

Draft Minutes
American Eel
Great Lakes/Canada Threats and Population Dynamics Workshop
U.S. Fish and Wildlife Service In cooperation with
National Marine Fisheries Service
January 31 — February 2, 2006
Buffalo, New York

Wednesday, Feb. 1

Disease Presentations

Disease Session (9:00)

Lake Presentations (10:20)

Lake Session (11:00)

Stock Assessment & Peer Review Presentation

Stock Assessment Discussion

Conceptual Model of Annual Abundance Presentation

Population Dynamics Discussion

Thursday, Feb. 2

Contaminants Presentation

Contaminants Session

Population Dynamics Presentation (Diversity of habitat use in anguillacolid eels)

Population Dynamics Session

Tuesday 31st

5:30-7:30 p.m. Richardson Room - Opening remarks and social hour (hosted by Great Lakes Fisheries Commission-GLFC)

Wednesday 1st

Present: Invited experts: Bob Graham (Dominion Generation), Len Machut (State University of New York), Ken Oliveira (University of Massachusetts), Catherine Couillard (Institut Maurice Lamontagne), Dawn Dittman (USGS Tunison Lab), Alastair Mathers (Ontario Ministry of Natural Resources), Rob Macgregor (Ontario Ministry of Natural Resources), Guy Verreault (et de la Faune du Quebec), John Casselman (Queen's University), Karin Limburg (State University of New York), Paul Angermeier (USGS Virginia Tech), Joseph Hightower (USGS North Carolina State University), Tim Brush (Normandeau Associates), and Peter Hodson (School of Environmental Studies, Kingston) was present on Feb. 2nd. Also invited but unable to attend last minute was Kevin Reid (Ontario Commercial Fisheries Association). Commission representatives were Lydia Munger (ASMFC), Heather Lutz (GLFC), and John Dettmers (GLFC), and tribal nation representatives present were Jim Snyder (St. Regis Mohawk Tribe) and Neil Patterson, Jr. (Tuscarora Nation).

NMFS and USFWS: Heather Bell (NE Region, FWS), Dave Perkins (NE Region, FWS), Marty Miller (NE Region, FWS), Wilson Laney (South Atlantic Fisheries Coordinator, FWS), Michael Twohey (Midwest Region, FWS), Marie Maltese (International Affairs, Headquarters, FWS), Amy Roe (Courtland ES, FWS), Kofi FynnAikens (Lower Great Lakes FRO, FWS) Marci Caplis (NE Region, FWS), Steve Patch (Courtland ES, FWS), June DeWeese (Courtland ES, FWS), Kim Damon-Randall (NE Region, NMFS).

8:02 a.m. Introduction (Heather, Mike & Marci): March convened the meeting at two minutes after eight and advised that her watch would be the clock we used to maintain the schedule. She covered the logistics of restroom and telephone locations. Heather Bell asked that participants turn in their Information Quality questionnaires, and asked that each person put their name on their placards. She noted folders for everyone were on the back table, along with some papers that John Casselman brought. Heather asked that any additional papers brought be given to Marie, and they would decide whether they needed to be copied for immediate provision. John Casselman noted that the two papers he is providing are from 1997, and appear the same, but they are not. They are background for some of the things he wants to talk about later.

Marci noted there are power strips in the room for those who want to plug in their computers. Marci briefly reviewed ground rules, including one person speaking at a time, and taking any sidebar conversations outside the room. Heather noted that we are being tape recorded, so sidebar conversations would be picked up.

Heather noted that Ken, Len and Bob are on our first panel, and Ken and Len would be giving the presentation for us.

8:06 – 9:00 Disease Presentations (Ken Oliveira & Len Machut): Ken noted the presentation that he would give is based on work done by one of his graduate students for her Master's. Ken asked that for the glass eel and yellow eel stages, we are essentially considering benthic stages, but the effects of the parasite may be more pronounced on the silver eel, the pelagic stage.

The parasite has five life stages that it goes through. It becomes sexually mature in the swim bladder of the eel. The intermediate hosts are believed to be copepods, in either freshwater or estuarine settings. If the copepod is consumed by a fish, it can infect such intermediate hosts. Right now there are up to 12 families of intermediate hosts. The parasite doesn't seem to be too picky. The parasite was originally described in 1974, and was originally known as a different species. It went through Europe in a hurry, and was found in Texas in 1995. The eels in Texas were known to have come from the east coast. It was found in SC, in the Chesapeake, and Hudson River. Ken found it in SE Massachusetts when he returned to Massachusetts, from Maine.

Ken's objectives for the study they did was to determine the northern extent of introduced parasite (*Anguillicola crassus*) range in the US, examine the prevalence and intensity, and to examine the effects of the parasite on eel. They looked at the northeastern distribution of the parasite in yellow eels. They skipped CT because Steve Gephard had already sampled there. They skipped NH because of its short coastline. They did electroshocking in freshwater, and looked for a minimum of 25 eels per site. At some sites it was very easy, at others more difficult, with only 15-20 eels at a site. Ken also did some collections of silver eels and examined them for parasites. Those samples were composites of the entire watershed.

