length compositions were also tallied separately for those regions. If we take a look at the breakdown of landings on the upper left versus release with 15 percent release mortalities on the right; you can see the majority of landings are recreational at least in the more recent years. There is a small headboat component and a small but not negligible component from longlines.

On the releases; the releases are dominated by releases from the recreational fishery. Now there are alternatives to the MRFSS. When we talk about recreational landings – and you guys are aware that we have alternatives to the reported landings – these are ratio-adjusted landings to try and put the MRIP and MRFSS data on the same scale.

We've used two different methods to try and do that ratio adjusting. One is to use the ad hoc working group's recommendation. It is an annual by state basically. In Florida we are essentially two states, so it is annual by coast for Florida; adjustment of the landings. We've also used the Southeast Fisheries Science Center's method to make those adjustments by mode of fishing ways, coast or state, and area fished.

We've got two different scenarios of adjustments there. You probably need a little help later on to decide whether we want to use or use this information to try to specify a base run. If we array those particular landing scenarios for the recreational fishery together and put that on top of the SEDAR 15A results, you can see some differences.

We have the unadjusted landings, which are basically tracking what we used in SEDAR 15A, except in the earlier years; 1981 through 1984, it looks like. We have a few differences there. We also have differences especially in the early years on whether we use the Southeast Fisheries Science Center method or the Ad Hoc Working Group method. Of course, the later period, 2004 to 2013, is all from the MRIP, so there is no adjustment for those years.

Commercial harvest, these show the input stream we used for SEDAR 15A and the input stream we were using for the update assessment. They are largely similar. We have some difficulty in defining what a vertical line, hook-and-line type of fishery was before gears were required to be reported on trip tickets or whatever else we're using for defining commercial harvest.

That has caused a little bit of shifting in the input streams for the hook-and-line fishery. The same with longline; if it wasn't clear to us whether it was a longline or not, it got cut into one pile or the other. We tried to use the information on a fisherman's saltwater product license application to define whether they are longline fishermen or not.

Anyway, you can see there are some differences, but in the later years they pretty much track each other; so that may not be a huge problem. With the headboat harvest, we've also got some differences. We're adding in discards along with the landings Plos we're throwing in animals that we feel would be – and this is based on at-sea operations of releases of fish that we believe would die to immediate release mortality. We add that into the dead discards from headboats.

There is not a whole lot of data prior to 2004 directly from the headboat survey, so we are trying to apply the observations we have from the at-sea work to hindcast what would be discards and immediate release mortality. The same way with the recreational harvest from the MRFSS survey, we are trying to hindcast what those discards would be.

Almost all the B2 fish reported – well, actually if you look at the B1 fish, which is supposed to be fish that the interviewer didn't see but was reported by the angler to have been caught; it could have been discarded dead; it could also have been cut up for bait or things like that. Well, if you look at those codes that are used for classifying the B1 fish, practically none were discarded dead.

What you are seeing basically is all the fish that were classified as B2s, we've added to that a little bit of immediate release mortality. Those are probably why you're seeing the differences between the input streams. The sources of discard data; we had the Coastal Fisheries Logbook Program from Kevin McCarthy. That is pretty standard for SEDARS.

We have information from the MRFSS survey and MRIP. The Southeast Headboat Survey has some information since 2004, and then we have samplers on headboats that provide us with some coverage of headboat trips. There is some difference in the immediate release mortalities that we observe on those trips. There is about 6 or 7 percent in the Keys and about 14 percent from the Southeast Florida area.

It is probably due to the depth at which the fish are caught on those headboat trips. It is probably shallower in the Keys and much deeper in Southeast Florida. Here is how we have constructed the discard stream for SEDAR 15A for hook and line, for headboats, and recreational. Of course, there are no discards from the longline fishery. They are all considered dead when they are brought up. These are reported in logbooks, and they are not anything where we have observers on board.

The ASAP 3 model, we used ages 1 to 25 Plos, just as we did in SEDAR 15A. We took the length compositions from the fleets, the year, and the region, trying and come up with those age compositions. We're using age-specific mortalities averaged over ages 3 to 40. We've defined four fleets, which is a little change from what we had in SEDAR 15A.

In 15A we had five fleets, commercial hook and line, longline; and we had another category which is sort of a catchall for fishtraps and actually lobster traps and any other trap where it was legal for fish to be retained, and spearfishing and other kinds of gear that might have been used or trips that were unknown.

We tried to apportion the unknown trips into the other gear categories, if we could. Headboat and recreational ran without the other two gears. We took commercial landings' data as usual, from the general canvass, from our trip tickets. The MRFSS or MRIP and Southeast Headboat Surveys supplied the recreational data; biological samples from TIP, from FWC, from North Carolina DMF. Discard lengths were from the at-sea sampling on headboats.

Southeast Florida we had a fairly long run of data, 2005 to '14. From the Keys we had a little hiatus between 2008 and 2009, but it still provided us with a fair number of observations that we could try and get a handle on what the rate of discards were and the fish that floated away rather than swimming down was. Basically that covers that.

Indices; the indices were the coastal fisheries logbook; it is a Stevens and MacCall method. MRFSS, we changed from a targeted or caught kind of criteria for building these subsets to cluster analysis, headboat the same way. For the fishery-independent indices, we built them from the NMFS RVC data, which is a proportional random sampling underwater visual survey. They also provide length frequencies, which we then took our age-length key and converted to an age composition.

The FWC FIN index, which is an age zero index, they are using 183 meter haul seines, and we get length information from those haul sets. They are basically set in the Indian River Lagoon,

more southern Indian River Lagoon. Our last index was from Riley's Hump. It is Mike Burton's study of some of the spawning aggregations. It is a diver-based survey. We don't get length information from it, but we got the average number seen per dive.

The model includes steepness as one of the estimated parameters. Generally we turn it loose and let the model try and estimate it. We've also fixed it at certain values. It seems to be centering on – the objective function seems to have a low point around where the model if it was turned loose would estimate. We feel that is a fairly good indication that it is not the starting value that is affecting where the model is estimating steepness.

The catch abilities are constant; and the starting values for selectivities, we used an asymptotic selectivity for hook and line and longline. We've linked those particular indexes to the fleet for the age compositions. The double logistic we specified for headboat and recreational data based on what we observe in your catches. We've also linked those particular indexes to the fleets.

This is a little bit of change for the recreational index that we used before. Before we had used a total catch index developed from the MRFSS data to run against the total population that ASAP would construct during a model run. Now we've linked is specifically to the MRFSS recreational or MRIP recreational fleet. That has represented a big change in the model structure, I think.

These are the index series from SEDAR 15A; and the update, again same thing. The red dotted line is SEDAR 15A; the black line is the update. The indexes are fairly similar, small departures, and that represents a change in how Kevin has constructed which trips he pulls in from the logbooks.

It kind of jockeys around those estimates for the index a little bit, but they are very similar to what he used before. The headboat index; you can see the two series we have on the left between '81 and about '91 is the first index series we used. The second index series was from 1995 up to 2006.

But if you scale them each to their means and compare them to the index that we've constructed for this one, they are fairly similar, a little bit of departure in 2005, it looks like. For the MRFSS/MRIP index, the cluster index is a lot flatter than the SEDAR 15A index. There is some change based on whether you use the targeting/caught criteria versus a cluster subset approach.

The index of abundance; basically there are some changes in the index values both for the age zero index and the RVC. The RVC is probably – in 15A the index was based on exploited sizes of fish. That may explain why there is such a large departure in those two index series. Riley's Hump, we had some differences. I am not sure if that is just because we added more data and we get different solutions for the standardization.

I am not really positive why that is, but we also had one more year. We had 2001 in the SEDAR 15A index. We don't have it in this one and I'm not sure why, but that is what we have. Putting them all together, you can see how the indexes are behaving in the model. Recreational and headboat fleets; they are tagged to the same ages, between two and eight. The longline fleet is ages four to twenty, and the hook-and-line fleet is between ages two and nine. Those are how we're relating the indexes to the age comps and how we're assessing the model fit or how we're

letting ASAP add those fits into the objective function. The fishery-independent indexes; we used that FIN index for age zero, but actually we were pushing them to age one.

These haul seines were made in the estuary where the young mutton snapper are settling out. By the time they are getting toward age one, they are going to be moving out of the estuary, so we get very few age ones. It is probably 90 and 95 percent age zeros in that particular sampling survey. We're pushing that age zero to give us an idea of what age ones might be.

That is basically in the southeastern section of this particular area. Now how well that assesses age one population is something that is probably open to debate. With the NMFS RVC, we're using a double logistic function to model how well the length frequencies convert to ages and how that indexes to the population. It goes between ages one and 12.

By age 12 it was down below 1 percent, so they are not seeing very – at least the way we've aged them, they are not seeing very old fish there, but they do see some fish that are in the 500 and 600 millimeter range; so they are seeing some large fish there, but mostly they are seeing young fish that we estimate to be in the age one, age two, and age three. That is what our age-length key would translate their length compositions to.

For Riley's Hump, there is no length data so we can't apply an age-length key to them; but we've asked Mike what he believes the ages of fish he's seen. He has made a guess at between ages five and fifteen would be reasonable for animals that he encounters there. We've included that age range for the models he used in assessing fits.

Here is what the model estimates for our selectivity at age for fleets and for the fishery-independent indices. The fleets pretty much go along with what we've put in as starting values. There is a little bit of shifting around, but basically they are not too bad. For the RVC; that is the only one that is markedly different than what we have put in as starting values.

We would not have them be quite as – we don't see animals or we don't convert those length frequencies that come from the RVC to quite that pattern of selectivity that you see here. Otherwise, it is not doing badly and actually the model thinks it fits pretty well. Here are the model fits landings by fleet. The residuals are not terribly bad.

Sometimes you get some that are high and over three, but by in large the model is trying to fit those landings pretty closely. The discards by fleet; discards are always tough because we don't actually have a lot of good information on them; but the model fits aren't terribly bad for those either, but you do get some departures.

Again, for all of the fleets they are not doing too badly as far as estimation goes. The indices also fit relatively well. There is some patterning. We would like to get more random pattern, around zero, for the residuals, but they are not terribly out of whack. There are some large residuals sometimes for the headboat and recreational fleets, but we kind of expected that, anyway, because we have so little information on discards.

For the indices, we were surprised at how well the indices were being fit by the model; but I guess that is a big of good luck. The Riley's Hump Visual Survey has fairly large residuals for the first year, but the model thinks it is not terribly bad. It really likes the RVC survey. Average

F; the first fully selected age is age three. It seems young, but this includes both the landings and the discards; and that is where that tends to move that fully selected age down a little bit.

We got a reasonably good stock-recruitment relationship. Our benchmarks are F 30 percent SPR. That is how it has been managed in the past. We actually have a stock-recruit relationship that possibly could be used. We used MCMC. We haven't done likelihood profiles, but I will try and get some of those done. We've been using MCMC instead.

We have gone to looking at multiple chains. Because Markoff chain Monte Carlos, you start with a value and you continue on from that, even though you are adding random variation, you've got a starting value that you are continuing on in the chain. The advice that we're seeing from folks like Geoman, he is saying instead of running millions and millions of samples, to go ahead and run samples from different starting points to build different chains; apply your burn in separately and auto-correlation; assess all that separately, then you can put those chains together as different samples, and that should give you a little more representation of the simulated data.

When we do this, this is an amalgam of four chains that already had burn in and auto-correlation removed. This is our trace plot on the top; that fuzzy caterpillar kind of thing. You want that kind of fuzziness there with no particular patterning in it. The QQ plot looks pretty good, and the resulting frequented distribution isn't at that, and this is for steepness.

For F geometric, again not too bad; and SSB 2013, I like it actually. I just wanted to point out that for F geometric – and we've looked at the retrospectives, which are coming up in the next slide, but you will see that the F 2013 and SSB 2013, they tend to be a little different. And the last year, which is what you guys are familiar with, you tend to get some changes when you add the next year's data into it.

The F 2013 tends to be a little higher, and you will see this from the retrospective, about 7 percent higher in the terminal year; then if you add another year of data, it tends to reduce some. SSB tends to be a little lower, but that comes up about 3 or 4 percent. Here are the retrospectives.

This is on the preliminary base run, which is an unadjusted MRFSS landing series. You see a little bit of difference in those terminal years. Those are where the landing series ends. You can see for the fishing mortality in the upper left they tend to be a little overstated. The moons rose – I see it is 5 percent on here. For SSB, which is on the upper right; that tends to be about 7 percent understated; so when you add another year, it comes up a little bit.

For recruitment, it tends to be about 10 percent too low, so it should come up – actually 10 percent high; they go backwards; so it looks like it is 10 percent low; and when you add the next year, it should come up a little bit. In a way if you are going to have a retrospective pattern, this is probably a better one to have.

These are some of the model runs for F and SPR. In the upper part of SEDAR 15A, you can see the solutions for F at 30 percent SPR is about 0.34. If we look at those for the update assessment, it is about half that; so at 0.17. For sensitivity runs, we have runs where we have the two types of ratio adjusters for the MRIP/MRFSS data series. We have one next to the one above the bottom.

If release mortality is higher than 15 percent, which is what we are using for the base run, and then we have one where what if we're wrong with natural mortality, if instead it is around 50 years; what would the benchmark values look like? For each of the runs I have the Fmsy solution where the model was free to estimate steepness.

Those are fairly similar for the base and the sensitivity runs; a lot higher in SEDAR 15A. There are some differences there for Fs. The SPR is also estimated a little high, we think. In SEDAR 15A they are a lot lower in the update and the sensitivities. For spawning stock biomass, we used a total biomass instead of female biomass.

Those values are about twice as high as what we're using in the update assessment. You can see the SSB at 30 percent SPR was 3,148 metric tons; for the SEDAR 15A it is 2,234 in our so-called base model. We are estimating that SSB at the benchmark to be a lot lower than what we estimated in 15A.

Now, if you use the landings series from the ratio adjusters, the SSB comes up a little bit. If you include higher release mortality, which these are the long-term release mortality where we don't have any data; the SSB adjusts upward. If you think we've understated the overall natural mortality value too high; so if you lower it, then SSB has to come up to support this level of mortality that it sees in its population.

The yield values are on the right. The yield at 30 percent SPR in the old model is about 688 metric tons. For the update model, we're estimating about 391 metric tons. If we use the yields from the adjusted recreational runs; that number comes up to about 440 metric tons, somewhere in that range.

That is all I have for you. I guess I need a little bit of guidance on what we should use for a base model. Should we be using – and the rationale for using the unadjusted MRFSS data was that it was most like what we had used for SEDAR 15A, so we had a way of comparing what we had in SEDAR 15A with the modified update assessment that we're using now; but we should probably be using the ratio-adjusted landings in order to calculate what the yield should be. It is really sort of a judgment call that I am hoping you guys will help with; you know, what should we designate as a reference base run and what should we have as sensitivities? Do we need to have other kinds of runs in here that you would like to see? Thank you.

DR. BARBIERI: Thank you, Joe; that was a very thorough update. With that, I am going to open for questions since Joe is here and see if any of the committee members would have any specific questions for Joe. Of course, since you are not going to be seeing the report and perhaps an adjusted assessment with final results, there is always the opportunity between now and the time that we put together our report that perhaps you can send us that presentation, if you haven't yet, for the committee members to take a look and then provide us some input and suggestions and ask for specific things.

I didn't want to completely cut everybody out of the opportunity to ask questions if there are any. Thank you, Joe, I appreciate it, and I think we are right on time. With that, let's recess for the day and we reconvene at 0900 tomorrow morning.

(Whereupon, the meeting was recessed at 5:55 o'clock p.m., October 29, 2014.)

The Science and Statistical Committee of the South Atlantic Fishery Management Council reconvened in the Crowne Plaza Hotel, North Charleston, South Carolina, Thursday morning, October 30, 2014, and was called to order at 9:00 o'clock a.m. by Chairman Luiz Barbieri.

DR. BARBIERI: Welcome back to the final day, day three of our October 2014 SSC meeting. We are going to have a number of fairly intense presentations and discussions today. I believe Nick's presentation is going to be the first one in order; a discussion of Agenda Item 10, Snapper Grouper Regulatory Amendment 16.

Of course, you may remember that we had an introductory presentation by Myra a couple days ago that gave us sort of the council and regulatory amendment perspective on this. We were looking forward to having Nick come in and give us some of the technical presentations on this issue.

DR. FARMER: I am going to run through an analysis that was put together, a pretty big collaborative effort between the Regional Office's Sustainable Fisheries Division and then our Protected Resources Division, ESA Sea Turtle and Fisheries, and Marine Mammal Branches. We also had substantial input from FWC.

I need to recognize Jessica Powell, Barb Zoodsma, Tim Gowan from FWC and some of his colleagues. Lance Garrison from the SEFSC also provided some data for us; and Andy Herndon and Andy Strelcheck and the many people down at the Southeast Fisheries Science Center provided substantial review on this.

This has gone through what we termed the full and comprehensive scientific review form the Science Center, and we have attempted to address their many comments in this presentation. We think that we've handled those, and we will send a response to them I assume at some point. Otherwise, this will just be incorporated in the amendment and get certified through them that way.

Regulatory Amendment 16, as you guys know from the introduction from Myra, is considering removing or shortening the black sea bass trap season closure, which is currently closed from November through April; or spatially designating some closure boundaries to give fishermen an opportunity to set trap gear during that November to April time period. The goal is to increase socio-economic benefits to those trap endorsement holders while maintaining protections for ESA-listed whales.

The species of concern here is the North Atlantic right whale as well as some other ESA-listed whales. The South Atlantic contains the sole known calving area for the endangered North Atlantic right whale. There is something like 500 individuals, possibly less, of this species, so it is a highly endangered species.

The concentrations are the highest in the core calving area, which is between Florida and North Carolina, from November 15 to April 15, but the whales move throughout that wintering habitat, and between the southeast and the northeast during a November to April timeframe, so November 1 through April 30. Entanglement in fishing gear is a leading cause of human-induced mortality. Your main causes are basically vessels strikes and entanglements.

Typically when scientists observe an entangled whale, the entanglement can't be dialed down to what fishing gear it's from and you can't really assign it to a particular fishery or location. However, there was a review that was done in 2005; and the references in the report that I provided to you guys. But basically when the fishing gear could be identified, it was typically either pot gear or gillnet; and pot gear represented 71 percent of the entanglements when the gear was identifiable.

Looking at black sea bass management; we're in a pretty dynamic time period with this fishery. Basically, starting out in the late nineties, you had the fishery kind of building, the stock was getting progressively fished down, and then you see some recovery going on. That was leading to what we would say would be a derby condition.

There was initially a quota closure in the commercial fishery on May 15, 2009; and recall during this time period the fishery ran from June through May; so that was right towards the end of the '09 season. The following season I think people caught on and there was a quota closure the previous year. The fishery really accelerated in terms of its harvest, and this graphic here is landings in pounds gutted on the Y axis and the fishing season on the X.

Then the colors denote the different months, so the proportion of the landings coming from each month; and you can see that the later months begin dropping out starting in the 2009-10 season. You get this quota closure on December 20, 2009, which I remember that actually being announced right around the time of the December council meeting in '09, which was an interesting meeting in North Carolina. Then in 2010-11 it closed even earlier; that was in October. Then in 2011-2012, it closed in July, so it really only lasted 45 days. It was a very short season.

The council then enacted Plan Amendment 18A. That instituted a trap endorsement program, which cut the number of participants in the fishery substantially based on catch history. You couldn't use trap gear if you did not have a trap gear endorsement along with your snapper grouper permit. It also implemented a 35 trap limit, a 1,000 pound trip limit.

It increased the black sea bass commercial size limit and put in a requirement that they needed to bring their traps to shore at the end of each trip rather than leaving them out in the water. That was followed by a quota closure in October of 2012; and then Regulatory Amendment 19 was implemented, which over doubled the ACL for the commercial fishery.

Because there was a concern that trap gear had not been in the water since December of 2009 and there was a concern with interactions with whales; they also implemented that trap gear closure from November to April. Then Regulatory Amendment 14 will be implemented in this upcoming season.

One of the interesting things that does is it switches the fishery from a June start date to a January start date. Obviously, one of the concerns of Regulatory Amendment 19 was that you were going to have trap gear potentially in the water, because the season would last longer than the previous quota closures that allowed it.

What we were having before was a quota closure prior to the November time period with that June start date; but now you are going to be starting in January, so you've automatically got

several months during that November to April right whale season. Regulatory Amendment 14 will have a January start date, and it also puts in some trip limits for the vertical line at the beginning of the season to attempt to slow the harvest with vertical lines so that the trap guys have an opportunity to fish after that April closure ends.