Eels were euthanized with clove oil. They were weighed and length measured. Parasites were counted, and parasite length measured. Prevalence is defined as what percent of the population was infected, and mean intensity is defined as the number of parasites per animal. Ken showed us examples of healthy and grossly infected swim bladders. Prevalence of infection in RI ranged from 69 percent to 28 percent. Ken noted that he did his Ph.D. in 1991 and 1992 and never found a parasite, and last summer he found 35 percent in the same river. In MA, if the low values are pulled out, all the large ports have prevalences over 50 percent. Ken indicated they were looking at a potential ballast water situation. The high was 76 percent in Boston.

In ME, some of the same pattern is evident, with the percentage high in Portland, a major port. In the Kennebec, the percentage was 60 percent. In the Sheepscott, the percentage was 31 percent. Again, in the mid-90's Ken noted the parasite wasn't there at all, or was in very low prevalence. The infection rate has clearly increased in the last ten years. The Penobscot River has 26 percent, and that is as far north as the parasite has been found. It wasn't present in the Machias River.

Ken noted that there is a significant relationship between parasites and size, with larger eels having more parasites. This would be expected because bigger eels eat more. What is scary is how small eels can

be and have the parasite. They caught one 70-mm eel that was already infected, so glass eels can be infected, Ken believes.

Ken indicated he could determine no external differences between eels with parasites and those without them. He noted the literature does provide some external characteristics, such as anal redness, but he didn't find them. They looked at length-weight regressions, and found no difference at all.

For silver eels, in the Paskamansett River, Ken had two years of data. In 2003, the prevalence was 28 percent, and in 2004, it was 52 percent. In 2005, his gear was wiped out due to high flows. The mean intensity in 2003, was 2.9, and in 2004 was 5.8. Ken noted that he has kept some of the eels in the lab for as long as 15 months, and the eels are still alive and well, so the parasite is very persistent in the silver eel stage and will be present during the migration to the Sargasso. It appears to have a long life span.

Conclusions: The Sedgeunkedunk Stream in ME is the furthest northern point the parasite has been found. There was no latitudinal trend, but there are possible ballast water introductions. Temperature is not an apparent limiting factor. Ken said there is no reason why the four rivers in which the parasite wasn't found, shouldn't have them. Ken noted they are knocking on the door of Canada.

With regard to the influences of the parasite, all size classes are infected; there is no apparent detrimental effect on eel weight and length in the yellow eel stage; but, there is a suspected negative impact on migrating silver eels. There is no work that has been done to test for any effect on buoyancy control, when the swim bladder has been compromised. Ken noted that we can only speculate about the impacts on the silver eel, but he noted that full swim bladders probably don't function very well.

Ken/Heather Bell called for questions. John Casselman clarified that temperature didn't appear to be a limiting factor. Ken was asked if there are temperature loggers in the rivers that didn't have the parasites. Ken indicated that they are looking to get some data. The question was asked if there are springs in rivers that might keep the parasites alive during the winter. Ken noted that they may not infect eels at low temperatures, but once the temperature gets high enough, they become infective.

Alastair asked about intermediate hosts. Ken indicated there are 12 families that can carry the intermediate stages. Len noted that in Europe, a wide variety of invertebrates has been found to carry the parasites. Ken noted that intensity and prevalence match up pretty well. Catherine asked if the prevalence increased with length class, i.e., did Ken look at prevalence by length class. Ken stated the general trend was for higher prevalence to correlate with intensity. Catherine asked about seasonality. Ken noted they did their work in the summer, so he couldn't answer that question.

Bob Graham asked about infective stages in the swim bladder wall. Ken indicated they didn't find them. Bob asked Ken about looking for the smaller stages. Ken noted if the adults are there, the smaller stages will be as well. Ken noted that it would be more difficult to look for the microscopic stages. Ken noted you can see them through the wall of yellow eels. Bob noted that in NC waters, there has been increasing prevalence, with length of the eel.

John Casselman suggested that age might be a factor. Ken didn't think it was. He suggested that size was more related to diet, and the intensity of infection likely akin to accumulation of parasites. Ken noted this parasite has to be ingested, and he was surprised that glass eels could ingest them. John noted they had looked for the parasite, in thousands of eels, and never found them. He stated there is some sort of gradient, for it not to be present in the St. Lawrence. He asked Ken to comment on the transport.

Ken agreed that transport was an issue. He didn't want to say that it was creeping up, but it was a concern.

John noted that they have a buyer who is importing eels, and he is concerned about eels inoculating northern populations. Ken felt there is a good chance any imported eels will be carrying the parasite.

Karen indicated she had a question, but she would hold it so we could move on.

8:37 a.m.: Len thanked Ken for covering a lot of the background. He indicated that he would focus on smaller watershed systems, in tributaries of the Hudson River. They wanted to extend the results of previous studies into the tributaries. He indicated he would discuss eel densities and their infection. He noted the population density is an important factor in transmission of the parasites. Eel condition and susceptibility will be discussed. He also intended to discuss prevalence and intensity.

Six study sites with varying degrees of barriers were selected. There are no fish passes on any of the barriers. Eels may be able to pass them but not other species. They sampled using electrofishing. They brought into the lab 232 eels of 1,938 captured. The watershed areas ranged from 47.9 to 166.4 square km. Eel penetration varied, and Len indicated that may be a function of the number of barriers. Whether the barriers are man-made has an influence. There is a strong negative correlation between declines in eel density and barriers, with declines above the barriers precipitous. Len indicated they have developed a model for estimating the eel populations above barriers.

Len discussed size distribution. They found high prevalence in even the smallest size classes. The small tributaries are very important nursery habitats for eel growth. Len noted that size does matter. There are fewer, but larger individuals, above the barriers.

Len discussed eel condition. They performed a Gap analysis and found a relationship between increasing urbanization and eel condition. They also found a relationship between condition and barrier intensity (the first barrier reduces density by a factor of 10). A few eels can migrate beyond the barriers, but they are of markedly lower condition, however, lower condition also coincides with higher growth rates. As barrier intensity increases (and eel densities decrease), eels able to reach these habitats are able to grow faster.

Len referenced the Morrison and Secor (2003) paper, where prevalence increased significantly over time. In Len's studies, they found lower infection rates and prevalence than in the mainstem. They didn't find any relationship with latitude, or with length for infection intensity. What they did notice was that the more urbanized systems have more prevalence and higher intensities. They looked at "country" eels, versus "urban" eels, and found a trend of increase, but it wasn't significant. It looks as though there may be some anthropogenic impacts, perhaps due to more stressors in more urban systems.

Len asked if there was a silver lining to those "damn dams." He noted that prevalence and intensity did both decline above barriers, in general. The data were compromised by one young eel that was infected with four nematodes. Beyond six barriers, there were no parasites. The conclusion was that barriers are slowing nematode invasion.

With regard to eel condition, they found the same effect as Ken. Yellow eels with parasites have the same condition as healthy eels. There was no significant difference.

Len summarized: The first barrier appears to reduce eel density by a factor of ten. He noted that one of his co-authors has received funding to put an eelway onto a system, and he noted that will open up the system to the parasites. Increased urbanization negatively affects eel health. Infection with the parasite is lower in tributaries than the Hudson mainstem. He wondered if this was simply because the invasion of the parasite was caught in early stages, or was a general trend. The invasion is ongoing. It might be infected by barriers, but will not be stopped.

Len thanked his field crews, and the Hudson River Foundation for their financial support.

Joe Hightower indicated that most of the other studies have seen a length effect. Joe noted the size range in the tributaries might be lower than the mainstem, so that might explain the effect. Len indicated that their size range was actually larger, and they got one eel that was silvering, but it escaped. The highest intensity they found was in a large eel, but by and large, their intensities were lower. He noted that with the lower intensities they are observing in tributaries in general, that might wash out the length relationship. He noted that Mary Moser in her earlier studies in NC didn't find any length relationship either.

Jim asked if contaminant levels were compared to parasite levels. Len noted they are presently collaborating to look at that question. It will be interesting to see if there is a relationship between PCB and infection rate. They aren't looking at heavy metals.

John Casselman noted that Len had referenced the "dam dam" effect. He asked if the fact that the dam increasing the density downstream shouldn't be something of concern. He noted that could affect the sex ratio, as well as the likelihood of infection.

Len agreed that was an important factor. In the Hudson River in particular, the barriers cut off prime eel habitat. Some watersheds with strong urbanization pressures still have prime eel habitat in them. The habitat isn't being fully utilized. The two factors play off each other. Eel density below the barriers is high. The key is to get them above the barriers. In the tributaries below the dams, they are finding densities very high, but very low in similar tributaries upstream.

John Casselman asked if we could say if barriers have been increased downstream, and asked Ken if we could say the sex ration has been affected. Ken noted that it was hard to attribute the density effect to the dams.

Catherine noted that urbanization could have other effects, aside from the stressors. She noted that increased organic inputs could affect the parasites. She asked Len to clarify his findings about reduced condition above barriers. Len noted that he couldn't say to what this is due. At the moment, they are just saying that urbanization is having negative effects on eels. The two most heavily urbanized streams exhibit severe stream degradation.

Heather Bell asked what the condition was that Len looked at. Len noted it was a mathematical model from Sokal and Rolf, using a regression. They looked at the residuals and compared it to a trend line.

Wilson asked that Len clarify that eels above barriers grew faster, but were in worse condition. Len indicated that was the case. He noted that eels at higher density below dams might be competing with each other. Karin noted that it could be a life history strategy for the eels to grow longer upstream, then move downstream and put on weight. Len noted that some of the otolith analysis that has been done seems to confirm that might be the case.

Ken asked about the sex ratio. Len noted they are working on that issue, but at the moment, they believe the barriers have a bigger effect on growth, than on the sex ratio.

Bob Graham noted that on the James River, with a series of seven dams, the characteristics of dams seem to have an effect on whether eels can pass them. He suggested that dam factors could be incorporated into the mix. Len noted that some of the barriers they investigated were natural ones, such as waterfalls. He agreed with Bob that it was worth looking into, and might present an even clearer picture of what is happening.