I think Myra walked you guys through the alternatives for Regulatory Amendment 16; so unless anyone really wants a refresher on those – and I know there are a lot of them and they can be kind of complicated – we can kind of blow past the alternatives and maybe revisit them in the interest of time.

Unless I hear any opposition, what we will do is if you guys have a question about a specific alternative or why it was proposed by the council, let's come back to that; but then we'll just skip right past those. The objectives for the analysis that we performed was to simulate the potential landings of the black sea bass pot endorsement holders under each proposed alternative and then to factor in those landings from other gears and predict when the ACL would be met.

We also wanted to consider the seasonal distribution of where they are fishing for black sea bass with the trap gear and also the seasonal distribution of North Atlantic right whales to compare the relative risk of right whale entanglement under each of the proposed alternatives. Our data sources were as follows.

On the left there you can see we used the Southeast Fisheries Science Center's Commercial Logbook, which is a self-reported logbook from commercial fishermen that reports trip level landings by species, gear, area; and since 2004, depth. We used that data from 1998 through 2014. We limited the logbook dataset to endorsement holders only. We simulated a 1,000 pound trip limit for them and we simulated a 35 trap limit. That kind of handled the trap gear kind of baseline data.

Then for the other gears, which are becoming more and more important in this fishery, we used the Southeast Fishery Science Center's Commercial ACL Data, which are aggregated dealer records of catch by gear and species. That includes landings from vessels both with and without federal permits.

The Science Center's commercial logbook only has federally permitted vessels, and that would include all the pot gear endorsement holders. But there is a substantial amount, and it is increasing through time, of landings coming from folks who don't have federal permits that are fishing for black sea bass in state waters.

As the stock rebuilds, you would expect that will become more and more prevalent. We used that data from the commercial ACL dataset from 2002 through 2013. We also used the ACCSP trip ticket data because the commercial ACL data only goes through 2013. We used the ACCSP trip ticket data for the 2013 through 2014 season; so June 2013 on.

That has got a simulated dealer trip tickets in there, and they provide catch by gear and by species. That includes landings, again, with vessels with and without the federal permits. What that allowed us to do was we could simulate the 300 pound trip limit that will be implemented for these hook-and-line guys by Regulatory Amendment 14 through January through April; and

then we also were able to simulate the thousand pound trip limit that will be implemented for them from May to December. Those kind of created our baseline for data sources.

Now, obviously, there hasn't been any trap gear fishing since December of 2009 for this November through April time period. There is a lot of uncertainty given the underlying dynamics of a massive ACL increase, a pot gear endorsement, a lot of changes to what the pot gear guys are allowed to do with their pots, how many traps they can have out on the water and various other things.

There is a lot of uncertainty in terms of both where they are going to fish, if they were able to fish in the winter, and how they are going to fish and what their catch rates are going to be; and the stock is rebuilding underneath all of that. We felt that we needed to really have a lot of sensitivity runs in this analysis. Some of them we developed ourselves and some of them are in response to comments from the Science Center.

We have three methods for dealing with the potential spatial distribution of fishing during a possible trap gear winter season. Scenario A is based on the spatial distribution of trap gear endorsement holder landings under simulated 18A regulations; so that is with the 35 trap limit and the 1,000 pound trip limit from November through May, based on data from 2008 through 2009, so that 2008/09 season, which is the last season where the November through April time period was fully open, because they didn't close with a quota closure until May of '09.

Scenario B is based on the spatial distribution of trap gear endorsement holder landings from this most recent season that we have data for, which is the '13 and '14 season. Now they didn't fish in the wintertime there, so what we're using there is we're using the October catch rate from '13 and '14 – sorry, we're using the mean distribution of where the landings occurred during that entire June through October '13 and '14 time period. This is where people are fishing.

That would basically account for any changes that may have happened since the last winter season with regards to where these endorsement holders are fishing, who still has an endorsement, have they moved their boats to different locations? That is going to address some of that stuff, but what it wouldn't address is are there any onshore/offshore dynamics between a winter and a summer season?

Scenario C was based on the spatial distribution of the trap gear endorsement holder landings under those simulated 18A conditions based on a mean of data from November through May, 2006/07 season through the '08, and '09 season. This was a scenario that was suggested by the Science Center.

The main reason they suggested that is they felt that using just the winter season from 2008/09 could potentially lead you down the wrong path, because there was an economic crash in '08; and so they felt that if we took a little bit longer time slice, we might mitigate some of those impacts for the economic crash in '08.

Soak time is an important variable when you are considering the risk of right whale entanglement, because the main source of risk here that we're talking about is that vertical line in the water from the trap on the bottom leading up to the buoy on the surface. How long that line is in the water is kind of a proxy for risk.

That was only reliably reported for the 2013/14 season. It has been reported prior to that; but the problem is that a lot of the fishermen don't understand and there are some inconsistencies in the way they interpret how they are supposed to fill out the soak time field. Some people fill it out as the total soak time on the trip; some people fill it out as the mean soak time for a trap.

Some people fill it out as the mean soak time for a set across all their traps, but they make multiple sets. It gets very confusing. We were able to use – the Science Center actually went back into the data into the raw logbook records and provided us kind of a rectified 2013/14 soak time dataset.

We were able to use that to compute what the soak time is. Actually, I think that this is a reasonable thing to apply to the past, because the new regulations are such that past soak times are probably not representative, even if they had been reliably reported, of what current soak times would be in the fishery, and that is because you have these requirements.

You have a 35 trap limit which you didn't have before; so you are probably working with fewer traps, so you could soak for less time or more time depending on that. You have to bring them into shore afterwards, so you are not going to have these really long multiday soaks. There have been a lot of changes there, so I think this is reasonable.

The way that we assigned it to the historical data is in a tiered approach. If we had a soak time tied to a vessel ID for this most recent season, we would apply that soak time to that same vessel ID in the previous season. If we didn't have the vessel ID, then we would try to tie it to the permit owner. If we didn't have the permit owner, then we would try to tie it to the same area, month, and depth.

If we didn't have that, we would assign it to the same area; and then if we didn't have that, we would assign it across the entire coast. It was a tiered approach of backfilling the soak time. Then you multiply the soak time times the number of traps to get your total soak hours. We looked at four different sensitivity runs then for catch rate, because it is important to consider catch rate when evaluating these alternatives because you want to know the tradeoff between the area that is closed, which is going to cover the spatial distribution of the catch; but also the catch rate, because you could hit a quota closure prior to the end of the season.

So all alternatives are not created equal with regards to their opportunities for guys to go fishing and catch fish at a high rate. We used four different catch rates, the first one was based on the 2008/09 catch rates from November through April under Snapper Grouper 18A regulations. The second was based on a seasonal pattern from 2008/09 under those regulations, but then scaled up by the 2013/14 catch rates.

That basically accounted for the fact that you have this kind of seasonal dynamic in 2008/09 where fishing was higher in the wintertime. The catch rates were higher in the wintertime than they were in the summertime.

But when you look at those high catch rates in the wintertime, in that 2008/09 season, they actually aren't as high as the summertime catch rates in 2013/14, because the stocks recovered underneath. This approach basically scaled up the 2013/14 summertime catch rates using the same ratio as you saw in the winter of 2008/09; so you take that October basically catch rate and

you scale it up to the ratio of October 2008/09 to November 2008/09, October of 2008/09 to December 2008/09 and so on.

The third scenario here we used just the October 2013-14 catch rates and said that catch rate will be constant through the winter. That was a way of handling just a constant scenario let's say that catch is consistent from 2013/14 for the rest of the year. The fourth scenario we looked at the mean of 2006/07 through 2008/09 catch rates under those 18A regulations. That was also to address the Science Center concern about the economic crash in '08.

In terms of catch rates for other gears or months outside of that November through April winter season, we assumed those were equal to the observed 2013/14 catch rates, with the Regulatory Amendment 14 regulations applied. The catch rates for the vertical line had that 300 pound trip limit applied to them for January through April and then the 1,000 pound trip limit after that.

Then we needed to look at right whale distribution as the next kind of sensitivity scenario under this. There are two different models that we were able to use. The first one that I will focus on is the one here at the bottom; that is the Gowan and Ortega-Ortiz2014 model. This was a model that was published in Plos One earlier this year.

This model basically takes the Southeast U.S. North Atlantic Right Whale Surveys and models them as basically a predicted distribution of sighting probabilities on a semi-monthly basis. There is a fair amount of data in there. It comes from the 2003/04 aerial survey season all the way through the 2012-2013 aerial survey season. The model inputs were sea surface temperature, depth, distance to shore, distance to the 22 degree Celsius isotherm, and some interaction terms.

We used three different scenarios for this Florida through South Carolina area. The first was a mean which assumed that the temperature conditions basically would be the mean of the temperature conditions observed across all those aerial surveys from '03 through 2013. The second was a warm scenario; and that used the data from the warmest year in the time series, which was 2011/2012.

The third was a cold scenario, which used data from the observed time series in '09 through 2010. Those last two, the warm and cold scenarios, were things that we had talked about in our initial document, but the Science Center thought it would be good to go ahead and simulate those explicitly; and so we did add that in response to their comments.

Off of North Carolina, there is less data available and so that couldn't be modeled in the same way. The Gowan and Ortega-Ortiz2014 data from Florida to South Carolina, we got semi-monthly model outputs, which were then averaged into a single monthly average so we would have a November average, December average and so on.

The North Carolina data, we only have data from '05 through '08, so Tim Gowan from FWC used the same modeling method and developed, based on the UNCW aerial surveys, a long-term average, so a November through April average of where right whales are distributed off of North Carolina. His inputs for that model included sea surface temperature, depth, distance to shore, and bathymetric slope.

I should stress it was important to deal with the right whale distribution in this way; because if you based your conclusions exclusively off of sightings data, obviously you would be biased by the sightings effort. The more you look, the more you are likely to see. These models adjust for that issue.

I have a little animation that pops up. The model outputs between North Carolina and the Florida to South Carolina models are not directly comparable. Because they are modeled slightly differently, their regression outputs are not comparable. One might be on a scale of thousands; one might be on a scale of ten thousands, or whatever; and it is simply because the inputs are different.

If you could somehow merge these two models, then you could kind of make a combined model; but Doctor Gowan did now feel that was going to work given the input data that was available. These models are treated differently; and the way that we present risk is going to be presented as a Florida through South Carolina risk and then a North Carolina risk.

You can't really compare between the two of them, so you have to equilibrate in your mind I guess those impacts. In order to do the analysis, basically we did a daily accounting based on monthly models. We have monthly models of black sea bass traps, and that is landings and effort.

We have monthly models of the whale distribution from Florida to South Carolina and then a November through April model for North Carolina. You overlay those on top of each other, and you get a fishery weighted risk to your North Atlantic right whale. Obviously, you might have a lot of whales in an area; but if there is no fishing in an area, then your risk is zero. If you have a lot of whales in an area and a lot of fishing pressure in an area, then you are going to have high risk in that area.

This doesn't explicitly assume an entanglement rate. It simply just assumes that the amount of risk to right whales is proportional to the overlay of the right whale probability of being there and the trap pot gear probability of being there.

(Question asked off the record)

DR. FARMER: We don't have an explicit-like knowledge of what the entanglement rate is for a right whale. We don't know that a right whale swimming through the water encounters a trap at, you know, five trap hours equals a risk term of five percent. We don't have that kind of data. What you are doing here is you are just treating things in a relative way.

You are assuming that all things being equal, a right whale moving through the water is more likely – let's say that there is ten trap hours here and five trap hours here; well, if the right whales are equally distributed in those two areas, then the risk in the place with five trap hours is half that of the place with ten trap hours. Does that make sense?

We just assume that the entanglement risk, which is unknown, is uniform throughout. I will get to some discussion of that and how right whale behavior might impact that term. Anyway, you've got this fishery weighted risk to right whales as a daily term. Then you can compute the cumulative risk over the course of the winter fishing season until a quota closure is reached.

DR. BERKSON: We can talk about this later if you want, but I wanted to bring this up. Going back to the Gowan models; how well do they fit? You could look at incidence of the right whales from the aerial surveys and you can put together a model using environmental factors. The model might not be worth a darn or it might be dead on. It just depends on what variables you pick and there has got to be some sort of sense you can give us as to how much confidence we have in those relationships.

DR. FARMER: Dr. Gowan would obviously be much more equipped to address those model fits than myself; but if you look at the Plos One paper, which is publically available on line, the model fits for that paper are pretty good especially for right whale data. It is a pretty good model fit.

Then the North Carolina model is actually the appendix contains the discussion of the model fits and the residual diagnostic plots; and that is a reasonable model fit. Certainly the Florida through South Carolina model has a better model fit, because it is based on much more data; but the model fits are pretty good.

DR. BERKSON: Was there any validation that was done; did they pull out any data?

DR. FARMER: Yes. I talked with Tim, and he actually gave a presentation at the Regional Office. He went through all the classic residual diagnostics, cross-validation approaches. He even did some external validation work where they predicted where right whales would be and then tried to match that to their survey effort and looked at what they observed in the following year.

It seemed to correspond pretty well with the model predictions of the right whale distribution. They are actually in some cases now using this model to help stratify their survey sampling design because it worked so well.

DR. BARBIERI: Good questions, Jim. A couple points; one regarding your question – and I think this might help – in terms of how much at least internal review, of course, this is not a fisheries model; but because we have that core of stock assessment scientists, we took a few days to basically sit down with Tim and review all of this before it was finalized.

There were some discussions and suggestions and modifications in looking at the diagnostics and all of that before I went out. I don't have all the details or any, for that matter; but just for you – because that is a very good point. For us without reading the actual paper, you don't know.

DR. FARMER: The paper that is referenced on the previous slide is open access. You see the reference there at the bottom. If you are online right now, you can search Gowan and Ortega-Ortiz 2014 Plos, and it will pop right up. It's got all the figures, plot fits, et cetera.

DR. ERRIGO: I just sent it to everyone. I figured after the question, we probably would want to see it, so I did it for you.

DR. FARMER: You might want to also look at the online version as well, though, because my experience with Plos is when you wrap the PDF sometimes you don't get the supplemental

material; so if there are residual diagnostics and stuff, which I am not sure if there are, then you'll get the higher resolution figures and stuff like that. Anyway, it is available so have a look. When you get to the last step here on this slide, the daily accounting, then you get a cumulative total risk and you get cumulative landings until the quota closure is hit.

Basically what we do then is because these model outputs are just unitless scalars at this point, we relativize the comparisons; and so to relativize the comparisons, remember Alternative 1 for Regulatory Amendment 16 has no pot gear in the water November through April. That is the status quo alternative. That has zero percent risk; we define it as zero percent.

Alternative 2 is the full opening to the trap gear. We define that within each combination of scenarios; so remember we had the three spatial distributions, we have the four catch rates, and then we have the three right whale distributions; so within each scenario, the value that comes out for Alternative 2 is then defined as 100 percent risk.

Then you make all your comparisons in terms of the total unitless scalar that has accumulated over the fishing season relative to the total that comes from Alternative 2; and so that cancels out a lot of the concerns that come with, well, how do we quantify it because we don't have entanglement rate and that sort of thing?

This just gives you a relative look at the efficacy of each of the closed area scenarios. It is important to consider it this way, because a closed area that slows the catch rate of the fishery might actually result in more risk than another one that results in an earlier closure. If you close early, maybe you cancel out a couple of months of having gear in the water when right whales are in the water; whereas, if you're open later, maybe you have more days with right whales in the water; so let's say that your risk is 10 under one scenario and it is 50 under another.

Well, if you have more days of 10 that sum to a number higher than that; you could get kind of an inverted scenario. It is important to look at not only where the risk is but also how long it endures. Moving into the results – and I've got a lot of them to present here – the first would be the spatial distribution of landings and effort.

We look at figure one here and it just shows you the reported depth of fishing by sea bass trap gear endorsement holders for the most recent two seasons. This is important to look at because almost all the alternatives in Regulatory Amendment 16 are depth-based alternatives. These external or offshore boundaries for the closed areas are based on a generalized version of some bathymetric contour, 25 meters or 30 meters and so on. This is a figure and you can see the bulk of the fishing happens between 20 and 30 meters depth.

The second figure here is the box plot of captain-reported depths of fishing for South Carolina black sea bass trap gear endorsement holders by fishing year. You can see fishing year running there along the bottom from 2004/05 through the 2009-2010 season. I've broken it out into seasons within those fishing years, so you have your fall/spring, your summer and then your winter.

We're looking at the reported depth of fishing to see is there an inshore/offshore dynamic for the winter fishery. I've heard a lot of statements from the fishermen that the black sea bass come inshore in the wintertime, we fish closer to shore; and it kind of works out; both because the

weather conditions are rougher, so they don't want to go offshore, anyway. The black sea bass pot gear seems to fish effectively in the wintertime. The fish pot up more when it is cold, and they draw a higher price because they look differently and maybe they look a little bit healthier or whatever and so they get a little bit higher price.

I will say that one of our goals with these sensitivity runs was try to address this inshore/offshore dynamic of fishing. Looking at the South Carolina plot here; this is the one where we saw the strongest signal of an inshore/offshore dynamic in terms of they fish deeper in the summertime and shallower in the winter; but it is not a significant difference as you look at the box plot.

Whether that is a case of there is a little bit more but it is not a significant difference of inshore/offshore fishing or whether it is just that this is captain-reported depth and they tend to report kind of the same depth when they go out on a trip, and that is just their depth that they fished at; and maybe they aren't reporting depth specifically; that could be it as well.

Another issue is that the depths, of course, and the areas fished are reported on the trip level and not a set level. It could be that they made a set at 15, a set at 20, and a set at 25, and they just called it 20. Maybe that is kind of what they do and maybe there were some differences there but they are not being captured by the data.

I am not sure what is going on there, but I think that those dynamics are captured pretty well by the wide range of sensitivity runs that we did. I am not expecting you guys to be able to actually interpret this graphic. It is in the report and this is Figure 3A through F. This is the spatial distribution of landings, which is on the left side of each of the little figures, and effort which is on the right side of each of the little figures by month for the winter 2008/09 season.

This is kind of our Scenario A. I just put all the figures up there to stress that we have data under this scenario for each of the winter months. Here is a zoomed-in version of Figure 3F for April of 2008/09. It is basically a heat map, right, so you can see that there is more fishing off of kind of the deep end of Murrells Inlet and then in between Jacksonville and Wilmington than there is say off of Savannah during this time period.

You can see that it has been proportionalized as a percentage of the total landings and a percentage of the total effort. That was done to protect confidentiality for the fishermen. But you can get a sense of where fishing is taking place under this Scenario A in April. Here is Scenario B, and this is based on the summer of 2013/14 season.

If these two look different; one of the key take-home messages is that if we had just used the winter 2008/09 fishing distribution; that might have led us down the wrong path; because remember there have been a lot of changes since that time both with regards to where black sea bass are, how much the population has recovered, and then also all these different regulatory processes that have changed who is even allowed to fish with pots and then how they fish with pots.

This scenario was attempting to encompass some of those dynamics in terms of space; because if all these pot gear endorsement holders have moved to South Carolina from North Carolina or vice versa and we just used the old data, we would have missed that; so we needed to have a sensitivity run that encompassed that.

Then our final scenario was the one recommended by the Science Center; and this is the mean, which is why there is a bar across the numbers at the top there. For the winter 2006/07 through 2008/09; you can see that we're looking at again six different months under this scenario rather than a static version under Scenario B. This is Figure 3H through M in the report.

I've talked a little bit about the black sea bass fishery being dynamic. This figure tends to show that. You can see that basically what you've got here is a 3-D figure with the month running along the X axis, the fishing year running along the X2 axis or the Z axis, and then catch per trap day heading up the top.

What you can see here is that you have higher catch rates early on in that time series in the wintertime and then you can see that it shifts to higher catch rates in the summertime. That is really all I want to show with this figure is you've got that quota closure and that derby mentality taking over; and then you have no fishing going on in the wintertime in the most recent seasons.

Here is the output for the sensitivity runs on catch rate. On the left-hand side here you are looking at catch rate, which is pounds gutted weight per day from June through May for the different scenarios. The first one in red is the status quo, and so you can see there was no trap gear fishing November through May under the status quo.

The second one there is the yellow dash line; that is Scenario 1, which is based on winter 2008/09. The third one down, which is kind of the tan color is Scenario 2, and that is based on winter 2008/09 scaled to the 2013/14 catch rates. You can see that is by far the highest catch rate.

That basically is saying that we had these really huge catch rates in the summer of 2013/14; but if they had left them open in the winter, based on historical data you would anticipate that the catch rates in the wintertime would have been even higher. That is kind of our high scenario there.

Scenario 3 assumes that the 2013/14 catch rates from October are constant throughout the winter season. Scenario 4 is that mean of the '06 through '09 seasons. You can see it kind of falls in the middle of the other scenarios we had already evaluated. Then the green there is the other gear landings.

You can see they've got a pretty fairly high catch rate for those other gears, and that is based on the 2013/14 season. Then on the right-hand side is just kind of a caveat to that really high catch rate there in Scenario 2. Is that unrealistic? Maybe, we sure didn't see that in winter of 2008/09. But if you look at the status of the stock with regards to the number available to the pot gear fishery, so the exploitable abundance from the stock assessment, SEDAR 25, in 2008/09 relative to where it is anticipated to be in 2015 on that figure on the right; there is a lot more fish available to the fishery. Maybe that high scenario could be a realistic catch rate; time will tell.

Then the next thing that we needed to map out was where the right whales are. This is based on the two different models we received from FWC. At the left here is the November through December time period. We used December as a proxy for November because there is not enough aerial survey data for a model in November. Similarly for April, we used March as a proxy. Those two months have the same distribution of whales in the model.

On the bottom there on the left, you can see the scale running from green to red, with red being the most concentrated and green being the least for the right whales between Florida and South Carolina. Then on the top there on the left, you can see that scale running from kind of the lighter blue to the purple. That is the model for North Carolina. It is a bit more discrete up there off North Carolina.

It is a migratory pathway during that time, although there is some short-term residency as well. As we advance through here, you will see kind of the most, I guess, dynamic portion of the model is just the spread of the whales in that core calving ground. That is the part that really draws your eye. You can see the November through December and then you've got January on the right, then February, and then March through April. This is all in Figure 7 in the report if you want a closer look at it.

DR. REICHERT: Nick, remind me, the numbers for the whale sightings; what do they represent again?

DR. FARMER: Those are almost like a unitless number. It is a sightings' frequency within the model or predicted sightings 'rate. I don't think you could explicitly tie it to like sightings per hour or something like that. I would have to check with Tim about that; but my understanding is that the numbers for the Florida through South Carolina model are not comparable to the numbers in the North Carolina model.

That is kind of the take-home message, which is why he did color coding instead so that you can see it is more of a visual overlay of where things are. For the sensitivity runs for the warm and the cold, you can see for the cold that the distribution is not as compressed inshore by the Gulf Stream; and so the whales spread further offshore and they also seem to occupy a little bit more of the southern portion of the territory with regards to their core portion of the range.

In the warm model, the edge of the Gulf Stream compresses the right whales inshore a bit more because that is a thermal tolerance issue for them. They are found more inshore and distributed a bit more north/south; so that is why we did those two sensitivity runs. Then the next step is overlaying the fishing effort on top of the right whale distribution to compute your monthly relative risk to right whales.

You can see this is now Figure 8. On the left is the scenario based on the 2008/09 commercial logbook data; that is Scenario A. Then on the right is the 2013/14 data, and that is Scenario B. You can see the differences with regards to the risk to whales in November between those two scenarios and where the core of the fishing pressure seems to be located. We can advance through these slides.

Here is December, January, February, March, and then April. Underneath that we also are simulating the quota closure dates, so you can see on the right here – and I expect you guys can all read that because I can read it from here – is all the sensitivity runs that we did for one of the right whale distribution scenarios.

This is the output from the mean scenario for right whales under spatial distributions A, B, and C and catch rates 1, 2, 3, and 4. I don't know; it is kind of like a Rapunzel hair plot, but you can see you've got these quota closure dates that can vary under those different sensitivity runs; and

sometimes they don't get to the November/December portion of the right whale season. That has an impact on the relative risk to right whales, which is why it is important to model that quota closure date.

With regards to cumulative effects, this is a zoom-in on Figure 10 – and I'm not sure why the axis seems to have disappeared on two of the three plots. It showed up on my screen; but, anyway, you've got Scenario A1, so that is fishing spatial distribution Scenario A under catch rate 1; Scenario B1 and Scenario C1. In this figure basically you are looking at quota closure date on the left-hand side, and that is the yellow bars.

The top there, which isn't showing up for some reason, is December 31st; and so under Alternative 1, status quo, you don't get a quota closure, so it is open through December 31st. Then you can see under Alternative 2 the quota closure is much earlier, and I don't know why the axis isn't showing up, but it is much earlier. And then you've got a few alternatives that also get you to the end of the season.

DR. BERKSON: Can you define what you mean by cumulative effects?

DR. FARMER: Yes; what you're looking at here is the cumulative right whale risk while considering a quota closure. If you did not consider a quota closure, then your right whale risk might look completely different, because you just overlay where the right whales are and where the fishing gear is and you call it a day.

But because some of these scenarios allow the fishing to take place at a higher rate or a lower rate, they close earlier or later so there is less total risk. You have to consider the dynamics of the two components together, so in cumulative; otherwise, you're missing a point.

DR. SCHUELLER: On the left, the quota closure dates just for the yellow bars; and the green and blue bars are?

DR. FARMER: Relative right whale risk;, and again there is a legend that is not showing up either; but there is the green, which is the Florida through South Carolina relative risk and the blue is the North Carolina risk. You can see it is scaled between zero percent and 100 percent, with Alternative 2 being 100 percent and then the status quo, Alternative1, being zero percent.

DR. BERKSON: Well, you used the expression if a closure was considered; you mean if the closure was in place, correct?

DR. FARMER: No, what I mean is if you did this analysis and you broke out the components separately and just projected when is your projected closure date and then how much right whale risk is there per alternative rather than considering them simultaneously; you would have misleading information.

If you assume that you've got right whale risk January through April and also November through December; okay, there is your total risk; but what if the fishery is closed due to a quota closure somewhere in that November to December time period or before November to December? Well, then you don't have any of that risk November through December.

DR. BERKSON: Why is that considered versus actual? I don't see the point you're making.

DR. FARMER: I'm just saying that this is a realistic way of looking at it because of the way we manage these things. If you are not also considering the dynamics of the fishery and when a quota closure would take place to keep you from going over the ACL and you just looked at these alternatives as regards to right whale risk; some of the alternatives might be more effective for right whales than they are otherwise represented or vice versa. You have to think about when they are going to close.

DR. BERKSON: No, I understand the analyses you're doing and the point and I think I understand what you're showing. It seems to me that each of these options is the cumulative risk under that scenario.

DR. FARMER: Correct.

DR. BERKSON: For the closure that takes place that you are modeling.

DR. FARMER: Correct.

DR. BERKSON: Considering sounds like a potential thing that might happen rather than something you are definitely modeling.

DR. FARMER: No, it is being modeled. Sorry if I was misleading in the way I spoke. Yes, that is being modeled.

DR. BERKSON: It's the way I heard, not the way you spoke.

DR. FARMER: There is another figure that will be a little bit easier to interpret than this one coming up right here.

MR. CARMICHAEL: The relative right whale risk, when it says 100 percent, that means it has the same risk as Alternative 2?

DR. FARMER: Correct.

MR. CARMICHAEL: That is really just a scalar to Alternative 2?

DR. FARMER: Correct.

MR. CARMICHAEL: So it all starts with whatever the risk is in Alternative 2?

DR. FARMER: Correct.

MR. CARMICHAEL: Which we probably don't really know what the real world risk is of Alternative 2 of whale encounters or do we?

DR. FARMER: Well, it is that you don't have real world knowledge of what your entanglement rate would be; and these models are outputting unitless numbers. Well, they are sighting

potential or sighting frequency numbers. It is not a one-to-one translation, so the only way to make these comparisons between alternatives is in a relative sense. Whether or not these denote a level of risk that would be jeopardy under a biological opinion, for example, which I think is what you're driving at, would be a decision that would be made by staff in Protected Resources.

MR. CARMICHAEL: No, I don't think I was describing anything like that. I don't even know anything about that. I am just trying to put it in terms of a whale swims by and runs into a net. You always try to – at least I do because I am simple; I always try to put things in a sense of what goes on in the real world, the same with fish live and fish die and fish get caught.

It is kind of like you want to estimate discard mortality, but you don't know how many fish are caught and you don't know how many of them die when they're caught. We're going to have a risk of discard mortality, but we can't put it in any currency that a fisherman could understand. I guess that is where it gets difficult when you think about how we deal with this down the road and looking if there is anything you can hang your hat on that puts this in some sort of real world information you can see to help people understand; and I think it is important to make it clear, because people see 100 percent, they can think 100 percent risk to right whales, wow!

If I am a passionate whale person and I see that, I am going to be like, man, 100 percent risk to right whales. Well, that means they are going to encounter a right whale; but it really doesn't mean that at all. It might mean there is still only a 1 percent chance that you would encounter a right whale; we just don't know is the bottom line.

DR. FARMER: Sure, and I guess two things in terms of the relative comparison there. Basically what you're doing here in your analogy with discard mortality would be you are comparing if you keep a fish on the deck of a boat for ten minutes versus you use a barotrauma releasing tool and send it back down to the bottom immediately after pulling it back up; maybe you don't know what your release mortality rate is, but you could probably make a conclusion that would be pretty scientifically sound to say that one would be better than the other under that scenario. In this case we actually have at least a quantitative scale that you can make that comparison to; so you don't exactly know what that hundred percent translates to, but I think the comparisons in a relative sense are reasonable.

DR. BARBIERI: By the way, I just want to point out; I already this morning am trying to drink as much coffee as possible, because having a group of eight people who do nothing but work with models every single day, go over two days; it was like having to pry that into our skulls; you know, for us to be able to really understand what was going on. That was like two days.

It was very difficult. There were a few things that happened, like, for example, the metrics that you are talking about for us was difficult to understand that this cannot be based on science. There was a correction, that unitless that I was telling Marcel about, there has to be a correction factor for the probability of you actually being able to see given the flight path, given how often they come to the surface and would actually be spottable.

There are all these kinds of correction factors that come into play that makes for our simple fish people brains kind of very difficult to understand. I'm just trying to bring perspective that it is not that we are picking on the presentation or the analysis. It is simply that trying to wrap our heads around this is difficult.

DR. BERKSON: I think John brought up a really good point about how that percentage could be misinterpreted by people who haven't taken the time or the background to go through this. A very easy way to fix that would be to have that be something like relative right whale risk units on a scale of zero to 10, where the maximum is 10. It is still all relative; but people don't zero in on that percentage and assume that 100 percent means 100 percent risk. A very, very, easy thing to change and you can explain it; the same amount of text explains it in the paper.

DR. CROSSON: I looked at this paper for a few days as one of the people at the Science Center reviewing it and it made my head hurt the whole time. Maybe an economist is an even simpler brain than a biologist. I kept thinking about it and finally I guess, unless I am completely misinterpreting it, if there is a right whale in an area and there is a black sea bass pot with a line going to the surface in that same area, there is some probability of an entanglement that is above zero. It could be one in a million; it could be one in a hundred million.

Now what we're talking about with this – so it is unknown, but what we're talking about this relative percentage is the percentage within that. If it is one in a million possibility that you're going to have a right whale entanglement, then what would be between zero and 100 percent chance of that one in a million thing happening; and that is an extremely difficult concept to get your head wrapped around.

Then, of course, on top of that, we have the fact that there are all these different changes in regulations that have occurred. The new black sea bass pot permits have occurred, the economic changes that have gone on in the fishery where you are forced to use older data to try and map where fishermen are putting pots now. It is really a very complex thing, and you are not alone.

DR.BUCKEL: I think it is the risk of what; and so we don't have the probability of entanglement, but this is suggesting its risk and entanglement. What we really have is like a co-occurrence. I do predator/prey work, and the first thing is do they overlap in space and time; and that is what you have here.

Then the second thing is what is the probability that prey ends up in the stomach; and if it is zero, then the predator is never going to have it in its stomach. We're missing that probability of the prey being in the stomach. What I looked at was a biological opinion from 2006 and NMFS biological opinion.

I will have to pull up the wording, but it is extremely unlikely and discountable; and that is when you had an order of magnitude higher effort. I think backing up to what John said and taking a simple look, it seems like if that biological opinion still holds if there is not a new one since 2006, then that probability has got to be even lower, because we have a lot less effort.

DR. BARBIERI: That is a very good way – I mean, that helped me already kind of think about that in those terms, because of actually how he described it.

DR. CROSSON: Well, we do know a few things. We know that there – unless I am mistaken – there has never been a documented right whale interaction with a black sea bass pot over the lifespan of that fishery. That is one bit of information we have. We also know that effort has declined because of the 18A black sea bass pot endorsement system and the trap reduction; and then right whales have increased

We know we have some estimate of how much the population of right whales has increased over the past, because that is going to also increase the risk. There are like ten different variables that are moving around at the same time.

DR. GRIMES: We are maybe getting ahead of ourselves here, but you brought this up and it is an interesting question. How do you reconcile this relative right whale risk that you have here with some kind of ESA jeopardy standard that will come along later on about it is okay to kill so many right whales; the actual risk.

DR. FARMER: I don't know if she is available, but I think Barb Zoodsma was at least on the webinar and had wanted to call in to address right whale specific questions. I am not doing the biopsy; I don't know much about the biopsy. I am in SF; I don't do the PR stuff. But my understanding is that if there is a substantial change to the fishery that needs to be considered, then there would be a biological opinion that would be performed.

Then they have to make a call as to whether or not that is jeopardy. I would imagine for right whales it is going to be a pretty low threshold of tolerance. You said how many right whales can die; well, I think the answer there is probably zero. You look at how many there are, there aren't many. There is probably more people in this hotel than there are right whales in the whole North Atlantic.

DR. BERKSON: I haven't seen one in this hotel.

DR. FARMER: That is because he was entangled earlier. Also, there were some statements that there are no documented occurrences of entanglements in black sea bass pot gear. Well, I am not sure if that is entirely true. There certainly have been a high percentage of the occurrences of entangled right whales in fishery gear were in pot gear, maybe not black sea bass pot gear, hard to say because the gear is not marked enough to be able to make that distinguishment; and it is obviously dragging on the right whale in the water for a long period of time; and a lot of these things die at sea and are never seen.

These entanglements may be happening at a much higher rate than is documented. Obviously, seeing a right whale is hard enough, both because they are rare and because they are out at sea, and they are the same color as the water and they sit low in the water. That is why ships run into them all the time; so seeing it is a hard thing. Well, pretty frequently, often enough that it is a big concern for the population sustainability. It has made some big impacts on shipping, which is a big economic hit.

DR. ERRIGO: I was also trying to read through and understand the models; I guess I don't really have as good understanding as I thought. Just looking at like say Florida, Georgia, South Carolina model; if you look at these sightings' model, it does not calculate your probability of sighting a whale given a set of circumstances?

DR. FARMER: Is that a question for me? That may be indeed what it is doing. I think that you are actually right is basically what you're doing is you are taking the number of sightings as an input and then offsetting with effort in some sort of, you know, probably a zero-inflated type of modeling approach and you are fitting it to compute a sightings' rate.

Now the point being is that the way the effort is conducted off of North Carolina and then the Florida through South Carolina area is different; and the sightings' rate and also the behaviors of the whales in those area is different. Those models aren't necessarily comparable with regards to does a sighting rate translate directly into population abundance?

I think the answer is probably not exactly. I have got a discussion slide on that coming up that whale behavior being different can impact sightings' rate, and that is well known. But in a relativistic sense, it gives you an idea of the distribution of the population in space, which is kind of the key that we're trying to get to here.

DR. ERRIGO: Let's just look at the Florida/South Carolina one, because that is much better parameterized it looks like than the North Carolina model, which had much less data to work with. It calculates the probability of sighting whales given a set of circumstances in a given area. If you look at, let's, say pot effort and look at those same set of circumstances that occur when pot effort is occurring; spatially shouldn't you be able to overlay and get a probability of sighting a whale in the area where pot gear is being placed, given the depth and the area and the temperature and all the factors of the model, and then get like a probability of co-occurrence? It seems like we should be able to calculate that.

DR. FARMER: That is what it is doing; that is the overlay that is happening there. But the point is that a sightings' rate, assuming uniform effort, doesn't exactly translate into a number of whales. But by doing that overlay, yes, you are looking at by definition where the whales are overlaid by the pot gear effort.

You are getting basically a multiplicative scalar there that has units but don't directly translate into an entanglement rate because the entanglement rate is unknown; but you do get a relative comparison that can be made between both spatial cells with regards to where the overlay is, also across months where the overlay is, and then between alternatives because portions of the effort get removed by each alternative.

DR. BARBIERI: Nick, before you go on, I think there was a list of names here of folks on the webinar; that if you want to take a look at perhaps not right now, not to interrupt the presentation, but it would be good for us to see if we need to call for reinforcements in a way to help us explain some of these things, we have some whale biologists.

DR. BERKSON: Nick, you may be getting to this, but I am interested in the work you may have done looking at uncertainty within a scenario. You have done sensitivity runs to get some sense as to the uncertainty between scenarios, so major changes in catch rates or water temperature or those kinds of things.

Within any one of these scenarios, when you pick all of these factors, it is like you are going to run this with all of these factors; we are coming up with a point estimate, and we know that we can't have a lot of faith in those at those point estimates. There is just a lot of uncertainty out there. Have you done anything to look at how to characterize that uncertainty and how to compare these scenarios in terms of that uncertainty?

DR. FARMER: Well, what you would do in a more classic approach is your historical data would be more informative of your present conditions; but we have so many regulatory changes

that you can't run like a classic regression model on catch rate – I tried it – to predict what the future catch rates would be. It would come out very similar to these catch rates right here; and then you would have confidence limits on that catch rate.

That would be your within-scenario uncertainty. Dr. Gowan's model also I assume has probably some model-based lower and upper confidence limits within its outputs as well. But what we felt was a better way of capturing the actual uncertainty and what is happening here; I guess the point is that the inter-across model uncertainty is much higher than the intra-model uncertainty in this case.

What we are attempting to do here is to bookend the realms of reality that we might be operating in. Again, keep in mind that the main output of this is still going to be a relative scale between zero and 100 percent, comparing the alternatives, which is what is needed for the council for Regulatory Amendment 16.

They are going to need to look at what is the relative biological impact and the relative economic impact of these alternatives. We have economic impacts, because we have closure dates, and they can compute how many days of black sea bass pot gear fishing that gets and how many days of vertical line fishing that gets under each of the alternatives.

Then we have a biological impact, which is the percentage of the right whale risk on a relative scale. You can make those comparisons that say – I mean, the most obvious one to point at is Alternative 1, status quo, with no pot gear in the water November through April has a much lower risk than Alternative 2. What is that risk level? As a unit of right whales killed per trap pot hour, we don't know; but from a scale from zero to 100 percent, you've got it.

DR. LARKIN: I think this is something maybe we would talk about later, but it is curious to me that economic impact – I mean, I don't see any economic analysis in the report. It all seems to be synonymous with landings. I was wondering if there was going to be a point, because the goal is to increase socio-economic benefits; is there any attempt on what the costs of these alternatives are going to be, because some of them have quite significant impacts.

I mean all the gear restrictions have costs. I think some of the different alternatives in terms of when the fish are harvested and the quality of the fish might have impacts on the value of what is landed. I think that is a side point to what this discussion was going to. I think I sort of perked up when you were sort of saying this is how the economic benefits were captured compared to the biological benefits.

DR. FARMER: To that point; typically the way these things operate is that we'll come up with a model of what is going to happen with regards to landings. We provide that to our economic staff, and that is broken out by mode and vessel and all that stuff. All those inputs that they are going to need have been developed at this point, but this was finished. We had to beg for a late deadline to turn it into you guys so that you would have time to look at the report.

But certainly for Regulatory Amendment 16, as all regulatory amendment and plan amendments for the South Atlantic, there will be an economic analysis and that will be associated I am sure with the monthly landings and the value of the monthly landings by gear. We've got spatial stuff in there.

If they have the technical knowhow and they have a way of quantifying it, they could even look at the cost of motoring out to an open area within a certain alternative and get those sort of ex-vessel costs and sort of things like that.

DR. BERKSON: Going back to my point; there are less classic ways of looking at uncertainty within a model run than to go through the formal methods you are talking about. One of the reasons you do that is to better understand your model and the effectiveness of your model. I agree that what is most needed is a comparison of the output from the different alternatives.

But I think it is also very, very instructive and informative to see how well this model is capable of providing this kind of insight. I think you wouldn't have to do that for every alternative to gain that kind of knowledge. I think that is a critical step in the process.

DR. FARMER: Do you have some more detail on what it is in particular that you're looking for?

DR. BERKSON: I would be happy to talk with you about it.

DR. FARMER: If you want to talk about it on the record, one of the goals of this process for me is to get the input of the SSC on how, if we need to, we can make this a better write-up. I think we've addressed a pretty fair amount of uncertainty in here; but if there is some other stuff that you think is going to contribute to the council's decision-making process; I am more than happy to do that.