John Casselman suggested caution should be used in interpreting eel growth. As they put on length at certain stages, there is an apparent decrease in condition.

9:00 – 10:00 Disease Session (Lead – Heather)

9:10 a.m.: Heather noted that she had learned about Len's work after the first workshop. She noted that we had gotten kind of wrapped around the axles regarding barriers, and felt that there was a lot of work that we would have to do, to characterize the barrier threat. She indicated that she would like to finish the disease session, but later would like to challenge the folks who are barrier experts to spend some time thinking about how Heather might characterize the permeability of barriers. She might have to have 50 parameters, none of them in the Corps of Engineers database. She noted that she might be able to do some Gap analysis, which would give her a relative idea of permeability.

Heather turned to the questions regarding the disease issue. **Question 1.** The first question is: What are the current hypotheses around the impact of *A. crassus* to the long-term persistence of the American eel population?

Karen noted that one researcher to whom she had talked in Europe was adamant that the parasites do sap the energy of eels, but also stated that PCB likely has equivalent effects. Heather asked if there is any reason to believe that information would transfer. She noted the European eels do have a longer migration.

Paul Angermeir noted that there are other diseases we haven't discussed. He noted that some of the other things could affect portions of the population. He suggested that we should first seek what is the relative importance of the parasite, versus all the other things out there. Heather noted that was question number three.

Karen noted that it would be worthwhile to continue the dialog she had established.

The second part of question one was: Is there any reason to think that American eel would have different results. Ken didn't believe that they would. Heather indicated that was her conclusion. Ken noted that the parasite was relatively new to both European and American eels. Tim Brush suggested there might be some threshold above which the impact might be manifested.

Question 2. The second question is: What information do we have on the effect of *A. crassus* with regard to capturing prey items, ability to escape predators, and other sub-lethal effects. Joe noted that since two studies found no differences in length-weight relationships between infected and healthy eels, which suggest their behavior hasn't been affected. Paul noted that the impact might be different between lakes, versus rivers.

Question 3: We have had presentations on the parasite, are there other "diseases" or parasites we should be concerned with at a population level? Bob Graham noted that viral infections reduced swimming ability in European eels, and we haven't even looked at it here. He indicated that he could tell us which reference it was, the one by Henican et al. Heather asked how often viruses had been identified in American eel populations. Ken indicated they are there, but nothing compared to the magnitude of the parasite. Heather asked that any papers on viruses in American eels be sent to her or brought to her attention.

Catherine noted that eels kept in tanks often developed bacterial infections, and that some viruses present in the individual will become active when the individual is stressed, such as during metamorphoses.

Len noted that in the Wyanskill River, they found a number of eels with ulcers which they couldn't relate back to the parasite. They don't know how the skin ulcers originated. There were also a few cases of inexplicable die offs. Urbanization could be having an effect. In dealing with urbanized watersheds, we might be dealing with worse problems.

John Casselman noted that he wasn't an expert on viruses, but the VHS family that is causing problems in Europe is hard to document and identify. He noted that we have had some strange die-offs of eels that we haven't been able to explain. He noted that in 1986 and 1987, there was a die-off, and modeling suggests that perhaps a third of the eels could have been lost in Upper St. Lawrence/Lake Ontario. He contacted the Europeans, but they didn't mention viruses at the time. He noted that there was another die-off in 1992, in the lower St. Lawrence, but of lower magnitude. John noted that VHS was present in Lake Ontario, in other species.

Catherine noted that we don't have good lab facilities, and there aren't many folks working on eel diseases. She suggested that specialized tools are needed to look for eel diseases.

Marci moved to **Question 5:** What are the mortality rates associated with *A. crassus*? Morrison and Secor reported loss rates of 9 and 24 percent with no statistical difference between fresh and brackish water sites. Can we extrapolate these values range-wide? Ken noted that he had eels in his laboratory, with parasites, that have gone 16 months without feeding, and still showed no mortality, although they are starting to get thin.

Joe asked Heather if she recalled what the loss rate in the Morrison and Secor paper was defined as? She didn't. Bob Graham thought, and Joe concurred, that the "loss rate" included all losses from the

study are, including emigration. Heather asked, if it was premature to presume some mortality rate, if we could come up with some idea of the prevalence rate. She thought the Europeans had set 15 parasites as some sort of threshold. Len noted the Europeans found up to 100 percent infection in their populations. He thought that we were seeing increased infection rates. He wasn't sure how we could determine a mortality rate. He noted they had some eels that died after being electroshocked, due to internal bleeding in the swim bladder, but wasn't sure how you could quantify that.

Bob Graham noted that any time you get over 20 parasites; the bladder begins to be packed.

John Casselman suggested that it wasn't mortality we were worried about; rather it would be behavior during migration, and the depth to which they could descend.

Tim Brush noted that from Ken's lab experiment, the direct effects don't appear to be great, but it is what John is talking about that we need to be concerned about.

Heather suggested that perhaps we need to rearrange the question. She noted if we just look at *A. crassus*, then perhaps we need to ask, what is the effect on fecundity of silver eels with over 20 parasites? If the prevalence rate is up to 70 percent, and set a threshold of 15, then are we looking at a quarter to a half of the eels that are migrating out? How big of a problem is this, at a population level? Marci noted if it spread to 100 percent of the European population, it isn't going to check itself in the American stocks.

John Casselman agreed that was an important point. He noted that most of the fecundity on the US east coast could be affected by this parasite. We have lost a lot of fecundity in the upper St. Lawrence, and now we have this threat for the rest of the coast. He felt this was an important issue.

Tim Brush noted that parasitism is common everywhere. Typically the parasite evolves with the host to achieve a balance. With an introduction such as this, the approach taken should be conservative in trying to define impacts.

With shellfish, novel infections can certainly wipe out local populations. With this particular parasite, we don't know for a fact that there aren't other new diseases or parasites coming in, but we do know that we have this one.

Bob Graham noted that when the parasite was first introduced to Europe, it seemed that the greatest impact was early on, and now it is less, at least that was the impression he had.

Ken noted that he questioned that presumption, since the European infections began in the early 1980's, and that isn't many generation times for an eel.

Heather noted she had the impression that the physiology of the Japanese eel might have allowed the development of certain immunity. She wondered if we know how it has tolerated the parasite, and wondered if the American eel might develop some immunity in the future. She noted that air bladders packed with parasites would not seem beneficial, and wondered if American eels would develop the immunity.

Paul noted that *A. japonica* has a much shorter migration route, so the impacts could be proportionately much less.

Len noted that it is likely that American and European eels may not have had enough time, evolutionarily speaking, to develop any antibodies to preclude bladder infections.

Tim Brush noted that was a very relevant question, and we weren't likely to obtain an answer in our lifetime.

Rob McGregor asked if there was anything we could do to slow the spread?

Bob Graham noted that we had a situation in NC where we might be introducing the nematode to a system where it isn't present. He noted that we have some ideas about looking for the parasite in the

upper system. He noted that American eels have been sold in the upper watershed, so the parasite could be present there already. He noted our concerns were impacts to other species.

Catherine noted that eels in urban areas were more susceptible, because there are more stresses, so if we can reduce the stress, we might be able to reduce the infection rate. She noted also that some of the eels don't have the parasite, and wondered why. She advocated experimentation to determine the effects of other stressors on immune response.

John Casselman felt that we should underline the concern about eel transport. He asked if there was any control on the transport of eels as bait. Heather suggested that we check with Lydia about that question. Rob noted that both Commissions participating should address this issue (GLFC and ASMFC).

Marci turned to **Question 7**: What do we know about what limits *A. crassus*? Heather noted that temperature, and freshwater versus estuarine don't appear to be factors. She asked about full-strength seawater. Ken Oliveira responded there neither appear to be limiting factors.

Question 8: What other threats would synergistically increase the negative effect of parasite infection. Heather noted urbanization clearly is a factor, as well as other contaminants.

John Casselman noted that he didn't believe the parasite was a factor in the mid-1980's die-off.

Heather indicated she wanted to return to question 7. If we know what the intermediate hosts are, can we assume that eels that never go into freshwater can be infected? Ken and Bob noted they could. Ken noted that sticklebacks and other estuarine species can carry the parasite. Bob noted the potential intermediate hosts were very broad.

Karin asked about "cleaning" eels with nematocides.

Len noted some studies had been tried in Europe, and they couldn't find an easy control.

Wilson noted that he, Bob and Joe had discussed treating eels, and had concluded on the Roanoke at least, that if clean eels were passed upstream, they would likely just be affected again.

9:52 a.m.: Heather asked that we do introductions again, since everyone was now present. Everyone did so. Marci introduced herself. Marie noted that there was a possible Appendix 2 listing of the American eel, so anyone with thoughts about that could talk to her. Heather Bell noted she was a desert mammalogist by training, and that is why she is glad the rest of us are here. Heather Lutz. John Dettmers noted the Great Lakes Fishery Commission is very interested in this process and pleased to be here. Alastair Mathers. Paul Angermeier. Rob McGregor. Len Machut. Ken Oliveira. Tim Brush. Bob Graham. Jim Synder. Neil Patterson, Jr. Catherine Couillard. Guy Verreault. Dawn Dittman. Karin Limburg. John Casselman. Wilson Laney. Michael Twohey. Margaret Wooster, former Director of Great Lakes United, is writing a book and looking at Atlantic salmon and eels. Marty Miller. Kofi FynnAikens. Kim Damon-Randall. David Perkins. Steve Patch. Amy Roe.

10:02 a.m.: Marci excused us for break and asked that we return at twenty after the hour.