I guess I don't fully understand what it is that you are after; and I would love to get input not only on what you think should be done or could be done, but also what everyone else thinks of whether or not that is necessary, because that is going to drive Regional Office priorities and staff time and that sort of thing.

We try not to do things that aren't going to contribute additional knowledge that is going to be critical to the decision-making process, because we've got a lot of stuff going on. But if you think it is going to be something critical, I would like to hear about it.

DR. BERKSON: I think doing more simulation modeling and looking at more alternatives for parameter values, and not just the primary parameters that you are using in here, but some of the more interaction-type terms, other factors that go into this kind of model, looking at the uncertainties in the Gowan model, seeing that those carry through into this.

There are a lot of steps along that way where there is uncertainty in model structure and parameter value; and right now it looks to me like you've got a three factorial design where you are looking at three levels of uncertainty, something like that. Well, we know there are a lot more than three factors of uncertainty involved here. It is a matter of making a list, prioritizing those, brainstorming and seeing what can be looked at easily and what can't; and not trying to do everything, but trying to do more than has been done. Like I said, I think that can be discussed and we can brainstorm that a bit.

DR. BARBIERI: Just to that point; I think, Nick, the perspective here is this is not thumbs up or thumbs down. This is really a way to sort of add icing on the cake. Of course, the Regional

Office, the agency in general is going to have to look in terms of what the timelines are to have this in front of the council and do we have time. Like any other assessment or quantitative analysis that we do; we could do a heck of a lot more if we had more time.

This is going to be something that I think the committee was going to be presenting some of these suggestions in the interest of providing you with some of that input. It may not be realistic for those analyses to be completed in time to address the regulatory needs of Protected Resources as well as the council actions.

DR. FARMER: I guess my one comment along those lines is I hope we can agree that the inter-model uncertainty that we've captured in these scenarios is probably a lot broader than the intra-model uncertainty. I guess my concern with regards to time spent developing those sorts of alternative scenario runs is that ultimately your output is still a quota closure data that then gets compared within that group of input parameters to quota closure dates under different alternatives; and then a relative overlap or relative risk to right whales that then gets compared to all those other alternatives.

The absolute value of the number coming out of that for each of those runs might be different, I would suspect, because the intra-model uncertainty says the same thing; that the relative comparison between the alternatives is going to be probably pretty consistent. We've done three by four by three by six months of sensitivity type runs here.

If you look at the outputs, they are all very consistent. There is some difference in the range there; but because they are scaled within each run, the outputs are all fairly consistent. That actually leads me perfectly into the next graphic. These are the outputs under the mean whale distribution run. You can think of this kind of like a limit control rule.

You are looking at the sensitivity runs relative to the projected closure date on the Y axis and then your relative right whale risk from Florida to South Carolina on the figure on the left for the X axis and your relative right whale risk for North Carolina is in the figure on the right. What you are seeing there in the numbers that are color coded is the model outputs across all the sensitivity runs for catch rate and spatial distribution of trap gear pressure within the mean right whale distributional model runs.

You can see that they are fairly consistent here. Alternatives 1, 5, and 7 up at the top left there provide the longest season and the greatest protection to right whales. Alternative 6 is all kind of clustered there as a slight step down. 8A and 8B get you a long season, but have relatively high relative risk to your right whales.

Four is kind of in the middle there; and then you see 8A and 3 have more risk in about the same season length as 4 and so on. The basic point here is you can see that within all those different catch rate scenarios and all those different spatial distribution scenarios, you get a fair amount of clustering in outcomes.

I have this for the warm scenario as well and then again for the cold. I could overlay all those on one graphic, and I think the story would still be about the same. The scenarios that result in the greatest protection for right whales and the longest season are 1, 5, and 7.

DR. BERKSON: I will only beat the horse dead one more time. This is really helpful, this is extremely important, and I agree with you this is showing consistent results. When you go back and show me the previous slide, those are point estimates and that implies to me we know those values and we know those values with certainty.

We don't know those values with certainty for any one of those alternatives. Nowhere in this presentation or the paper, I think, are you giving us the sense as to the uncertainty within any one of those alternatives, more of the uncertainties, uncertainty that can be captured given what we know and how that would compare between alternatives.

Once again, I don't think you have to do that for every alternative, but I think to get a sense as to what we know versus what we don't know, there is a lot of stuff going into this model. Just the Gowan stuff, there is a lot of uncertainty I have no doubt in the environmental parts of this model that are in here.

What difference does that make and how much confidence can we have in the numbers coming out of this? You are right; it is not going to change probably the relative comparisons of the alternatives, which is what we ultimately need to know. I mean, I am a modeler and so I am really curious as to how well do we know output of individual runs?

DR. BELCHER: Can you go back to that previous slide? I think in some of this, again back to Jim's earlier point about using that probability, it really does – it is hard to digress your head away from certain aspects of what that number says. If you take that scale away and you look at it in terms of the risk component, you can see where the lower values are.

But my question and concern is who is going to determine what level of risk is acceptable for a right whale? I understand the overall probability is very, very low; but a 25 percent chance of occurrence, is that going to be acceptable for ESA, because all it takes is one animal. One animal is it because of the small numbers of individuals.

The probability, even though it is one in a million, one in ten thousand; whatever, still when it happens it happens and it is a critical piece. We can look at that. We can set forward alternatives that have lower risks, but are those risks still going to be good enough for the ESA people? That is I guess kind of that overarching thing.

DR. BARBIERI: I think this is a good question, Carolyn, and I wanted to jump ahead of Nick here just because having discussed this extensively with different agencies and seeing what the expectations were for what the SSC input here would be is that we are really more in the line of Jim's questions, suggestions, and recommendations.

We are really evaluating the analysis of scientific validity. Does this represent the best scientific information available and provides credible output, like what we did yesterday for those stock assessments, but without really making a judgment call on how much is enough, because that is really a Protected Resources, as I understand it, and it is not really a council function in that sense; and that creates a complication because how can we evaluate fully what the analysis is producing without knowing where the reference points basically are. We are looking at the mechanics of a stock assessment but without looking at how the outputs compare to reference

points, basically. Those reference points exist within the Protected Resources Division Universe, and I don't know where they are. We are not being presented with them.

DR. CROSSON: Actually, my thoughts are similar along the same lines. One of the questions I have from a regulatory perspective or an economic perspective is that I don't know what the regulatory consequences would be if we shifted from the current closed season that we have to one of these other alternatives; and then there was a right whale entanglement, or even worse a mortality, what would be the regulatory consequences under that circumstance?

Would it result in just shifting back to what we have right now with the particular closed season or could it be something that would be more severe than the current status quo that we have. Could it result in an entire shutdown of the black sea bass pot fishery; I don't know that. But those would have potentially very different economic signals about what would be the best thing to do right now.

If it would just result in going back to the current status quo, then I would think that any of these alternatives would be something that would economically result in benefits; but without knowing what the regulatory consequences would be of a right whale interaction, I have a difficult time quantifying that.

In terms of the cumulative effects, well, this one and a following slide, following Sherry's point; some of these scenarios having very long seasons is beneficial, but one of the things – and I suspect this is the case but I haven't looked at the data – they are shifting the year to begin on January 1st; and I know that winter black sea bass pot fishery is very – I don't know if it is particularly lucrative, but it seems that is a filling in a slot for market-competing snappers and groupers that are not available at that time of year.

I suspect that they are getting a higher price in the winter months than they would be if they were fishing for it in the summer when lots of other market-competing species are open. What Sherry brought up about that the price questions I think it something that is really going to have some impact on looking at these different alternatives, because season length is not the end all and be all.

DR. SCHUELLER: Yes, I would like to go back to Jim's point and say that I agree with him. While the means that you have here will not change so the relative rankings will not change; the mean may give the illusion that one is better than the other where if you actually looked up the uncertainty within, it would all be equal. You think you are picking a better option, but maybe you're not. I think that Jim's point is very important.

DR. ERRIGO: I am noticing there may be some confusion. When the season changes to a January 1st start date – and I can have Michelle or Ben come up to confirm – the pot season still does not start until May 1st. Basically if the closure is removed, it only will allow pot fishing in November and December. There will never be pots in the water January through April, regardless.

DR. FARMER: I think that is incorrect. I think that Regulatory Amendment 16 contemplates allowing pot fishing throughout that November through April time period. Under Regulatory Amendment 14, which is being implemented now, pots would be closed January through April

and also November and December. They are not going to be opened by Regulatory Amendment 14 for November through December; they are still closed. The only way that pot gear would be in the water in November through April is through the actions that are being contemplated in Regulatory Amendment 16.

DR. BARBIERI: Nick, I hate to break the flow and if there are additional questions that are on this point; but we have been at this for an hour and a half; and if we are at a good breaking point, I think it would be good to take a little bit of a break and kind of return, unless there are some questions that are so –

DR. BUCKEL: I think this will be a quick one. The 8A and 8B have to do with trying to allow for – my understanding is a migration period through North Carolina, because they are closing a portion of December, but then opening it up something like January, and then allowing it open maybe off of North Carolina when there is no migration of right whales through that area, but then closing it in time for the migration back north.

Unfortunately, the North Carolina whale model, you couldn't do by month, so it is difficult to address that subalternative without some monthly information on whales. You mentioned it is the corridor; that is where they are migrating through, but then there was also maybe some are occurring there throughout the year. Having the sighting data by month I think might really be helpful for the council and for us to choose to think more about that subalternative 8A-B.

DR. FARMER: In response to that; I think the sighting data off of North Carolina, if you looked at it by month unadjusted for sampling effort and considering the limited sampling effort that went into it would be more misleading than looking into the actual model output that we have right now, which is at least trying to control for some of that.

The thing is if you look for something in one place, you are more likely to see it than you are in a place where you've never looked. That is the basic issue that you're having with all of these extremely low abundance type species; and we've dealt with this before with the speckled hind and Warsaw grouper, which I presented to you, where there was some argument we should look at just point occurrences and base closures off of those.

It was like, well, maybe not, because you've only looked there. There are other places that these things could be. This model is attempting to adjust for that using data over a longer set of years. Now, of course, if you could develop the model and break it out on a monthly basis, then that would, of course, be informative and helpful data.

But in terms of the sighting over that time series, we do have documented knowledge that, yes, the whales do occur Florida through North Carolina, throughout that whole region during that entire November through April 30th time period. I think that there has been some confusion as to whether there is a migratory period or not.

Yes, there is a core calving grounds that is November 15 through April 15, but that actually has extended up into North Carolina; and there are peer- reviewed papers that talk about that. Also, there is a constant migration. Even in the calving areas, the residency times are usually less than a month. These things are moving back and forth even with their calves. That whole area is a place of concern during that entire November through April time period.

I wouldn't get too concerned about the lack of monthly. Yes, it certainly would be a huge contribution to the model and probably would change the North Carolina percentages a bit; but with regards to the way that Subalternatives 8A and 8B are structured, if you look – and I can't get it to pop up, I don't know why, John; but if you look at the way they are structured, the problem that you have with those is that with Subalternative 8A there is no closure between December 15 and March 15 the way that it is written right now; in any place.

Then under 8B, the closure only applies November 1 through December 15 and March 15 through April 30 in that area. You are only protecting them during what I think the council was under the impression is the migratory period; but the point that they are probably missing there is that the whales are moving back and forth throughout that whole time period, and there are whales there the whole time.

DR. BUCKEL: The UNCW data that were used for the model; are there any transects that were done each month that could be used where you could break those transects out and say, okay, let's look for some monthly patterns to see how much of it is what you are saying that they are always there every month of the winter versus there is some evidence that there is a larger number of sightings on a giving transect in December and April, but then January/February there is less. That would be helpful.

DR. FARMER: Well, I will go back to Dr. Gowan and see if I can get a response from him about that. He is very knowledgeable about that sort of stuff.

DR. BARBIERI: Nick, if it is okay with you and it wouldn't break the flow and we are at a breaking point, let's take a ten-minute break and see you back here in about ten minutes. I am going to request SSC members to please return to your seats. We still have the bag limit analysis to finish; and then if there is time, we'll go back to the Hogfish ORCS and try to get that finished as well. Nick, if you are ready, we can reengage.

DR. FARMER: It is challenging to project the impacts of this regulatory amendment and those proposed alternatives. There have been substantial changes to the fishery; and there is no trap gear that has been in the water in the last five years. We have run many scenarios to try to capture the range of uncertainty.

We've got the four catch rates, we've got three spatial distributions of traps, and we've got three spatial distributions of whales. As you saw in those previous plots, the results are pretty consistent across that range of sensitivity runs. Here is a figure that kind of sums it up in a graphical way; green is most protective, red is least protective, and we've tried to break it out on the scale of right whale risk in the words low, moderate, high, and very high; so 0-25 low, 26-50 moderate, et cetera.

Most protective, obviously, Alternative 1, there is no trap gear in the water November through April, risk of entanglement zero. Alternative 7 had a relatively low risk off North Carolina and very low additional risk off Florida through South Carolina, 0-2 percent. Alternative 5, pretty similar outputs to Alternative 7.

Alternative 6 kind of gets into the low to moderate range off of North Carolina and low to high from Florida to South Carolina; from 14-58 percent; Alternative 4, low to moderate, 22 to 35

percent off North Carolina, moderate to high off Florida to South Carolina, 23 through 68 percent, and so on. The highest risk coming, of course, from Alternative 2, which would allow trap gear fishing throughout November through April, although in a lot of those instances you get a quota closure.

In terms of the discussion, it is a dynamic time for commercial black sea bass pot fishing. You've got the 2013/14 season, 99.6 percent of the ACL was caught, and that was with no traps in the water November through April. Nearly all the scenarios for Regulatory Amendment 16 would result in a quota closure.

The dynamic transition of the commercial fishery – and I've talked about it – used to be a peak winter season fishery from1999 to 2009, became a derby fishery with high summer catch rates and early quota closures from 2009 through 2012, 18A was implemented, which caused effort restrictions and trip limits; Regulatory Amendment 19 after that with the increased ACL; and then Regulatory Amendment 14 with new trip limits and a January start; and now we're looking at Regulatory Amendment 16.

There are a lot of things going on in this fishery that make the past data not the best predictor of the present. Reduced trap gear participation was offset in a lot of ways by increased other gear harvest. In the 2013/14 season 68 percent of the commercial harvest came from other gears. That compares to a 28 percent average from 2004 through 2013, so a lot more participation in the other gear sector.

That could be that the stock is rebuilding, and so it is easier to get with other gears, and it probably also has a lot to do with guys who lost their black sea bass pot gear endorsement that are now catching them with other gears. With regards to uncertainty in catch rates, number one, Scenario 1, which was based on the 20008/09 winter season; does not account for the fact that the stock is rebuilding; but the winter catch rates that you observed in 2008/09 are pretty similar to those that were observed in the summer of 2013/14.

Scenario 2, which is based on the winter catch rates of 2008/09 scaled to the catch rates observed in 2013/14, so you get that really high catch rate in the winter, doesn't account for localized depletion. It could be that you've got this high summer catch rate; and then you get into the wintertime and they've locally depleted some of the stock and you don't get those winter catch rates. It doesn't account for that.

The catch rates that Scenario 2 predicts have never been observed in consecutive months, but it might be realistic because the stock abundance has increased substantially. Scenario 3 doesn't account for the temporal dynamics of catch rate between the winter and the summer, because this is based on the summer 2013/14 data. There could be fish movement that happens there, could be adverse weather conditions reducing the number of potential trips. There could be increased catch rates because the pot gear is more efficient in cold water.

Scenario 4 does account for the potential impacts of the economic crash and high fuel prices in 2008/09 by averaging over the last three open winter seasons. However, it doesn't account for the stock rebuilding. With regards to uncertainty in the spatial distribution of fishing, Scenario A did not account for shifts in the core distribution of fishing pressure since the 2008/09 season. The stock might have shifted, because it is rebuilding.

Endorsement holders may have moved or may have dropped out. Scenario B, which is based on the current season, doesn't account for the inshore/offshore dynamics between the summer and the winter. Scenario C is similar to Scenario A in that it doesn't account for shifts in the core distribution of pressure since those old winter seasons or any endorsement holders that may have moved where they fished or dropped out of the fishery.

There is uncertainty in the spatial distribution of whales. There are three whale distribution scenarios. I think the average is probably the most appropriate for projection unless you know something about future conditions. The warmer model suggested that the right whale distribution would be compressed closer to shore, which makes the spatial closures more effective, because most of them are based on either distance from shore or depth, which is highly correlated with distance from shore.

The colder model had the right whale population located farther south and more broadly distributed offshore beyond closure boundaries; and that one predicted the closures would be less effective; and less is spelled with two S's; I don't know why there is only one there. There was insufficient data to evaluate temperature impacts off of North Carolina.

Also something we didn't evaluate in conjunction with these whale distribution scenarios would be any impacts on the spatial distribution of black sea bass pot gear catch rates, or fishing effort. I've heard tell that catch rates are higher when it is colder, and it is easier to catch black sea bass when it is colder.

Maybe you would get an earlier quota closure under that colder scenario, which would probably actually make it look more like the average because you would get less time with black sea bass pot gear in the water because the quota closure would be reached sooner. Uncertainty in the spatial distribution of whales is also informed by the fact that we have limited data.

Off North Carolina we don't have a time dynamic model as you pointed out. Off of Florida to South Carolina, the December distribution represented November and the March distributions were used to represent April due to a lack of data for those two months. The model implicitly assumes that the detectability of whale, which is the number of sightings, is equivalent across the spatial domain.

It treats the effort as uniform, but it also treats the probability of sighting as uniform. We know that detectability varies due to whale behavior. It is lower for migrating whales because they spend less time on the surface. It is easier to see a mom and calf that are the surface just chilling out and breathing than it is to see a whale that is moving in a huff from one spot to another.

That is simply due to the fact that these are aerial surveys and they are looking for whales, so seeing one on the surface is a lot easier than seeing one underwater. Visibility is obviously worse in poor weather conditions; so aerial surveys are less likely to see a whale in poor weather conditions.

The models are also based on the right whale distribution on the primary wintering grounds, not in the migratory corridor; and the residency data suggests that there are steady movements of these whales between the North Carolina and Florida throughout the Mid-Atlantic each winter. The model also averages across years with relatively low and years with relatively high sighting

frequency. That applies to the main model. Unlikely that this averaging would have a substantial impact upon the projected relative risk. That is a statement from Dr. Gowan.

The modeling approach implicitly assumes that the right whale entanglement rates don't vary by gender, size, space, or time. Obviously, certain behaviors or size classes of whales in certain locations at certain times might be more inherently vulnerable to entanglement than others. If I had to take a guess, and I am not a right whale biologist, but I would assume that a calf would probably be more vulnerable to entanglement than an adult.

Certainly, I would imagine that the mortality rate for a calf would be higher than for an adult. Trap gear soak times are not time dynamic and so these comparisons are relative. That mitigates most of the impacts of the fact that trap gear soak times aren't time dynamic. Now if soak times in the wintertime are shorter than soak times that were computed from the summer 2013/14 season, your total number of the right whale risk overlaid with the fishery would be lower.

The division through Alternative 2 mitigates most of those impacts. However, if you have a shorter winter soak time, that could result in a lower total in those final two months of the season, November-December. If under Alternative 2 you closed in October and then some of these other alternatives remained open November through December; then your total number that is adding in there for those other alternatives, November through December, would be less than the number that is in there right now. Then when you divide through by Alternative 2, you get a lower percentage. That could have an impact on that algorithm right there.

MR. CARMICHAEL: I was just wondering why that says entanglement rates, because I was understanding we were sort of modeling more the overlap, because we have information on where the whales are seen and information on where the fishery occurs, and we don't really know anything about entanglement rates. It seems like it is kind of surprising that is coming up here.

DR. FARMER: Correct; because the overlay is assumed to be a proxy implicitly in this model for entanglement rate; so the point I'm making there is that if your entanglement rate is uniform across the outputs from the whale overlays, then these relative comparisons that I'm making are fair comparisons in a relative sense.

However, if there are places where the whale entanglement rate or sizes of whales, or whatever, where the entanglement rate would be different than uniform, then that would throw a wrench into the relativistic comparative approach that has been taken here. Does that make sense? What I'm saying is –

MR. CARMICHAEL: Relatively; I mean kind of. I guess I was kind of on board with understanding the whales are here, the gear is here, there is a chance they may bump into each other; and that is sort of what we are modeling. You're now saying, well, we are also assuming that it is also reflecting entanglement rate. Relatively speaking, it is kind of a jump to see how that is justified in any way.

DR. FARMER: Perhaps this would help. Let's say that if entanglement rate for some reason is lower for whales that aren't moving around a lot versus whales that are moving around a lot; then in places where they are moving around a lot, then you would have a higher relative risk

then is being shown here. In places where they aren't moving around a lot, you would have a lower relative risk.