10:00 - 10:20 *Break (hosted by GLFC)*

10:20 – 11:00 Lake Presentations (John Casselman)

10:22 a.m.: John began his presentation. He noted that he was going to give us a quick run-through of the upper St. Lawrence stock and put it in context. He noted that some of the people in the room hadn't seen the work. He noted it was updated to 2005, in most cases. For the long-time datasets, the trends are the same. Several other groups have now assembled similar datasets, and the agreements are excellent. John noted that he is confident that these datasets are now final. The DFO datasets are also final. The presentation will also review the US datasets. He will present an examination of the decline we are seeing, put in terms of context. The COSEWIC document is in final draft. He has brought a draft that examines the Canadian population and puts it in context. He has eight or ten slides on that, and will talk

about changes in relative fecundity, and will look at it in the context of changes over time as well. The Canadian subpopulation will be put in context.

John gave us some background. The eel resource was long-valued and heavily used resource across the extensive eastern North American range. Since the mid-1980s, eel catches have declined across most of the range, and more dramatically in the 1990s, particularly in the St. Lawrence.

John reviewed the chronology of prehistoric and early historical significance. In 1100-1600, prehistoric St. Lawrence tribes used the eel extensively, and it was similar to salmon on the Pacific coast. John provided an extensive cultural history of the use of American eels. In 1634, the Montagnais autumn fishery used net-mouthed weirs and caught 200-300 eels per lift, and St. Lawrence. In 1688, French missionaries reported that a good fisherman of the Illinois nation could spear as many as 60 eels a day (and these came up the Mississippi). In 1916, 100 tons of eels were annually trapped from Oneida Lake, a tributary to Lake Ontario. Construction of dams eliminated this resource.

John showed the up to date data documenting the recent decline of American eels. The catch of eels started to decline in the US about six years prior to the Canadian decline.

John noted the commercial price of eels. As catch declined, the value increased.

John looked at American eel harvest by regions. He divided the east coast into eight regions: 1) southern states; 2) central states; 3) northern states; 4) Scotia-Fundy Region; 5) Gulf Region (of the St. Lawrence); 6) Newfoundland Region; 7) Lower St. Lawrence River; and 8) Upper St. Lawrence River and Lake Ontario.

The southern states, central states and northern states all show dramatic declines.

In Canada, the Newfoundland Region showed an initial increase, coincident with the groundfish closures. In the Gulf, the harvest seems to be increasing recently. John noted the major east coast buyer is in New Brunswick.

John moved to the Upper St. Lawrence, and noted that Guy could be presenting this part of the presentation. He noted that many of the fisheries are very old and traditional. Some of them are based on silver eels. He noted he didn't present the silver eel data. Guy noted that silver eels compose 70 percent of the catch. John noted that was an important point.

John noted the commercial catch had declined precipitously recently and was closed in 2004. John noted that in Lake Ontario, the decline was earlier, and progressively later as you move downstream. In Lake St. Francis, the decline has been less, but the fishery is very small.

John showed recent data from the eel ladder at Moses Saunders Dam. The eels there are older. In 1982-82, there were thousands of eels a day moving up the ladder. Today there are only 50 a day. There has been a slight pulse up, to 200 a day, recently, but this is merely a shadow of the former numbers. John noted the spike is in the fall, and has apparently always been there, but was previously masked. What this means is that there has been a little bit of recruitment.

John showed an index derived from trawling. The index shows a decline beginning in the 1990s, and recently they are catching no eels.

John showed data from eastern Lake Ontario electrofishing. He noted the data show some interesting day-night patterns, he thought because eels were using interstitial spaces in the rocks, which have since been filled by zebra mussels. His catch is up a bit in 2005. He is contract-fishing for the government.

John showed us the correlation between the eel ladder and the electrofishing catches, and they correlate well, with a five-year lag factored in.

John reviewed habitat changes. He noted that substrate has changed. Zebra mussels came into the system, and initially increased but have now decreased. The dead shells, however, are still there in the interstitial spaces, which can't be purged or cleaned. The rock piles that used to be suitable eel habitat

are now mounds. John proposed that optimum growth temperature for eel was in the interstitial space, now that they go offshore they likely don't have that optimum growth temperature. John noted there are huge windrows of dead mussel shells on the shore. John noted the water clarity has increased dramatically, such that you can see the bottom in 24 m depths. There has also been a change in the fish community and nutrient level, alewife have increased dramatically. John showed us the head of Main Duck Island in 1983 and it was much more turbid. John showed us a photo of an American eel, taken by a diver, and it exhibited scratches on its head, which John attributed to zebra mussels.

John showed us data on the harvest of eels, versus the zebra mussel expansion, which shows that eels declined markedly after the mussel invasion, but a distinct "signal" is not there to connect the eel decline with the mussel invasion.

John showed us model estimates of cumulative fishing, mortality and exploitation rates for a cohort of eels ascending the ladder.

Fisheries are being closed, harvest reductions are proposed, and safer downstream passage in the St. Lawrence River system is now being examined. Fecundity of the large-bodied SLR-LO eel stock may provide more than 25-30 percent of the total reproductive capacity of the species. This is being examined in detail.

John noted there is synchrony now between the European and American eel (St. Lawrence) stocks, and he believes this is related to the North Atlantic Oscillation Index (NAOI).

In summary, loss of recruitment at the extremities of the range is strong evidence of a universal decline in this panmictic species and forewarns continued declines. The question John asked is, will they disappear from the Great Lakes Basin?

John noted the other presentations he has, attempt to put the upper St. Lawrence system in context.

10:52 a.m.: Marci asked for questions. Dave Perkins asked what they used for area, for the electrofishing. John indicated they put GPS on the boat, and measure it using that. Dave asked, when he made estimates of historic abundance, how did he translate that to area. John noted there was hooking, spearing and electrofish. He noted the hooking was illegal. He interviewed some of the fishermen, and asked them about the areas they fished. He also had some seining estimates as well.

Bob Graham asked about the decline in abundance in the mid-1980s, to what was that attributable. John indicated it was Mirex, a contaminant. Eels going to Europe were tested and rejected by markets there. New markets were found that would accept the product.

Paul Angermeier asked about the data from Lake Oneida. He wondered how extensive such data were. He noted that the Illinois data showed 100 T annual catches. He suggested that we could use such data to generate some estimates of what the historic patterns were. Paul noted the Illinois eels were likely large females as well, and suggested that they would have been making a major contribution to fecundity as well.

John agreed that in some areas they had to be very abundant. He noted that when eels are caught now in the upper Mississippi, that is printed in the local newspaper.

Catherine asked about the impact of zebra mussels on the eels. She wondered if the eels had suffered any dietary change, because of their shift in habitat use. John noted that was a good question. He noted the change in alewife abundance over time. He felt that would be difficult to factor out, but it might be easier to assess the change in eel behavior. John noted that the optimal temperature for eel growth was present in the rocky shoreline, but the temperature is lower offshore, and may be sub-optimal.

Tim Brush asked John to return to one of his earlier slides for commercial harvest. Tim noted the figure was used in the ASMFC FMP, and he had questions about it then, but they were never addressed. John projected the slide of the southern, central and northern state landings. Tim noted that he had commented, if you went back to the 1970s, the only conclusion you could reach was that there had been a decline. He noted that no one had asked, what about the prior period, when eels were at a low level as

well. He asked if there could be a cyclic pattern. John noted that the early data reflect a period in which eels weren't targeted, so he would not use the early data. The eels were literally fed to chickens. In the period from the 1970's on, the price is going up, and the catch is going down, so John feels that it is legitimate to examine. He agreed with Tim that effort is problematic, but we have to use the data. Harvest or catch, is a reflection of recruitment, which is in turn a reflection of abundance. For the early period, when eels were chicken feed and worth only five cents a pound, you can't use them. Kim noted there is also reduced effort, as a consequence of World War II. John pointed out the war effect. He noted the silver eel weirs were not fished in the lower St. Lawrence, because of German submarines. Dave noted it sure appeared they were feeding a lot of chickens back in 1915. Paul stated that mapping reproductive potential will be essential to understanding population dynamics – we need measurement of escapement.

11:00 – 12:15 Lake Session (Lead – Heather)

Marci referred us to **Question 1d**): What do we know about the importance of specific lake habitat features that are “essential” for the sustainability of the eel population? Heather asked about the importance of the interstitial spaces between the rocks, was in escape from predation? John speculated that was part of it, but also it might be light. Heather clarified that it was a refuge, from predation, or light. John noted that eels were actually in the silt, during the summer, and asked what the oxygen levels might be.

Dawn commented, regarding burying or hiding, that this might have something to do with energy conservation, while they are not actively hunting. Dawn noted she had started seeing the same phenomenon in the Niagara and St. Lawrence. Ken noted that in sluggish rivers, he gets 95 percent of the eels in some locations popping out of the mud. This was especially true in RI.

John noted the oxygen content, or decomposition level of the silt, or the oxygen demand, has to be critically important. The eels must be existing at depressed oxygen levels, in the silt, so they must be extremely tolerant of it. There is a direct relationship between oxygen and growth, so if we degrade the habitat, the cover, then we are negatively affecting the growth of eels.

Ken noted that he has sampled in areas of silt, where the densities were just as high as in rocks.

Dawn noted, regarding mussels, she is seeing anoxic effects immediately below mussel beds. She suggested that eels might be impacted by mussels depressing the oxygen in shallow habitats. Heather asked if we had any idea what the oxygen threshold was for eels. She noted that one paper she had provided to us did find oxygen issues for eels.

John stated eels were much more tolerant of low dissolved oxygen (DO) than other species. He noted eels do bury themselves in silt during the winter. He noted they can live in a wet bag for days, but still felt that there would be direct adverse impact of degraded habitat.

Catherine asked if hypoxia in the bottom of the estuary could affect the migration of eels. John advised that was a great question. He noted they had this discussion in the eel working group. He wasn't sure at what depth they migrate, on their way out of the estuary.

Dawn noted the hypoxic water in Chesapeake Bay, and wondered about the correlation between that and American eel populations. She agreed it was an important question to ask about that relationship.