But we aren't accounting for that because we don't know enough about the way whales behave in one place versus another. I mean, we are seeing these things very infrequently. There is a very limited number of them. I think the assumption that you are forced to make here is that the entanglement rate is uniform across the domain until you have better information.

MR. CARMICHAEL: Yes; and I am not questioning at all the assumption. That is obvious, depending on the behavior, and there are different entanglement rates; but I was just sort of saying I wasn't under the impression up to this point we were assuming that what we're measuring is somehow a proxy for entanglement rates. I was just making a point that that seems kind of a leap given that we're just talking about sort of encounters. I think that is maybe a critical point to consider.

DR. BERKSON: To that point; I agree. I thought all along we were talking about this concept of co-occurrence and relative co-occurrence that is sort of unitless. Now we're talking about a proxy. I think that is one step beyond where this needs to go potentially and where it can go given the analysis. I think this might be an over-inference, you know, an inference beyond what the model is capable of. When that is brought up in slide 45, I am a little concerned.

DR. FARMER: To that point; the purpose of the right whale portion of this analysis is to get a sense of what is the relative risk to right whales. The risk implies that there is some kind of entanglement happening, right? We can't quantify that entanglement rate; but the overlap, of course, has to be a proxy for some kind of risk; and the risk by definition for black sea bass pots and right whales, that interaction that you're worried about is entanglement. Yes, it is assuming implicitly that there is some sort of background unquantifiable entanglement rate that is fixed across that space.

DR. JOHNSON: Yes, I think that may be the case, but I think what we run the risk of is playing sort of telephone with this. You hear entanglement rates in one area and then somewhere on here we hear 100 percent risk of entanglement. One hundred percent to me means, at least some people, probabilistically that it is guaranteed to happen.

I think it gets back to Jim's point of maybe taking away the percentages and starting at 10 and going to zero, so it is really more of a relative scale than something that people can latch onto and say 100 percent. I think it sort of gets in if you keep moving down the line we hear several different things and people read more into it than they should.

DR. FARMER: I certainly agree with Jim's suggestion there on making it unitless. It can be a scale from zero to 100, but not be a percentage; because the entanglement rate underneath all this is unknown.

DR. BARBIERI: Exactly.

DR. FARMER: One of the things that the Science Center wanted us to kind of get into more detail on – and we had provided a fair amount, but we have added some now – is with regard to effort shifting and the potential for that. If you look at these alternatives, there are a few

alternatives that have some opportunities for black sea bass fishing outside of the closed area. There are not a lot of areas that are available, but there are some.

Most areas that are remaining open haven't been fished November through April for five-plus years, so it is hard to predict what the catch rates would be in those areas. It is also difficult to determine how much effort, if any, might shift to those open areas outside of the closure, which of those open areas might receive new effort, whether fishing opportunities even exist in those areas beyond the closure, and what catch rates might be in those areas.

I felt that effort shifting was relatively unlikely because the ACL is projected to be reached. I also thought that the fuel cost to get 10 to 60 miles offshore might be a disincentive to upping your effort during that time period under a thousand pound trip limit. If effort shifting happens and it is hard to quantify what would happen, but it is pretty easy to speak to the directionality of its impacts. If it happens, if the shift happens, catch rates are underestimated, closures would happen sooner than are projected, the relative risk would increase in those open areas, but your overall entanglement risk could go up or it could go down.

The reason for that is you might have your increased risk in those open areas, but you are also having a catch rate that is higher than predicted because you've shifted effort and they are fishing more. You might have an earlier closure date, which might cancel out that increased risk. You get five, six days, for example, of increased risk outside of a closed area but then your quota closure happens sooner than it would have otherwise; and so your overall total for your unitless risk number might actually be about the same.

This is probably more applicable November through December, which is after Alternative 2 would be closed. It is probably most applicable in terms of concern for Alternative 4 off of Charleston, and I've got a little zoom in of one of the spatial distribution of trap gear effort figures. Alternative 4 is the green bar there off of Charleston.

You can see that there is actually some fishing pressure outside of Alternative 4 off Charleston's coast. It is also a concern for Alternatives 3, 4, 6, and 8 off of Daytona Beach, which all have some opportunities for fishing. You can see that Alternative 5 down there and then Alternative 7 is even farther offshore, and there is not a lot of fishing that takes place outside of those.

There may be some areas where there are opportunities for increased catch rates of black sea bass through effort shifting. Then that is the end of my presentation; and this is a right whale that was photographed, I believe, in 2009. You can see that there is some line on it there, which was determined to be pot gear that had been run through a tiller. It could not be identified to fishery because the gear wasn't marked during that time period.

DR. BARBIERI: First of all, thank you, Nick; this was very thorough and very detailed. You had the patience to bear with us. I thought it was so much better to have this question-and-answer period throughout the presentation, especially for such a long and intense presentation. It helps people understand things as we move along. I appreciate you being patient with us on that. Are there any other general questions before we start addressing the specific topics of our action items; any specific questions?

DR. SEDBERRY: I was just wondering if we had any data on – I think there are like 32 endorsements and they are allowed to carry 35 traps – how many of those endorsements are actually fishing and how many traps are they actually using?

DR. FARMER: We have data on that. I don't have it off the top of my head, but that is something that we could provide if needed.

DR. BARBIERI: I think Myra had that in her presentation, right, Myra. No it is one of those things, but it is relevant to this discussion to bring it up again.

DR. CROSSON: Well, it is not the number of traps; it is the number of trap lines that are going up, right, because all of the ones that are along the bottom are not –

DR. SEDBERRY: They are not allowed to use trawls.

DR. CROSSON: Right, but the line that is going to the buoy is the one that is the danger. Okay, I'm sorry; I'm getting it confused with another fishery.

DR. FARMER: In speaking with the Science Center in doing these computations on soak time; one of the things that emerged is the latest kind of fishing practice in black sea bass for the most part is one trap line per trap. Historically, there were some guys that did use trap trawls; but with the trap limit in place and some of these new regulations, most of the guys have switched over to one line.

DR. REICHERET: Remind me; the way the gear is fished nowadays, which is my understanding is slightly different from how it was fished in the past; was that included in some of the risk calculations? It is my understanding, but Ben or someone else can fill us in. The fishermen, because they are no longer allowed to soak overnight, that the fishermen stay with the traps, which could potentially play a role in the risk of entanglement.

DR. FARMER: Again, the comparisons are relative; so if there is a fisherman out there who is actually sitting on top of his gear and is going to remove his gear from the water if he sees a right whale, for example; I know we didn't account for that sort of behavioral dynamic. But, yes, we did account for the changes in the fishery. With regards to the trap limit, the trip limit, the endorsement holders are only the ones being considered.

Then also the soak times are coming from the most recent fishing season, which best reflects the new behavioral practices of the fishermen under these new regulations. The soak times now are shorter. Like if we had used the winter 2008/09 soak times, which we couldn't because they were poorly reported because of a misunderstanding on how to report them; but from what I have heard anecdotally they were longer than the current soak times; but we don't have any data to really support or deny that anecdotal information.

DR. MacLAUCHLIN: Just a comment on that; I think that the Regional Office could probably provide the real numbers; but of those 32 permits that are valid or the endorsements, they are likely all actively being fished even if somebody is like taking some time off, because they are worth from what we hear like $20,000. If you are not going to fish those then you probably would consider selling it.

If you are not going to fish for an extended period, so probably they are all active. Then also in 18A, when we were looking at the cap on the number of traps, most of the people that got the endorsements – actually all of the people that got the endorsement, in general their mean number of traps that they were fishing was around 15 to 20.

When we were doing that analysis and talking to the people who were only fishing 15 to 20, when there were other people who were fishing even up to 100 traps, they said in areas that are productive and fishermen who are experienced and very efficient, they only need 15 to 20 to make it worth their while. In general, it may be that is the maxim number of traps that will be in the water; but just from the data before that we used for 18A, it suggests that actually may be very high for what they are actually fishing.

DR. ERRIGO: I have numbers using data from 18A – I do not have current numbers – but it shows all the traps and the average number of trips that each person took during a year and the average number of traps they fished during their trips before and after 18A. If you want to see that, I have that. I tried to break it down by latitude to look at the areas where right whales were more likely to be seen based on the analysis that Nick did, but it is coarse.

DR. BUCKEL: That is exactly what I wanted to see, Marcel's and Mike's question about looking at this relative risk of co-occurrence using the prior effort, what was happening in the winters 2008'09 before compared to the recent amount of effort with the 32 permit holders.

DR. BARBIERI: Are there any other questions? If not, then let me draw your attention to the list of action items; and we have only two for this agenda item: review interactions analysis approach. We are basically doing an evaluation of the methodological approach for this analysis. We recommend whether the methods represent the best scientific information available.

MR. CARMICHAEL: I just say that is obviously kind of general; so I think we're open maybe to some discussion about how you want to go through and handle it. A couple of things that emerged during discussion I thought might be helpful in that direction; Scott at one point kind of once said these are some of the things we know; listing out some of the things you know.

Jim brought up some of the things about different uncertainties that might not be explored in here that may be important. I think maybe focusing on those two themes might help you maybe come up with a list of suggestions and help you get to your final conclusion.

DR. BARBIERI: To that point, Mike had already captured some of those comments. All we have to do perhaps is now, as John mentioned, start fleshing out some of that and bringing a few points that we can add to those that are already there or add some additional justification or points that we want to make so we can develop that narrative for our report.

Jim, let me put you on the spot since everybody is still sort of digesting this whole thing. Since you had brought up that very good comment about the uncertainties, can you look at those points there that Mike put up and see if those capture what you wanted to present or if you have something to add that would refine that?

DR. BERKSON: Yes, on minor changes for the first bullet, model structure I view different than the model parameters involved.

DR. BARBIERI: Is that relative to that first black bullet there?

DR. BERKSON: Yes; I wouldn't change that entire thing. This is wordsmithing, but it is a little bit more than wordsmithing, because model structure involves the parameters that are included and the functional relationships between the parameters that are included. That makes it sound like smaller changes the way it was written and like if the parameters were the only factor within the model structure that needed to be considered.

I would word it in terms of within model uncertainty, since what has been compared is between – yes, within model run uncertainty rather than between or among. I think those are good starts. I am just now reading the Gowan and Ortega-Ortiz paper, trying to go through there. It is a nice little one to read on the fly. I'm trying to get a sense as to the validation that was done and whether alternative models should be considered for that.

DR. SMITH: I really appreciate Jim's comment regarding this aspect of uncertainty. I guess what concerns me in particular is I don't think there was any accounting for parameter uncertainty specifically. It is hard for us to assess exactly how great that parameter uncertainty might be. It might be minor, it might be huge; but one thing we could do is go back to the Gowan paper.

There is not a lot of description of that in the paper; but if we go to Figure 4, they've got some prediction intervals. I'm looking at the prediction intervals, and they are pretty wide, and I suspect there may be considerable parameter uncertainty particularly in detection probabilities. I mean detection probability for a rare animal that is rarely at the surface is probably pretty tiny with a gigantic amount of uncertainty.

That would have to propagate through the model into these relative risk values. Again, it might propagate through in a small minor way or it could be massive; and it is that possibility that it could be massive that I think is giving us a little bit of concern.

DR. BERKSON: Everybody is wording this so much better than I have; so thank you all. I am beating the dead horse again, but what this study has done is it has assumed the best case scenario from the Gowan model is true. That would not be my starting point if I was doing something as complex as trying to look at probability of co-occurrence with right whales.

DR. FARMER: Just relative to that; I don't know; again, it is the wordsmithing, but the best case scenario from the Gowan model; I think, yes, we've got the mean output, mean conditions with the means as being parameter scenario; but then we also have two sensitivity runs on the parameter that has the biggest impact on right whale distribution.

The only parameter of uncertainty that is explicitly mentioned in the Plos One paper as being a big driver of a different spatial distribution of right whales; so there is some uncertainty captured there, it is not just the best case. I guess another thing I wonder about – and certainly this is something that I will need to speak with Tim about – is if you tweak an input parameter to the model and you're sighting levels go up or down depending on that; if they go up or down in a uniform way across the spatial domain; again when you divide through in a relative way, because we don't have an entanglement rate on top of this and this isn't a prediction of population size; it

is a prediction of sighting probability that is underneath it; I don't think it is going to have an impact on the relative scales.

Again, I am still under the impression – and certainly we can look into it if the Gowan model will permit us to do so; but I am under the impression that the broadest portion of the uncertainty has been captured by the bookends that have been done already.

MR. CARMICHAEL: You said the influential one; which parameter is that?

DR. FARMER: I think that is going to be the sea surface temperature, warm season versus cold, which also, of course, is interacting with the distance to the 22 degree isotherm, which is basically the edge of the Gulf Stream is what that is a proxy for, because that is a thermal tolerance limit for the right whale.

DR. BARBIERI: Nick, can go back up to Slide 39. That is basically in terms of the outcome of the analysis. This is sort of the bottom line that summarizes. I think that these results are going to then be presented to the council, and they have in sort of a ranked way of what is there. I think it might help the council – Nick tried to do some of this himself, but I think it would be very helpful for the council seeing this and getting some of our comments about the uncertainty, but also focus then on the strengths and weaknesses. We already have some comments there that identify some of those issues of how much confidence can they have in this output and what is the probability of error in some of those.

DR. BERKSON: Overall my concern about the model, looking at the big picture, I am interested in all the uncertainty stuff that I keep bringing up to better understand how the model works and how strongly we can say what we want to say coming out of that model. My bigger concern is that we don't overstate what the model can tell us; that we're not over-inferring or going two or three steps beyond what we're getting out of that model.

For instance, that percentage thing – I know Nick and everybody said we're getting rid of the percentage thing; it is not going to say 100 percent; but there are a few other things throughout that we just need to make sure, I think double check that we're not overstating. If we're all on board with that, I think the concept of relative differences work and that is probably a reasonable way to go with this analysis. I would still eventually like to see that additional uncertainty analysis done. Myself, I wouldn't necessarily think that the presentation of this would be dependent on that; but I am interested to hear what other folks have to say.

DR. REICHERT: My remark was just the percentages here, but we talked about that.

DR. SCHUELLER: I guess I am just still concerned because if you look at this and if you take Jim's recommendation and those percentages, whatever we're going to use, they expand, you may not get the same rank order or you may get 6, 4, 8A, and 3 are all the same, really, if you looked at the broad range of uncertainty.

Ultimately we're giving advice on choosing an alternative. This analysis is being used by the council to choose an alternative. If we're not providing that full range of uncertainty for them to choose so that they know maybe Alternative 6 and 8A aren't as different as it might show here or is implied; we're misleading them.

DR. BARBIERI: Amy; that is an excellent point. I think it would be good to explicitly in our report have a bullet that we can flesh out later that basically says – this is why to me I was focusing on this Slide 39, because it was like this is the bottom line result that they are going to get.

Then if we can tell them, listen, the models are models so they are just representations of reality and this is a fairly complex set of models, and then the analysis that was done here provides a lot of valuable results, but be cautious with the order there, because some of the uncertainties are not being taken into account perhaps as explicitly as we would like to see them be taken into account and look at this ranking with a grain of salt, because there might be differences in ranking given difference in areas of uncertainty being included into the analysis.

DR. BERKSON: I'm assuming that this analysis wouldn't just be used by the council; but if this was viewed as a major change in the fishing season or the fishing pattern or whatever; that this analyses would be input to the biological opinion. We're not just talking one of many pieces of information going to the council. We're talking about a key analysis going into the biological opinion,

DR. FARMER: Yes; and during the break I received some info from Barb Zoodsma and then also from Lance Garrison at the Science Center with regards to their availability to speak to you guys now or via some other form of communication that would be acceptable to you with regards to both the limitations of the input data or how it is collected and also to the dynamics of a biological opinion and how this analysis might be factored in there. They are standing by. I don't know how you guys would like to handle that; but if you want communication from them, it is available.

DR. BARBIERI: Thanks Nick. I think it is whether the committee members or any of the committee members would have any specific questions that would help clarify some of this review of the analysis that goes above and beyond what Nick as a fishery biologist is able to address.

DR. REICHERT: I just asked Myra; and I believe we do have another opportunity to look at this before the council makes its final decision. That potentially may help in terms of getting some additional information to us that we can then possibly use the next time we discuss this.

MR. CARMICHAEL: Definitely make a list of what additional information might be; just don't make it general. One thing I was thinking I wanted to ask Nick and maybe Myra or Gregg about the timing; and maybe it is just going to be semantics; but you talked about exploring more of the within model uncertainties up there.

That somewhat suggests Nick going back and doing some more work. I am wondering if that is practical; because if not, then our discussion should really be more about what are some additional uncertainties and caveats that might need to go with this to the council? Is there time in which you could do any of this stuff realistically?

DR. FARMER: In terms of speaking to staff time, the way that the model is designed right now, it is relatively easy for me to do runs on the mean model outputs by year with the data that is input in there. If you're just looking for a range of what the mean model estimates are based on

different temperature dynamic conditions and that sort of thing; like basically each year has the model input from that year and semi-monthly period; and it is the best possible model fit for that semimonthly period based on what was observed at that time; I have all that. The big kind of time suck for this thing is taking Tim's outputs from his model and getting them into GIS and slicing them by all the alternatives and overlaying the fishing pressure.

All that structure is done for that set of outputs from '04 through 2013 for the Florida/South Carolina in semi-monthly periods. I don't think you are going to get any different range out of that because the warm and the cold are the brackets for the mean out of that range. If we have to go back and actually refit Dr. Gowan's model or take the lower confidence limits or the upper confidence limits or some sort of Monte Carlos simulation on the input parameters themselves; A, I don't know if he is capable of doing that with the way his model is designed, and I don't know how long it would take him to do that.

He obviously doesn't work for the Regional Office; he is with FWC. I don't know what his priorities are, but I know he is really busy because even getting this was a hard thing to do. It was really great cooperation from him to get to this point. I think that seems to be one of the big drivers of kind of what you're looking for because the rest of the stuff – and I want to speak with you more during a break, Jim, about how we might be able to handle any of the other internal model uncertainty with regards to where fishing effort is going to be and how you would bracket uncertainty around that and that sort of thing, and the catch rates within each catch rate scenario and how you would bracket uncertainty on that. With Dr. Gowan's stuff, that would be a real time drain.

DR. BERKSON: I'd rather not individualize it to being what I want. I think we've heard other people around the table agree with these points. I might have brought it up first or the most times, but I'm not the only person to bring it up.

DR. FARMER: Well, I guess then to clarify, just to know more about what it is that folks are looking at; and maybe I can talk some more off on the side as to how the model is constructed, so that we can try to get a vision of what it is exactly that we're talking about. Like I said, other than Gowan's portion, these aren't regression models underneath, so they don't have input parameters, per se. It is scenarios of observed data from the past and present.

DR. BARBIERI: Gregg has some clarification on the time.

MR. WAUGH: Yes, in answer to the question about the council's timing, the schedule is for Nick to give a similar presentation to the council in December. The council is scheduled to approve it for public hearings. We've already got some public hearings schedule in January. Then they will review the public hearing input in March and hopefully select their preferreds if they haven't. Then the AP will review it in April and you all will review it in April. Then they are currently schedule to give final approval in June.

DR. BELCHER: I guess one of the things that I think would be helpful is I know we've said that there really isn't an idea of what the entanglement rate is; but in looking at the biological opinion snippet that John sent around, it indicates that there hasn't been any – at the time that they did it, it says that there are no documented entanglements or other interactions between black sea bass pot gear and right whales, and they site Pothenberger 2004.

Again, not knowing what the end of the timeline was and knowing that trap closures didn't come in until 2013; has there been anything in that time interval that would lead us to think any differently in terms of interactions with black sea bass pots? When you come down through like the snippet that we're looking at; the last statement is lack of evidence suggesting interactions between black sea bass pots and marine mammals.

The proposed provisions under the amendment to the Atlantic Large Whale Take Reduction Plan leads us to conclude that any adverse affects resulting from the continued authorization of snapper grouper fisheries are extremely unlikely to occur and are discountable. What has changed that we're kind of revisiting?

I guess that is more of the question. If it was considered negligible then, is there a reason to assume that we're above negligible now? Knowing that there, what; I am assuming if it is 2006, say 2004 was the last year of data; I don't know, but that is nine years worth of data with traps in the water during that time interval.

DR. FARMER: The one study that I am most familiar with, but I am sure it is not the only one, and the report has some more detail, is that Johnson et al 2005 study, which was able to identify some black sea bass – sorry, not black sea bass, but some entanglements of right whales to gear types; and of those 71 percent were pot gear.

You couldn't identify it to fishery at that point, but the gear is very similar to or identical to black sea bass pot gear. I know a priority item of the Atlantic Large Whale Take Reduction Team is to reduce the presence of vertical line entanglement risks to large whales. This also keep in mind also includes hump back whales and fin whales, which are also observed in the area.

I've got some slides on those, but there is no model underlying that sightings' data, but we can look at those if you want. But again that is a question that could be answered right now if we could get Dr. Garrison or Barb Zoodsma on the phone. I am not qualified to answer that question in any more detail than I have just provided.

DR. BELCHER: But again, to me, it just seems like the analysis; if the probability is low to zero, are we really talking about – the potential is not is the relative risk above zero? That is the ethereal question I have to ask is are we arguing about a moot point if the interactions are still considered to be zero to negligible and we have less gear? It seems like there is a lot more built into it.

DR. FARMER: Again, I am not a protected resources biologist, but I think I can speak to whether or not it is zero; and I would say that their assumption is that it is not zero. Otherwise, we wouldn't be here.

DR. BELCHER: Well, but semantics. Negligible to me says close to zero. I understand it is not going to be perfectly zero.

DR. BARBIERI: Just another clarification, because again it is very difficult for us not to go there; because when we review, for example, a stock assessment, we look at the assessed methodologies and the model fits and all the diagnostics that we usually use, but we also look at

the final outcome; do we have reliable estimates of the reference points for which we can compare model outputs to the estimated reference points?

In this case we are not being asked to really judge which one of these alternatives will be best or whether they is a bar in terms of whale conservation, because Protected Species handles that under a framework that is more in line with Endangered Species Act, and it is a different bar than what we can –

DR. BELCHER: But I guess I was getting more at the point of this unknown entanglement rate. They basically asserted that they have assumed that there is low to negligible entanglement rate. My question is, is there reason to assume it is higher or different than zero? They are inferring that there is no risk. They have obviously issued us a biological opinion saying that. My question is has that changed to say that it is quantitative above that? Is there information to say that there is something that we could at least assume is the entanglement rate? That was my only point.

MR. CARMICHAEL: It seems to me that is a really complicated question and it is beyond this analysis, which attempts to rank the alternatives before the council and which I think it would be good to deal with this and then we can talk about that real world number, which is critical to all of this; and consider if you want to get a presentation from the people that know about the whales and have them come to you and talk about that at the next meeting when this gets closer to the very end.

DR. SMITH: I think we may have moved on to a different topic; but to go back to the potential timeline to account for the parameter uncertainty things that we were talking about; my understanding of the projection model that you presented is that it is a deterministic model. I think what we're talking about is a stochastic projection and sort of an MCMC framework.

I suspect that is going to take considerable time to build that into the model, because we're talking about a whole new layer to the model that is not even there right now. I suspect that the timeline could be considerable. I just wanted to offer that insight.

DR. FARMER: Yes; this is the model that runs in SAS first; then it takes input from Gowan in Excel, and then it goes into GIS, then it goes back into Excel. There are a lot of interfaces. It is not just like I tweak a parameter at the top of the code and run it again kind of a thing. Anytime we get spatial analysis involved, things start to get really complicated and take a lot longer.

DR. BUCKEL: To the point Nick mentioned 71 percent of entanglements were pot gear; I just pulled up the Johnson Et Al paper, so that is true; but then it says for pot gear, 80 percent of right whale entanglements occurred in lobster pot gear. It looks like that is the gear.

Just to add to that; in the document which we were talking about this interaction analysis; when you are talking about this information that could allude to probability of entanglement, developing that a little bit more. Instead of just saying 71 percent pot gear, actually say that the bulk of what Johnson found was lobster pot gear would be helpful to the council. You mentioned a presentation from others that might be able to provide information on this. If there is information out there, it should be in this interaction analysis document.

DR. BERKSON: Yes, just a suggestion for Mike, who is doing the typing there, you say that the idea of doing an MCMC is not feasible? I would put not feasible given current resources, because it is feasible if we had more time or another programmer or whatever.

DR. BARBIERI: I think we have captured quite a bit. I am looking at since you are the one who writes this thing to make sure that you are happy in terms of – well, because your responsibility is to make sure that this committee produces the types of comments, analysis and recommendations that will be helpful to the council as it looks more into this.

I thought that in terms of review of the interactions analysis approach, I think we've got a lot of very good suggestions. I think it was great to get the timeline clarification, because Nick might be able to, working with others, refine this analysis a bit more; maybe not if that is the case. It happens to us all the time. But then there is this issue of recommending whether the methods represent best scientific information available. My interpretation at this point, John, is that because we have made so many recommendations, this is still potentially in flux. I don't think we are at the point of necessarily crossing that bridge yet; are we?

MR. CARMICHAEL: I don't know; and I definitely think given the importance of this and the amount of discussion, it would be incredibly important to have a clear statement that sort of wraps this up. At one point we started sort of with the strengths and weaknesses. Jim made the point that he felt the concept of the relative differences works and is a useful tool.

I guess the primary concerns, and there are a lot of details about them, but I think what sort of summed them up is being careful not to overstate what the model really represents; and really concern about whether or not the full uncertainties are reflected and particularly does that limit your ability to truly distinguish between alternatives?

I think it really comes down to that. Then the question is do you think that those couple of concerns is big enough that you would recommend to the council that this isn't quite ready for you to use to potentially pick a preferred alternative from amongst these. Do you think it is as good as they are likely to get and they are going to have to weigh the risk themselves?

DR. BERKSON: Well, that is tough. It is easy to clean it up so that things aren't overstated. I don't think a lot has to be done. There is no intentional effort to have done that in the first place, so it is just a little wordsmithing and number-smithing. In terms of the uncertainties; do they prevent us from seeing clear differences between alternatives; I don't know that we're going to know that until we do an analysis.

I don't think you have to do an analysis on every option to get a sense of that. I think you could do it on two or three that are close, two or three that are far apart. That is the kind of thing where if we brainstorm a little bit, I think we can be very selective about what we picked and try to get a sense.

Once again, you are trying to get a sense for the dynamics of the model and how the model works. I think a subcommittee of us or just a phone call or whatever and we can brainstorm that away. Nick could provide 70 percent of the input on that or 80 percent and we would take it from there; but I don't know that we can predict that without seeing more work.

MR. CARMICHAEL: It sounds like if you can find a way to determine the degree to which you can distinguish between these alternatives, then it seems like the tools will be useful to the council; and there are some members here willing to work with Nick in the next few weeks to try and maybe pick a couple test case scenarios to explore that.

DR. BARBIERI: Nick, thank you so much. I think that this was very, very interesting and invaluable discussion. Hopefully, we were able to provide some good comments to refine this and get an even better outcome.

MR. CARMICHAEL: One last follow-up for a theme that sort of floated around is the idea of better understanding what the real world level of interactions and entanglements and all of that risk really is. Is this something that you guys want to have someone come and talk to you about and have a presentation on that and dig into that?

DR. BARBIERI: Let me just jump ahead here, because I think Carolyn made a good point. This is not in our terms of reference, so to speak here, to cross that bridge. I know that Nick wouldn't be necessarily comfortable doing this; but if we are going to see something like this, a follow-up in April, I think that having somebody come in and speak to that would be extremely helpful.

DR. BUCKEL: Two things that haven't been captured yet that maybe the group doesn't feel strongly about it anymore, but it seems like a couple folks wanted to see relative risk, so Alternative 1 now is zero, because it is closed, alternative 2 is 100 with it fully opened, but comparing that using that same scale; but then get the effort from pre-closure, so early 2000 when there were 200 some permit holders and folks used 100 pots and left them out multiple months, to see how that relative risk – what the positive relative risk would be compared to this to give an idea of where we've come from in terms of decreasing relative risk.

MR. CARMICHAEL: In a way I guess that is maybe like the third dimension of it to say now scale the past to the Alternative 2, if it is what you feel like you are interested in.

DR. BARBIERI: This touches on what you had mentioned as well; but to what Carolyn brought up about, okay, over all this time what has changed in terms of the fishery, the geographic, and the effort that makes those likelihoods now be very different.

DR. BERKSON: Maybe this is what you were suggesting. I mean ideally I would like to hear a presentation from the Protected Resources folks about what is it they look for in an analysis like this, because we're not used to seeing that. I am guessing that even though it would be fascinating to see how the risk changed from 2000 until now, and I would like to see that; but they aren't going to care about that because what they are going to care about are the relative risks between options being considered now.

Once again, if we're talking about one in a billion versus one in a million odds and it is a value judgment as to what is too much; how is that decision made? There are a lot of things about this that are just foreign to me, and I am assuming foreign to most of us. If we are going to be facing more of these cases, which I'm guessing we will, coral and everything else, it would be nice to see how it all sort of fits together.

DR. BARBIERI: I think that it would be good to have as comprehensive a presentation from them as we can get – and that point is well taken – and help us in communications with the Protected Resources Division, kind of articulate to them what kind of presentation the committee would like to see.

And we have a presentation from Mike that also provides some additional perspective and information. Considering the time, JC, I am feeling that breaking for lunch might be – no? We still have the whole bag limit analysis. There are two attachments. I am not going to be able to stay here through three o'clock, but the steady hands of Vice-Chairman Reichert will definitely be more than adequate to drive the bus at that point. I was thinking that if we are going to go through one o'clock in the afternoon; I don't know who is going to be giving the bag limit analysis and how much is there; but from the document that I saw, that is fairly complex. No, it's short? Okay.

DR. CROSSON: Well, if we do do it, then it is ten to twelve right now, so I think maybe we can try and do it a little bit more compressed, grab something and come back rather than an hour plus or whatever we have been doing. I have a flight this afternoon at four as well, so I am not going to be here much past 2:30.

DR. REICHERT: Or the alternative, if w feel we can wrap this up by one, have a late lunch and then wrap the meeting up. I'm not sure if we can complete the rest of the agenda in the next hour.

DR. BARBIERI: Let's take a five-minute break.

DR. ERRIGO: I just want to make sure I captured that right. You wanted to hopefully have someone from Protected Resources come to the next meeting to present on like how they do their biological opinion, how critical decisions are made, how this analysis will be used.

DR. BARBIERI: If I could ask members to return to the table, so we can proceed with our analysis. We I guess unanimously agreed that we would plow through until about 1:30, earlier if at all possible, and go through our remaining agenda items. Bag limits.

DR. FARMER: This is an overview of recreational bag limit analytical methods. I have got to give a lot of credit to Dr. Jessica Stevens for preparing this presentation, so thanks, Jessica, for all your hard work on this. She did the bag limits that we'll be discussing in specific after this as well. These are methods that the Regional Office has been using, initially developed in collaboration with SERO and the Science Center well over a decade ago, long before I showed up.

We'll run through kind of how we work on this. With regards to the bag limit, we're basically simulating the effect of a bag limit by estimating the percent reduction or increase in overall harvest. It has been approved previously by the Gulf SSC as the best available scientific method. The goal of this presentation is to give you an opportunity to review the methods and report some guidance on the types of info that should be reported.

Our data sources here are catch effort files from headboat and MRIP and then also the Science Centers ACL landings' files, which are used to monitor the ACLs and try to prevent quota

overages. Those are put together and create a model. The first data source there is the Southeast Headboat Survey Logbooks; and historically those were paper based; now they are electronic.

I've worked with the headboat staff on some GIS mapping to improve the spatial reporting of that data. It is captain-reported, catch and effort data at a trip level, and then it gets expanded for unreported trips. Basically what the headboat guys do is they audit the office records for the headboat survey participants; and if a trip is recorded as having been taken but they didn't report it in their captain logbook, the headboat program will expand those based on the best possible proxy.

There are proxies both within the same vessel and month; and then if that is not available, they will use a similar vessel of similar size in a similar area to expand to account for those unreported trips. Those are your K factors. For the headboat analysis for bag limits, we use the K factor file to expand the data for estimates of unreported trips on a per vessel basis.

Then that expands both the number of anglers and the fish caught, so you can see a little graphic on the bottom; five fish times K equals seven fish. That is kind of how that accounts for it. With regards to the MRIP survey, there is dockside intercepts for the catch data. You are all pretty familiar with MRIP, I assume, at this point.

They collect data by fishing mode, charter, private and shore in two-month waves, and the catch is calculated per person. For the bag limit analysis, we're using harvested catch brought to shore whole, which is the A, and then the B1 catch, angler-reported harvested catch. There is also the B2, which is angler-reported release catch.

The MRIP adjustments for the bag limit analyses; what they do is they inflate the B1 and B2 values by the missed interviews; so if the number of anglers interviewed is less than the number of anglers that are listed as contributing to the catch of the B1s or B2s; that is multiplied by the ratio of contributing to interviewed parties.

Basically you are accounting for the fact that maybe the intercept guy went out and he interviewed four of the five people that were on the boat, so you are accounting for that catch from the fifth individual. The ACL landings file is a file that we used to monitor the ACLs. It is maintained by the SEFSC in Miami. It integrates the headboat and MRIP landings' data.

It provides post-stratification for Florida and North Carolina by species when it is applicable, so we've got some stocks where we assign the landings from MRIP that are from Monroe County by default assigned to the Gulf of Mexico. We reassign those to the South Atlantic to reflect the fact that most of those landings come from the Keys Reef Track, which is in the SAFMC's jurisdiction.

They also provide some hole-filling for average weight estimates when MRIP doesn't provide an average weight. Sometimes you'll get catch number that is higher than zero, but there is no average weight assigned, so they use a sample size minimum threshold of 30 to backfill those average weights. With regards to the post stratification, that happens in Monroe County and North Carolina.

There is a list of species there that are currently post-stratified, the newest additions being snowy grouper and blueline tilefish. The post-stratification is handled by SEFSC in Miami. We get these MRIP catch effort files from them. Those are input into a SAS code that was developed in collaboration with the Science Center a long time ago, and it is species-specific bag limit analysis file.

Basically the way that works is you get your catch effort data, you make your adjustments both for uninterviewed anglers in MRIP and your expansion factor for headboat. You break out your bag limit analysis when appropriate by mode, wave or month and trip. As you are going through these files, basically what you're looking at is landings per angler.

For MRIP that is based on the anglers contributing; for headboat it is based on the anglers after the K factor is applied. You look at your landings. Again, it is the A plus B1 for MRIP, and the landings post K factor for headboat. You compute the number of landings in numbers relative to anglers.

To model a bag limit decrease, basically you look at status quo being the current landings. You create fields for the various bag limits on a per trip level. If the landings per angler are greater than the bag limit, then the bag limit landings in the file is reset to the bag limit times the number of anglers. If landings per angler is three and the bag limit is two, then the new landings become two; and then times the number of anglers.

Then you tabulate you tabulate your landings by year and then month and wave. You calculate the percent change from the status quo. Then you have to aggregate the data sometimes in order to ensure that you meet a minimum threshold for sample size, because some of these waves you will have one fish, sometimes zero in terms of intercepts, so you aggregate across waves.

Sample size adjustments; we try to make sure that there is at least 30 fish or at least three trips per month in wave; else, we aggregate with surrounding time periods. For example, if wave 2 had low sample size, we would grab both the data from wave 1 and the data from wave 3 just in case there is a seasonal trend there; because if we always grabbed wave 1, maybe we would underestimate.

If we always grabbed wave 3, maybe we would overestimate whatever, so we grab both surrounding waves until we hit our sample size of 30 or above. With the ACL landings' data, what we do is we take the total landings by year, mode, month and wave. We post-stratify if needed and then we convert those wave landings to monthly landings by mode.

We basically just assume that landings within a wave occur uniformly across the days, and so then you get your monthly landings from that. Now obviously we make adjustments if there is a seasonal closure somewhere within the wave; so, for example, if red snapper opens in June and you have a wave with landings from May to June, then all the landings for red snapper get assigned to the June portion of that month.

Then we convert those monthly landings to catch per day by mode, and that catch per day is equal to landings divided by the days for month and mode. Then we create a prediction model after that, and it is a per day landings' prediction. Under status quo you just sum the status quo landings per day by mode and month for each day.

For the bag limits you do the sum of the percent reduction for mode and month under that bag limit times the status quo landings per day for that mode and month. Then you get a cumulative sum over time until you reach the ACL. Your prediction outputs include the closure date, days open, landings, and the percentage of the quota that has been achieved.

Other information is that we typically use the most recent data available; typically it is the last one to three years. We check for differences between years. If there are substantial differences, we report both. We provide tables by month and mode for the percent reduction from status quo, and we also provide the sample sizes, trip and the number of fish caught. For sensitivity runs, sometimes we look at the difference over different years to evaluate the sensitivity of the results.

Then for a bag limit increase, it is kind of the inverse of the process I just described, so your status quo equals the landings. If your landings per angler are greater than or equal to the status quo, then you increase to the new bag limit. This assumes that only those who caught the previous bag limit will catch the new bag limit.

Obviously, if you caught two fish in the past, that doesn't necessarily mean you would catch three. This represents a worse case scenario. It is intended to bracket the range of possibilities given that angler behavior is difficult to predict and their ability to catch a higher bag limit is not really easily driven out of the data. You have to make this assumption and then basically it is going to just give you the range of possibilities of what could happen.

For South Atlantic recreational gag, a bag limit increase was modeled for a recent amendment, and this is the example coming out of that. Right now recreational gag under status quo was part of the aggregate grouper bag limit, which has 16 species in it. You have an aggregate bag limit of three fish per angler per day of which only one can be a gag or a black grouper per angler per day.

In this analysis, first we remove gag from the aggregate and then we predict the impacts of a two and three gag per angler day limit under four different ACL alternatives. You use the bag limit increased methodology, which I described previously, and you investigate both the impacts as an aggregate and also on gag landings per angler.

This analysis used a combination of the 2012 and 2013 data. It didn't include previous years because the catch effort data is significantly different prior to 2012. There are really low sample sizes here, so there is a lot of aggregating that had to happen. For MRIP there were low numbers of trips, so it had to be aggregated across all months.

For headboat there was low catch, so it had to be aggregated across some months. There is a spawning closure January through April, so you assume zero landings in the prediction model. Basically you are assuming that there is no illegal retention by the recreational fishery during that January through April closure.

Here is an example of what the data looks like. You can see on the left the headboat data by month, and you can see that there are a very low number of landings; that is number of fish landed in 2012 and 2013 for the first four months. Those get aggregated, but then the assumption is that there are no landings during that time period, anyway, because that is during the closure.

For the MRIP private mode you can see that there is very low number of landings. Across all the months there are less than 30 fish, and so they are aggregated across the entire time period. Here is the output from that; here is the percentage increases to status quo by month. Your headboat, you've got by month for MRIP on the right there; you've got it aggregated across the whole year.

You can see for a bag limit of two, your biggest boosts are in June and November; for a bag limit of three again, June and November, and you can see that they're pretty proportionalized, because obviously right now the bag limit is one. You are just assuming that they catch two or assuming they catch three; so there is kind of a linear scale going on there. Due to extremely low sample sizes for the private and charter modes, the percent increases for MRIP are determined on the entire dataset.

You can see for private it goes up to 41 percent under a bag limit of two; and then it goes up 83 percent under a bag limit of three. For charter it goes up 27 percent for a bag limit of two and up 54 percent for a bag limit of three. Like I said, those are worse case scenarios, because you don't really have data beyond the bag limit.

The bag limit increase investigated also the aggregate bag composition from '09 through 2013. Aggregate trips that caught gag were 7 to 19 percent of trips under MRIP and 15 to 24 percent under headboat during that time period. Aggregate trips that caught gag or black grouper were 11 to 23 percent under MRIP and 18 to 29 percent under headboat.

Now, it is noteworthy that only two trips from '09 through 2013 reported catching both gag and black grouper. The gag bag limit increase is modeled as follows. The average landings per angler for gag under MRIP is less than or equal to 0.03. Under headboat it is less than or equal to 0.1, so very low landings per angler for this species.

The average landings per angler for gag and black grouper couldn't be computed for MRIP, because there weren't trips that caught both of them; and then under headboat it was 0.01 or less. Trips with gag landing per angler greater than or equal to one with MRIP is two to nine trips per year; so about 1 to 2 percent of all trips; under headboats, 6 to 19 trips per year, which is 0.1 to 0.4 percent of all intercepted or reported trips.

In looking at the trips that caught the aggregate bag limit for MRIP, you've got 3 to 16 trips per year that caught the aggregate bag limit, and that is across all those shallow water grouper species. That is 2 to 4 percent of all trips. Under headboat you've got 12 to 47 trips per year, which is 02 to 1 percent of all trips; so again not many trips catching this aggregate.

The top aggregate species caught in 2012 and 2013 for MRIP were blueline tilefish, red grouper, gag, scamp, and snowy grouper. For headboat it was blueline, scamp, gag, red grouper and sandtile fish. Here is what the cumulative landings' plot looks like under these different bag limits. Your status quo is the blue line there.

You can see you've got a closure through the end of April. Starting in May you start to get landings coming up, and that is cumulative fish landed, which is converted to gutted weight. There are different ACL options here that you can see. You've got a quota of 326,340 ranging down to 261,072 for this regulatory amendment. Your two-fish bag limit you can see is the yellow line there in the middle. The three-fish bag limit is the red line there at the top.

That kind of brackets the range of possibilities, assuming that people who caught one fish could go out and catch two, assuming that people who caught one fish could go out and catch three.

The gag bag limit increase comes out with this table here. You can see that you have four ACL alternatives there in the first column. You have your different bag limits in the second column, status quo, which is one within the aggregate; bag limit equals two and bag limit equals three, which are gag-specific.

Under none of these bag limit alternatives are you projected to hit the quota, so your closure date is just the end of the year for all of them. Your projected days open is 245 for all of them, and that is due to the seasonal closure at the beginning of the year. You can see your landings there, total landing relative to a percentage of the ACL in the final two columns.

At best, under the 80 percent ACL equals 80 percent of the ABC at 261,072 pounds gutted weight, you get 65 percent of the ACL assuming that everybody who caught one fish in the past can catch three fish under this new bag limit. The gag bag limit conclusions would be that increasing that bag limit will have a limited effect on landings, because few anglers are landing the existing bag limit.

Increasing that bag limit will have a limited effect on the aggregate because it is rarely caught; and the average catch per angler for any species in the aggregate is less than one. The gag limit is obviously rarely caught; and it is the third most commonly caught species in the aggregate. We also looked into why discards were happening.

We concluded that discarded gag was most likely discarded due to size limits or seasonal closures and not the bag limit that is currently in effect. Any questions on the general bag limit methods or gag bag limit in specific; else, I can move on to the blueline tilefish.

DR. BARBIERI: I don't know if you guys had a chance to look through our overview document – and perhaps JC can help me here with some of the idea of bringing this methodology before the SSC, some of the issues that we had to deal with in the past and some of our discussions while we are here.

MR. CARMICHAEL: It has been a while since you looked at a bag limit analysis. You looked at one for black sea bass some time ago; and I know quite a few of you that around then remember that. It was the situation where you got the documents about five minutes before the call started.

You made some rather pointed comments about that and about the inability to really judge the methods fairly given that amount of time to deal with it, but you were also aware that failure to act on it that time would have been essentially punishing the black sea bass fishermen, so you did what you could and you provided some general comments about that specific analysis.

But you sort of left the question of the general bag limit approach unanswered. Now that we don't spend as much time going over each and every amendment in its entirety through these meetings and we try to focus you more on specific analyses; when it came up about the blueline tilefish analysis, the question was that the SSC has approved it, I tried to go back and find where you had.

The only record of you guys commenting on a bag limit analysis in my time on the SSC was that black sea bass thing. I felt, well, to make sure we closed the loop up here and get a clear record, I wanted to give you this opportunity to talk about bag limit analyses in general and make sure you are comfortable with the methods. You are comfortable with how it is presented. You feel uncertainties are presented to the council properly and have some discussion about your role in dealing with these, because sometimes they tend to come up.

With the council meeting basically two meetings between your meetings, they can come up kind of quickly. In the case of blueline, we didn't have the opportunity to get that before you. That is something else we need to think about is the timing of the SSC's involvement on this and whether or not you want to review each and every bag limit analysis for a stock that may come up or whether you are comfortable with this general approach and it can go to the council with Science Center and Regional Office review and let it rip.

DR. BERKSON: Well, this has always been a pet peeve of mine; and John knows this. John and I were on the SSC 15 years ago. Over the years what we've seen is the stock assessment scientists; more and more pressure has been put on them to be transparent and to turn out report after report and page and page of results and analyses and everything else.

They go through peer reviews and the rest; but these analyses that then take that information and look at, okay, what regulations need to go in place because of those assessments, they were done on the back of envelopes and they had no review whatsoever for the longest time. Obviously, people worked hard on them and did their best with them.

But the amount of time spent on them and the amount of time with review, they weren't comparable in any way, shape or form. It is really nice to see this kind of presentation that shows what work has been done and allows us to review it and allows us to comment on it. I am pleasantly stunned.

DR. BARBIERI: Just another point of clarification to that before JB – and maybe JC can help me with that as well; but at some point in between you coming back – having been with the SSC before and coming back; this SSC made a decision not to see so many of these bag analyses, because basically our workload regarding stock assessments and some other analytical products really increased quite a bit. We had to deal with that control rule.

One of the issues is if we "certify" type of analysis, right now if the council staff or the Regional Office conduct, it is being reviewed by the Science Center; but if we see the methodology here and give our stamp of approval, then that can be referenced later on that same methodology and we don't have to see every single analysis until there is something that is done differently.

DR. BOREMAN: I am still wrestling with this pleasantly stunned comment. It sounds a little kinky. In terms of bringing all this analysis to the full SSC; is that necessary or can we like have a subcommittee of people who are really interested in this, maybe do it through a webinar offline, so we are not devoting valuable meeting time, because we only meet twice a year to this. That is another alternative, too, and it can be done sooner; quicker turnaround.

DR. BARBIERI: Right; and that is a very good suggestion that perhaps we can put that on the table and have the committee discuss and decide perhaps it doesn't have to be either/or. The

analysis will be taking place and they could be using certified methods, but we still have some SSC involvement that it can be first-hand involved in that to come and report.

DR. CROSSON: Make sure you appoint an SSC economist who perhaps specializes in recreational fisheries analysis who might not be in the room right now. I guess my question then is you've dropped something in there initially about the Gulf SSC having approved.

I know they have the more complicated situation there because I know some of their data sources are not MRIP; that they are using some recreational data that, as John pointed out, certain states have seceded and gone rogue. But generally the Gulf SSC, their regular SSC has approved this, and then this is how the process now works that the Gulf Council gets to see the actual analysis, but the methodology has been approved by the Gulf SSC overall?

DR. FARMER: My understanding – and this happened I think before my time, but a long while back this method was developed; and it really hasn't changed since then, and the files that go into it haven't changed since then. It really just takes these catch effort files and does what I've described; and so that was certified both by the Science Center and by the Gulf SSC.

Then whenever we do these analyses, they end up going to the Science Center as part of the amendment, typically either incorporated into the amendment or as a referenced appendix to the amendment. Then that, as a whole, is certified as best available scientific information at the end. There are multiple steps in that process.

You are right; the Gulf is more complicated. With their current bag limit stuff, we've got to get Louisiana survey data. We contact Mark Fisher over in Texas if we need a bag limit analysis from him, from the TPWD data, so it is definitely more complicated than this. But the fundamental point of the method is really pretty simple and hasn't changed. If it is equal to the bag limit, it goes down or it goes up, depending on whether you're modeling an increase or a decrease.

DR. BARBIERI: Just to understand the general thrust of what we're trying to get to here, unless there are other specific questions – sorry, Sherry.

DR. LARKIN: It is not a question; it is really a comment; because if we're moving towards this setting precedent moving into the future, this notion that if they can catch one fish they can catch three could have a whole suite of implications for what total landings are. It really assumes a lot on the socio-economic side; are folks going to sit out there on the water for how much ever longer it takes to get those additional fish?

It is simple, but I don't know if it fits with every species and with every fishery. I don't have a recommendation now, because it is thorough and laid out very well; but just moving forward, I would not sort of want to blanket sign off that we think that this is okay always, because there can be this whole suite of effects that happen after.

DR. FARMER: Obviously, a bag limit decrease is a heck of a lot easier to model than a bag limit increase in terms of having some form of certainty about what would happen, because it is actually informed by the data. Whereas, this other portion of it, the increase is not really informed by the data other than the fact that if they caught X they will catch Y.

That is a tenuous assumption; and the dubiousness of that assumption is outlined in pretty substantial qualitative detail throughout the amendment, both in the biological effects section and in the economic effects sections, because it is a hard assumption to make.

Especially for gag, you look at how few people catch one fish; it seems pretty dubious that they would be able to catch three unless they are sitting on top of a spawning aggregation, in which case I would like to know where it is.

DR. REICHERT: Echoing previous comments; you mentioned earlier that this is kind of a worse-case scenario, but may not be fully captured if there is potential change in behavior with this increase, which may affect the landings. I am not sure if you guys had an opportunity to look at this, although I know that is rather complicated.

DR. BOREMAN: I was going to basically mention the same thing. As long as you cast it as the worst case, because some fishermen are better than others; so 30 percent of the fishermen get 80 percent of the catch; so you know it is probably going to be less than what you have out there. But this is a typical problem that we have; especially dealing with species like striped bass and so on where you are developing conservation equivalencies and you are adjusting creel limits and so on. It is always an issue so it is a common issue; and the solution that you have is probably the best way to skin the cat.

DR. SEDBERRY: I was just wondering if we had any discard data that might indicate that maybe some of these extra two fish are fish that would formally be discarded but are now kept and that the release mortality might need to enter into that somehow?

DR. FARMER: Yes; and that is looked at, but for the gag in particular it appeared that those were size-limit-based discards rather than bag-limit-based discards. That is part of the equation right there.

DR. BARBIERI: By the way, for the socio-economists, I'm proud to say that even an old dog can learn new tricks. I guess through slides in Nick's presentation; the first thought that popped into my mind was like, well, where are the socio-economics here? You are learning me something.

DR. FARMER: The blueline presentation is next. I am not sure where that is in John's little folder here.

MR. CARMICHAEL: Let's circle back up to our action items and make sure that we're clear. The first one is about the approaches. You're comfortable and think it represents best scientific information. I take it you are happy with the methods and the results. The SSC role, we commented on having an ad hoc subcommittee review specific analysis, specific applications to specific species, so that we can do that timely and efficiently and not hold up the action, which I think is great.

Then we reviewed the gag and you provided some comments on there. I think that is good and I really like the idea of doing a subcommittee and include a sociologist/economist on there as well, because they do often bring a lot of very interesting insight into bag limit analyses in particular. Then I guess a question I have is I think it incumbent in the approach is the Science Center

review that goes into it; and I wonder if Science Center review comments should be part of what the subcommittee looks at, because we don't always see that.

I was thinking for transparency it would be nice to know what comments they made and just to be able to check off that they were addressed. I don't know when the Science Center review happens. My understanding is when someone prepares it, it goes to the Science Center for review before it would make its way to the council for consideration.

I see the SSC coming in between whenever the Regional Office is satisfied that it is ready for the council. When it comes to the council, it comes to the SSC first, because it is a technical analysis. It is either timing allows we do it here at a meeting; if not, we convene an ad hoc subcommittee, send it to you and hold a brief webinar to get your comments on it and share that with that everyone.

DR. BARBIERI: Well, I can tell you that in the Gulf it is really that step between IPT and SSC. It is supposed to go to the council and come to the SSC first; but when the IPT is satisfied that it has completed the integrated approach of the analysis, then it goes to the Center for that peer review process, and then comes back to the SSC for additional review.

MR. CARMICHAEL: The council has acted on blueline so we are not intending on looking at that.

DR. BARBIERI: Besides some of these suggestions and points that were captured by Mike, any additional input from the committee? I think we are really specifically being asked about this best scientific information available. It is not that this is going to be immutable. My understanding, if there is some change to this, that subcommittee that JB suggested can come back to us and say, listen, we need to look at this; there has been some change here, and it would be good for everybody to kind of take a look at this.

But for what was presented; that set of steps, the algorithm so to speak that they follow, which has been static for quite a while; do we have any major concerns where we consider this to be really the best that can be done at this point? If we can integrate more socio-economic into that; obviously it is going to be much better.

Hearing no concerns, I am concluding that the committee is thinking that this represents the best scientific information for the objectives of this analysis and within the parameters of the algorithm presented. We already went over our council work plan update the other day. Before we get into other business, I think a point of discussion, it will be good to get your input in general on whether we deal with the Hogfish ORCS right now.

I don't know if we were will be able to have all the pieces to do that or if we accomplish this between now and our next meeting through some conference call or webinar. I think we will have to have additional discussion and some decisions that will have to be on the record and not just completely as part of updating our report, because this is not something that was covered at this meeting.

DR. CROSSON: Are we going to have to do that, anyway, for the king mackerel numbers when John has completed running those different scenarios with the recruitment?

DR. BARBIERI: No; it is a little different, because in that case it is really more prescriptive. We have presented the parameters of the way that we want the projections to be configured, so all he has to do is add data and you're done. Well, we already have an expectation of what is going to come out the other end.

In the case of the hogfish ORCS application, there will be still some decisions to be made, and not all committee members may be comfortable using this or that. We always discuss issues a bit. It actually involves an official SSC ABC recommendation. It will have to be discussed in more detail that way. We can accomplish it if JC thinks that we have enough time to do it right now and get the discussion started. I don't see anything else here.

MR. CARMICHAEL: I don't think it will take that long to go through the hogfish stuff. It is just a few things. I think we can easily wrap it up here. I will be surprised if we're not done with this by one o'clock.

DR. BELCHER: My question was just the timeliness. If we can accomplish it, then I don't think it is a problem; but if it is something that requires more thought and discussion, what is the timeliness on when they need us to be reactive?

DR. BARBIERI: We have I guess the landings. Mike, do you want to go over –

DR. ERRIGO: I actually e-mailed this to everyone. Everyone should have this Excel spreadsheet; it has three tabs. This is the second tab; it is the landings. Red is recreational, green commercial, blue total landings for hogfish from Georgia, South Carolina, and North Carolina. The commercial landings basically are from the assessment.

I also checked it against the ALS database that I have and they are identical except for a single year, so I just used the data that I had because it is updated as of this August. Then the recreational data, I actually used the numbers that I had sent to me by the Science Center; because the way they estimated the weight in the assessment, I think they may have left out some of the intercepts that didn't have weight estimates or they use average weights.

I think the Science Center's methodology filled that out. Besides, we use the Science Center weight estimates to track landings for the ACL, so I thought that would be more appropriate. That is where the recreational landings come from in weight; but the numbers are actually identical, the landings and number.

Also in the Excel spreadsheet, I took all the pertinent indices that were in the assessment. If you guys wanted to look at that for trends in the fishery; there is some Georgia/North Carolina hook-and-line index, which spans the whole range. They are all fishery dependent. There is the South Carolina commercial trip ticket index, which is all gear but it is a much shorter timeframe.

I will show you a figure that has all three of them on it. Then there is the North Carolina index, which is a diving index. They had it broken down to these two pieces. This is the standardized average I think number of hogfish taken by that fishery. Then over here are just selected trips. I didn't get how they selected the trip and the proportion positive, but here are all the indices all together on a single graph. The red is the hook and line for the whole region. It is the logbook data.

The green dotted line is the North Carolina trip tickets for the diving fleet; and that shorter blue dotted line is the South Carolina trip tickets for all gear. Just for your information on the averages, South Carolina made up about 63 percent of the landings for the whole time series of the index. I did it for the time series of the index, so each index is a different length.

And then for the last four years; and 67 percent of the landings from the last four years; the hook-and-line landings made up 72 percent of the landings, but much less in recent years; more like 36 percent. The North Carolina spear fishery, which is the diving fishery they take by spear, was small. It was like 12 percent; and then it recently has gone up quite a bit to 22 percent all within the last four or five years. Those are the indices and their trends.

Hook and line shows a decreasing trend from the beginning, and then the other two shows like a sharp decrease at the end, but kind of flat for the rest of it. Then the very first tab is just the – these are the attributes that we looked at with the original scoring. That came from the one where we scored all these attributes and took the average.

And then this one down here, the moderately high, that was the one where we got together on expert opinion and modified this to this. Everything highlighted in red I felt was something that using information from the assessment, you might want to address those areas, because we now have information.

We have CPUE data, which we had nothing before so there was nothing to inform decisions based on that. Discard mortality; it was assumed that discard mortality was very low, because there is no bag or trip limit in this area for commercial or recreational. They haven't been meeting the ACL, so there is no closure that has been happening so far. Did I make a mistake? There is a bag limit in Florida.

DR. DUVAL: There is a five-fish bag limit I think throughout the region. There certainly is one in North Carolina; and then also commercially North Carolina several years ago, when was it, Brian, like 2010 or '09, we put in for hogfish commercially. We issued a proclamation limiting the commercial fishery to 150 pounds per day; and then for each additional day on the trip afterwards, you were allowed an additional hundred pounds for a maximum of 750 pounds of landings over 7 days.

DR. ERRIGO: Those are state-implemented regulations? Okay, so they would not be on the council's website or anything like that. Well, that changes it a little bit, because originally even with a recreational bag limit, the vast majority of the landings are commercial. Federally with no commercial trip limit and the depths that they are being fished in, most of the fish are above the minimum size. It was thought that discards would be minimal and therefore discard mortality was negligible.

But, North Carolina does have trip limits now in place, so that is not exactly true. Then overall fishery exploitation; although you rejected the use of the assessment to be used for making catch level recommendations, it did have estimates of the stock as being overfished, so all the estimates of current F were higher than each of the reference points Fmsy, F of 30 percent SPR, 35 percent SPR. It estimated the current SPR of the stock to be around 24 percent, 23.8 percent. That is all the information that is available, if you feel like you want to use it.

DR. REICHERT: That is based on the stock assessment that we saw. Yes, so we need to be careful, because we rejected that so we can't use it.

DR. ERRIGO: Right, so for here these estimates you may want to just not trust those at all; but the indices which have been standardized are still there and the trends are there. If you feel like there is useful information there; but they are fishery dependent. Then up at the top I actually filled out if everything stayed the same what the year of the highest landings was; what the risk of overexploitation scalar is for this risk of overexploitation category and the council's preferred from Amendment 29; that scalar for this risk category; and what the ABC would be.

If all those things stayed constant; this is what the ABC would end up being. You can score the things in red.; they are most likely not going to change the mean that gives you moderate. You may want to, however, now that you have trends, qualitatively look at this. If you change this category here, then you may want to – let's say you make this high, then you may want to say, well, maybe we think it's high, because we use the qualitative scoring for all species. We did not use the quantitative means; because due to the number of criteria, pretty much you always get moderate. Everything comes out to be around 2 when you take the average of all the scores.

DR. BARBIERI: Mike, I guess what you are saying is that you went through and applied basically the same type of criteria that we had already through the ORCS process.

DR. ERRIGO: I just used what was assigned to hogfish originally as moderate high. That was the risk of overexploitation that was originally given to hogfish when it was one contiguous stock for the South Atlantic. If you change this category, it will change the scalar; but the rest of the information will remain the same.

MR. CARMICHAEL: Mike pulled up the report on the other screen so you can see what stocks were moderate high and maybe help put this one into perspective and see if you are comfortable with that. You see hogfish before, when it was one stock, moderate high. Now we're just focusing on the northern component.

DR. BARBIERI: Basically based on this and thinking about the biology of hogfish; any disagreement with the moderate high or any thoughts on perhaps refining this and providing some different perspective or are we comfortable with this?

DR. SCHUELLER: To me it seems like the only consideration is, or at least what it sounds like, is this fisheries survey, and right now it is moderate at the bottom, right? That is the one that Mike had in red. He has in here decreasing trend in the last year. I guess to me that is the one thing that maybe we should talk about. If that is decreasing, does that warrant a change or not?

DR. ERRIGO: The red one is that logbook of hook and line, which is the whole area. That one has been decreasing over like the whole time period. And then the other two; the South Carolina one bounces up and down. It was relatively flat but it is very short.

Then the North Carolina spear is interesting, because it revved up very quickly. The fishery itself; there is a description of it in the assessment. For some reason in the last several years, the CPUE has been dropping flat; but it is fishery dependent, so that doesn't necessarily mean abundance is dropping. You do see some of the trends in a lot of the other indices.

DR. BARBIERI: Mike, the NCSC; that is trip ticket; that is all commercial trip tickets?

DR. ERRIGO: These are all commercial; there is no recreational index. The recreational, like the MRIP intercepts are extremely sparse. There is like two or three intercepts over the whole time period or something like that. I don't know about the headboat, what the intercepts were.

DR. BARBIERI: But the logbook, hook and line, the red line; that is not actually for the South Carolina/North Carolina.

DR. ERRIGO: That is for Georgia, South Carolina and North Carolina; and it is logbooks, so those are probably all federally permitted vessels. I don't know what kind of effort there might be from state vessels or the reason why you're seeing drops in CPUE is because of the catch and the effort is going away; I'm not sure. The number of boats has decreased, so it is hard to tell exactly why the trend is happening.

DR. SCHUELLER: I just want to say that is why prioritizing the indices that go in the model is an important process to undertake during a stock assessment. You should be demanding that every single time.

DR. ERRIGO: These were the three used in the model, which was obviously not accepted because of the amount of uncertainty in it. It was said that the logbook was the preferred, because it was a long time series and covered the whole range. The hook and line made up the bulk of the landings from most of the time period looked at during the assessment. The red line was considered the preferred index in the assessment; but the bulk of the fishery happens in North Carolina.

DR. SMITH: I think it is worth pointing out that obviously these are all fisheries-dependent indices so they are inherently unreliable. I question as to whether or not we can put a lot of stock into one year of decline.

DR. SCHUELLER: Yes; but I'm looking at the red one. If I was going to prioritize, I would prioritize the red one as being probably having the biggest spatial coverage. The hook and line is a little bit more – I don't know how this fishery works. I guess I have major concerns about the North Carolina spearing trip tickets.

If that is the bulk of the fishery, that is included within that hook-and-line logbook, anyway. If North Carolina commercial hook and line is the major component, it is included in this. Yes; it is fishery dependent, but it is the only thing we have and sometimes those fishery-dependent indices have some merits; and sometimes even if they are not used, they do match up with some of the fishery-independent stuff. Granted, we don't have that information, I am just speaking more broadly about indices in general.

DR. ERRIGO: I will say according to the description of the spear fishery, it has been dynamic over this period. It started out exploratory and then it ramped up as a directed fishery recently. There have been changes in how the fishery has operated according to the description of it that I read; but I have no personal knowledge of the spear fishery.

DR. SMITH: I agree with you, Mike; there are all kinds of things that could confound that. I bet if we plotted the number of boats with a GPS; that would correlate with this index, for example.

DR. BARBIERI: I wonder if Dr. Duval, with her background in molecular genetics, could give us some – I think that is from a previous life, but I am thinking about this being basically a disjunct population that was one of those kind of quasi-island biogeography kind of situations, right, where there is episodic colonization of areas and things kind of started developing, but the geographic extent of this and for it to have been separated for that long; but what are the risks of overexploitation, if there is any value for that component having that separate identity up there?

DR. SMITH: Are you referring to like a rescue effect from dispersal from Florida; is that what you're suggesting?

DR. BARBIERI: Well, we see this all the time. For example, the second most abundant larvae in the North Carolina Beaufort Inlet for the longest time happens to be gray snapper, believe it or not. There is a lot of transport that goes down from the Keys and is episodic in nature. I am just trying to think about the fact that something like hogfish; and you think about the distributional range globally of the different species of hogfish in general. They are primarily a tropical species; and during incursions into other kinds of habitat at times, but those populations are more disjunct and not necessarily part of the original sort of geographic range. I am thinking about how this thing got up there and how self-sustaining – Marcel.

DR. REICHERT: I am not entirely sure if I understand what you are trying to get at, because we can't have it both ways. We made a decision that this is a genetically different stock, different population. Church pointed out that it doesn't take much gene flow to have one population, so we made that distinction. Unless I completely misunderstand what you're saying, I would be careful to use that.

DR. BARBIERI: You misunderstood because I failed to explain correctly what I was trying to say. What I am trying to say if this is an episodic event, it was an evolutionary time that has happened and has allowed some kind of – it was like a colonization of those tropical species; that that would be additional risk for this one in terms of population persistence over time given the fact that there are only a limited number of episodic events that distributes the larvae and the probability of survival within that environment.

DR. SEDBERRY: Luiz, I think you might be thinking about this bunch of hogfish – I am not going to call it a population – a bunch of hogfish off of North Carolina as being isolated, and they are not. There is hogfish at that shelf-edge reef all the way up and even on to the Continental Shelf.

There are all life history stages of hogfish off South Carolina and Georgia and North Carolina. They just didn't have any DNA samples from South Carolina or Georgia. I think that is contributing to this idea of isolation; but I don't think it is isolated. I think it is a viable population. It is not some kind of refuge up there or refugia; it is connected to the rest of them.

DR. BARBIERI: Thank you, George, because that is exactly what I was thinking about. Yes, that helps a lot. I was thing this was some refugia that got disjunct and disconnected. It would be different, but you are right.

DR. SCHUELLER: Sorry, I have no idea what this has to do with what I put on the table, I guess. For the ORCS-specific scoring, we've already made the decision that this is a separate population based on the genetics; fine. If we're going to do this ORCS scoring, fine. The component piece that was in red that Mike put up there was the index. We've already said that we think that the logbook hook and line is probably the highest prioritized index. The question is are we going to use that to change the ORCS scoring or not, right?

DR. BARBIERI: Right, Amy. What I'm talking about is that the risk of overexploitation, like any population dynamics, is a balance between inputs and outputs. Positive inputs –

DR. SCHUELLER: But this index would reflect that inherently, right?

DR. BARBIERI: No, necessarily not over the time scales. We've seen this over time with different populations globally. My read of that is that index has been in a collective way flat. Some go up, some go down; and I would split the difference. That is just my particular opinion.

DR. BOREMAN: A couple of things; number one, this is the northern part of the range of hogfish, right? That to me is a clue that maybe they would be perhaps more susceptible at the northern part of their range, because they are out of their genetically ideal habitat or whatever you want to call it.

Number two, it is a term that we use in the Mid-Atlantic when we're looking at graphs and stuff; and we're trying to say, well, these past few years might – following up on what Will said – and the term is; is there any compelling evidence to change our minds in front of us here? The key word is "compelling". It is going up, it is going down, it is going down the past couple of years, but is it compelling enough? Given the variability around these estimates, the uncertainty; I don't think it is but other people might.

DR. BARBIERI: Are we there with moderate high?

MR. CARMICHAEL: There seems to be consensus. There certainly seems no compelling reason to change it.

DR. ERRIGO: Here is how it will stay; we'll consider it moderately high risk of overexploitation, which gives it the scalar of 1.25. The catch statistic is the highest landings from 1999 to 2007, which occurs in 1999, which is 32,184 pounds. Then you multiply by the scalar and get the OFL, which is 40,230 pounds. This is all whole weight. Then given the council's preferred alternative for most stocks in the moderately high risk of overexploitation category, the scalar was 0.7, which would give the ABC at 28,161 pounds whole weight.

DR. BARBIERI: Thank you, Mike, there we have it then. We just have to have some documentation of this discussion and procedure that we went through. The value is right there. I think we are down now to our second opportunity for public comment.

DR. GRIMES: I think you asked me to remind you about that Speckled Hind and Warsaw Grouper Committee thing. You are reminded.

DR. BARBIERI: Perhaps we can discuss this under other business, while we wait for Rusty. The Gulf and the South Atlantic Councils put together a joint committee to deal with South Florida issues; and during that discussion one of the things – and this is a joint council committee involving council members from both councils.

One of the discussions was to bring some level of cohesion or applicability of regulations and how we manage stocks for both jurisdictions and consistency in how management is conducted. This is something that committee identified as a critical issue, because there is a fishery in the Gulf that exists now for both speckled hind and Warsaw grouper.

Of course, we don't have an existing directed fishery in the South Atlantic. This committee set the ABC equal to zero back in 2008 or '09. Since then we have been debating on whether overfishing is still occurring or not and what additional level of protection should be implemented to rebuild those stocks.

During that committee meeting, discussion came up about those two stocks. The idea was to ask the Gulf and the South Atlantic SSCs to put together a subcommittee, a subgroup of members that would try to address a few issues to bring some resolution to this; you know, can we explain why we are treating those things differently; what is the status of the stock; are these the same stock genetically?

Is this the same populations just divided that way because of management reasons or are we talking about something different? That subcommittee, George, and Church have been participating; Marcel and I and John Carmichael have been participating in those discussions.

The bottom line is that we haven't really been able to move forward too much and have really anything, because somebody has to take the time to do some of the analysis, to go and search for the different types of data and pull all that stuff out and summarize all of that in a way that is intelligible to the group. We haven't been able to do that.

It is just something that Church for one has been asking from time to time, because we never communicate on this issue. Church is thinking did I miss some e-mail; did I miss the boat here and forgot to attend a meeting or you guys have not been meeting at all? No, we have not been meeting at all.

The main reason is we haven't been able to identify really a clear process for us to address those questions that we have been charged with. This is just something that I was wondering if the committee would have any kind of suggestions or recommendations; if there is anything that you can think of that could either help get that issue resolved or perhaps agree that it would be great to have that done, but realistically it is not something that can be accomplished.

DR. GRIMES: That is fine with me. All what I mainly remember was that you were trying to think of was there any way we could come up with some information that was relevant to what status of these two stocks was. I think there is a way you could use faunal-surveyed data from through time and the current ongoing landings' data to estimate some kind of a sort of phony catch series; what their catches of those stocks would have been if their abundance was the same in these faunal surveys or something like that; which is not much better than nothing; but if that

is all there is, that is all there is. Other people on that group may have thought of other things that might be done. That's all.

DR. BARBIERI: It is a tough nut to crack. I guess we can now proceed with the public comment, Rusty.

MR. HUDSON: Thank you, Mr. Chairman and the SSC; Rusty Hudson representing the East Coast Fisheries Section of the Southeastern Fisheries Association. We touched on a few things whenever we had the oral comment and written comment at the beginning of this process. We did not have anything on hogfish in there, but the one thing that stood out was the giant MRFSS spike there.

Having dealt with this numerous times in stock assessments and having heard NMFS scientists always have that same complaint about '81 through '85; some of that stuff is really questionable data. It sort of fits hand and glove with what I pointed out with the red snapper; the raw data issues there with the MRFSS back in the eighties and red flag there.

King mackerel; Dr. Sedberry had mentioned the gyres and the larvae and that type of stuff. Well, we had brought in the TIP data, but it was only the Florida/Georgia TIP data. It would be nice to have like the North Carolina stuff and maybe that would show us a little bit more and we'd be better informed.

We believe the spawner-recruit issue isn't really a big deal. We do side with the idea that we need to deal with something on the insanity ruling, because that was like out of nowhere and that was a hard hit. It is going to be real hard to explain it to stakeholders. I hope the council deals with this very rapidly at the December meeting and then whatever we can do to resolve that issue.

Black sea bass pots; to me I take issue between the idea of black sea bass pots and American lobster traps and spiny lobster traps and the trap/pot interchange. But with that said, we made these significant changes. I know there are eight alternatives in Regulatory Amendment 16; but 35 pots, maximum you are looking at 1,120 vertical lines.

You've got 15 of those permits in North Carolina, 9 or 10 in South Carolina – there is one sort of wayward right now possibly in transition – and 7 in Florida. Having run a shrimp boat up and down the coast of Florida all through the eighties; we would have the right whales right there within – from here to the door down here by the parking lot.

Generally it is three-quarters of a mile, a mile, two miles. The critical habitat has been in place since June of '94. That is an important fact. There was no real reason to change that up until this black sea bass pot thing suddenly reared its head. With the great reduction of pots that are not sitting there overnight anymore and it is a limitation of the 35 per vessel and a lot of vessels like Kari mentioned aren't using that many pots; in fact, I can substantiate that through some of the smaller boats that are down on our end.

As I talked with Nick and the idea of a spatial/temporal analysis that was sort of lacking about these differences; we wouldn't have the whales in our backyard November and December to speak of; when I say my backyard, Ponce Inlet. Then about the time we get to the late

March/April, they are already moved again. That is an important thought is that this idea of zero or 100 percent – and I liked what Carolyn had to say about negligible effect. Now, of course, I know they are not going to be real hep about going back to Alternative 2 the way it was; but Alternative 3 is the existing critical habitat. Getting that further analysis might be useful in the idea of narrowing down some of the concern.

As you also saw the peak with the 20 meter, the 25 meter, and the 30 meter is where these people normally prosecute their fishery. I figure that the 30 meter is predominantly off South Carolina, because you don't have that Gulf Stream veering in like we do off of Cape Canaveral; and Cape Canaveral is the southern boundary of where you can use a pot; and Cape Hatteras on the north end.

Both those areas, you have got a little bit more current to deal with. With that said, I am just feeling that there could be a little bit more sanity brought into the process even on this black sea bass; king mackerel, black sea bass, whatever. Every time we get waylaid by something, we, the stakeholders, it makes it a real difficult circumstance for the fishing community.

Thank you very much for everything and please keep in mind that TIP data, because the recruits are there. It is just a matter of if NMFS had the ability to do better surveys and stuff, we would probably have a little better information to work with. Thanks a lot and everybody have good travels.

MR. HARTIG: I'm the public; I wear lots of hats; I think it is appropriate. We talked of this about the assessment; and just having you going through the king mackerel assessment, I think one thing that we talked about was trying to have some way to have an inter-assessment ABC adjustment.

Your setup in the SEDAR process; you've got about five years between assessments. So in king mackerel – and we don't know how the projections are going to come out yet; but right now catches are depressed, F is low. That is going to continue until the stock starts to rebound. F will increase and fishermen will come back in.

Then we could have the possibility of bumping up against the trip limits and actually not being able to catch some of that productivity that is in that stock. Well, selectivity is at age two and three for most of your catch. That is a pretty critical time to be able to make an adjustment. Even if you decided that we wanted to change the SEDAR schedule, which we want to get away from; it would still be two years before you could probably do it.

So some way for the SSC between assessments to be able to look at the ABC – and in mackerel we're lucky that we have some kind of recruitment information coming from a fishery-independent survey. We have the information from fishermen that say, hey, we're bumping up against a trip limit, we need to look at this; and you have the TIP cohort analysis like Peter did.

There are some things you could feed into this that you could look at. I think when I talked to John he said, well, heck, we could just look at the recruitment function based on the information we have. Actually, they could do an update; just a quick recruitment-type update that you could look at and then we could possibly change the ABC based on that.

I think I would like to work with some of you in trying to form this into something that you could actually do into the future for stocks that meet these kinds of criteria, just something to think about. The other thing; thank you for your continued participation, all of you, and your dedication to our process. This is a great SSC we have. You all are well tuned to all the discussions. I certainly appreciate everything you do for us, because we couldn't do it without you. Thanks for that.

DR. BARBIERI: Thank you, Chairman Hartig. We really enjoy working with this council as well. I don't know if anybody else would have any kind of comment in regards to what Chairman Hartig brought up on the intermediate ABCs.

DR. BELCHER: I would be interested. I know John has brought it up a couple times about the Mid-Atlantic's rumble strip approach, maybe to see how that might be something that we could interlay into that.

DR. BARBIERI: That is one thing that popped into my mind. That is why I was looking that way thinking there is something else to be said, because that popped up into my mind. Then another thing is that I can see that in the Gulf at least we have done, perhaps here. Too, over the years, is have sort of a revised set of projections.

By the time that you have some fresh, updated information of recruitment and landings that are being estimated now or predicted to be at a certain level; you can reconfigure and rerun projections that integrate more up-to-date data. This committee could review those and provide you with a fresh set of ABCs that will be more in line with the realities of changes in population in fisheries.

DR. BOREMAN: Just to clarify on the rumble-strip approach; the rumble-strip approach is not a recipe for changing the ABC. In other words, that is the first step. If your stock is running outside onto the rumble strip off the road, what that does is just trigger a further investigation, a deeper investigation as to why. It just might be a weird index or whatever. It doesn't automatically trigger a new ABC. That is up to the SSC to take a look. What it does; it is an alert. It is the red light going on in your dashboard to pull over and take a look at your engine.

DR. BARBIERI: I guess we are actually past other business, but I am going to open up to any final comments from the committee before we completely adjourn. If not, I just want to point out that our report is to be provided to the council by 9:00 a.m. on Tuesday, November 18. This is this report that we are building, not necessarily our ABC workshop and the control rule workshop.

This is really our SSC report; and Marcel and I are going to be working on the first draft, and we are going to try to shoot that to the committee as soon as possible to all of you. We would really appreciate if you can put some time to providing some good suggestions there. This is one of those things that because we don't have a lot of time to build it, when you send comments back, it is very helpful.

Instead of you just acting like a manuscript reviewer that presents the problems; provide some suggestions; change things yourself and track changes, because otherwise one of us is going to have to spend the time to try and rewrite that. It may not be captured necessarily what you had

in mind. Since this is our report, we would really appreciate if you can put some of that. I think we have had a lot of participation in the building of the report; and am very happy with the outcome. Before I give Vice-Chairman Reichert opportunity for input as well, I want to comment on our new SSC members.

I am thrilled really with the new additions to the committee. I agree with Chairman Hartig that we have a wonderful SSC already; but having these two additions has been really, really good. I appreciate you guys joining us and all your contributions, the level of participation and all. It really adds up. Welcome aboard and thank you.

DR. REICHERT: Do we have the next meeting reminder?

The next meeting reminder; do we have that; so 2015 proposed dates; April 28 through 30; so we are back to what we had before; and then the October 20 through the 22; and that is it. Then meeting adjourned.

(Whereupon, the meeting was adjourned on October 30, 2014.)

Certified By: _____ Date: _____

Transcribed By:
Graham Transcriptions, Inc.
November 10, 2014

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PLEASE SIGN IN

In order to have a record of your attendance at each meeting and your name included in the minutes, we ask that you sign this sheet for the meeting shown below.

South Atlantic Fishery Management Council

ABC Control Rule Workshop:

Monday, October 27, 2014

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South Atlantic Fishery Management Council

ABC Control Rule Workshop:

Tuesday, October 28, 2014

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South Atlantic Fishery Management Council
Scientific & Statistical Committee Meeting:
Tuesday, October 28, 2014

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South Atlantic Fishery Management Council

Scientific & Statistical Committee Meeting:

Wednesday, October 29, 2014

+ THUR , " 30, 2014

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70	MacLAuchlin, K	kari.maclauchlin@safmc.ne...	5 min
45	Kinder, Kevin	kkinder@emory.edu	53 min
43	pugliese, roger	roger.pugliese@safmc.net	5 min
37	Hudson, Rusty	dsf2009@aol.com	172 min
29	Ballenger, Joseph	ballengerj@dnr.sc.gov	158 min
26	Smart, Tracey	smartt@dnr.sc.gov	176 min
21	Takade-Heumacher, ...	htakade@edf.org	83 min
21	Siegfried, Kate	kate.siegfried@noaa.gov	54 min
21	Bubley, Walter	bubleyw@dnr.sc.gov	156 min
21	Stump, Ken	kstump@oceanfdn.org	172 min
21	Bowers, Christian	cdbower@emory.edu	114 min

SSC DAY 2

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60	L, I	captaindrifter@bellsouth....	-24429113 min
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54	Nash, Chandler	cvnash@emory.edu	66 min
50	Mijares-Shafai, Cy...	cmijare@emory.edu	40 min
46	Cai, Jessie	jcai26@emory.edu	0 min
45	Gerhart, Susan	susan.gerhart@noaa.gov	354 min
42	Hudson, Rusty	dsf2009@aol.com	472 min
38	branstetter, steve...	steve.branstetter@noaa.go...	395 min
34	Siegfried, Kate	kate.siegfried@noaa.gov	230 min
32	Stump, Ken	kstump@oceanfdn.org	487 min
31	Ragazzo, Leo	lragazz@emory.edu	65 min
30	Kinder, Kevin	kkinder@emory.edu	70 min
30	DeVictor, Rick	rick.devictor@noaa.gov	212 min
27	Khinvasara, Snikha...	skhinva@emory.edu	66 min
24	stephen, jessica	jessica.stephen@noaa.gov	207 min
24	richardson, kate	kericha@emory.edu	63 min
23	Bubley, Walter	bubleyw@dnr.sc.gov	381 min
23	Hensley, Jasmine	jrhensl@emory.edu	68 min
23	sandorf, scott	scott.sandorf@noaa.gov	187 min
22	Kuhn, Sara	sara.kuhn@emory.edu	53 min
22	Powell, Jessica	jessica.powell@noaa.gov	391 min
22	Hoeninghausen, Mad...	mhoenin@emory.edu	73 min
22	Bowers, Christian	cdbower@emory.edu	95 min
22	Zoodsma, Barb	barb.zoodsma@noaa.gov	425 min
21	Riley, Elise	elriley@emory.edu	89 min
21	McGovern, John	john.mcgovern@noaa.gov	78 min
21	Farmer, Nick	nick.farmer@noaa.gov	135 min

21	Cheuvront, Brian	brian.cheuvront@safmc.net...	15 min
21	Mahood, Bob	robert.mahood@safmc.net	389 min
21	Ballenger, Joseph	ballengerj@dnr.sc.gov	389 min

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43	Garrison, Lance	lance.garrison@noaa.gov	166 min
40	Hudson, Rusty	dsf2009@aol.com	257 min
39	Slivinski, Luke	lms65@duke.edu	18 min
34	Flood, Kevin	kflood2@emory.edu	54 min
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28	Cai, Jessie	jcai26@emory.edu	67 min
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20	Lee, Jennifer	4jennylou@gmail.com	144 min