Jim asked if the die-off of zebra mussels could be contributing to fish die-offs as well. John indicated that he hadn't seen any kills related to oxygen depression, that might come from biological oxygen demand (BOD) related to zebra mussel die-off, but he noted that we couldn't measure what was happening in the interstitial spaces very well. He noted that he was more concerned about other invertebrate species. He noted that the debris from dead zebra mussel shells does change the dynamics of the substrate and habitat.

Heather asked about the density of zebra mussels in US waters. Someone said it was present in Lake Champlain. Dawn stated they are in all the finger lakes. Heather asked about the mid-Atlantic. Karin noted that if the dissolved calcium (CA) levels were 2 mg or higher, it would support them.

Marci noted that from one map she had seen, zebra mussels appeared to be moving along Interstate highways, on boats.

John noted that we need to be careful how we interpret the habitat changes. He noted the carrying capacity for Lake Ontario for eels, is very much higher than the number of eels currently there, but we just don't know if we could ever return to historic levels.

Heather clarified if the zebra mussels were changing quality, or quantity? John advised it was both; we're just not sure how much.

Rob McGregor noted that when zebra mussels first appeared, they thought they would lose walleye and yellow perch, but, the fish moved offshore. Anglers had to change their practices. Rob stated he didn't believe that zebra mussels would be a major factor in the decline of eels. He did agree that the zebra mussels would be affecting the use of the habitat. It is another factor chipping away at what we have to deal with, but not driving the decline.

Alastair noted in the past, there was probably a diversity of habitats, in which they could find appropriate temperatures and prey. It is difficult to say whether it is essential, because the species is so plastic.

Paul noted that zebra mussels don't appear to do as well in rivers, as in lakes. Karin voiced dissent. John Dettmers noted that zebra mussels have changed the habitat, but he didn't believe they are having a strong impact on the population. John noted the signals just aren't there, in John's dataset. He felt that access was a much larger issue. Access in the Mississippi River is an important issue as well.

Dawn noted that zebra mussel densities in streams had been documented as high as in lakes.

Marci moved to **Question #3**: How best can we characterize the changes in lake habitat over time? Heather noted the water is clearer, which might be a good thing. John Casselman noted that eels are benthivores as young fish, and live inshore. In Lake Ontario, with about a 20-year generation time, the eels enter at age seven and are there for a long time. They are benthivores and inshore, but about three years before they leave, they become piscivorous. The fishermen know this. They target eels using lines. The American eels seem to finish themselves off (prepare for migration) with high-energy prey, like alewife.

Catherine asked if anyone has looked in eel tissues, to see if levels of thiamine are decreasing. John noted that someone is looking at that question, to see how spawning might be affected.

John noted that alewife invaded Lake Ontario in 1878, so if there is something going on, it must be a recent phenomenon, because alewife have been a part of their diet for a long time. Eels in Lake Ontario got up to 18 pounds. John noted that the system has been managed, using salmonids and other species, so the alewives have been partitioned.

John Dettmers asked, given that so many other predators in the Great Lakes are consuming round gobies, are eels consuming them as well? John advised they are. He noted that if eels had been present in the densities they were in the past, round gobies couldn't have established themselves as quickly.

Bob Graham noted his understanding was that alewives had replaced chubs and floaters, so wondered if those were important in the past. John noted the alewives had become important a long time ago.

Bob Graham noted that some of the EPA indices might be useful in trying to assess the trends in water quality over time.

Rob McGregor noted that he had sent Heather some GIS data for Lake Ontario, which showed a lot of barriers prohibiting eels from access to the historic habitat. He felt that this was a factor that needed to be addressed. There are a lot of habitats that can't be used today.

11:34 a.m.: Marci asked John to make his other presentations. John made two short presentations, one from the COSWEC report, and the other that looks at changes over time.

While John was loading his presentations, Heather noted that the main points she would make to the public about changes in habitat would be the changes in the fish community, and ...XXX.....Rob suggested that the shift in distribution of American eels due to the addition of zebra mussels to the fauna was another change. Heather agreed and added that one. She suggested also that contaminants might be a factor.

John made his presentation on Canadian Eel Subpopulations in the North American Context. This was done for the COSEWIC Status Review. John began with a map of the Canadian subpopulations. The units they are using are Ecological freshwater units. He noted he prefers to talk about the Gulf of St. Lawrence, and Scotian-Bay of Fundy. He reviewed the new units. The units are: FEA1, Western St. Lawrence; FEA2, Eastern St. Lawrence; FEA3, Maritimes; FEA4, Atlantic Islands; and FEA5, Eastern Arctic. John noted that some of these really needed to be subdivided.

John noted the first thing they tried to do was to determine the contribution of different components of recruitment to the mix. They used discharge as a measure, and also used the harvest data, mean annual landings. He wondered if the harvest data were related to abundance, and noted they are making that assumption. They know there were dramatic changes after 1989. They assumed that the landings are some measure of abundance. They agonized over the Mississippi, and excluded it because they don't believe that there is either significant recruitment, or export, to/from the Mississippi. John used mean discharge in cubic m per second, and the mean annual landings, in tons. The US and Canadian harvests were about equal in the period studied (1970-1989).

John turned to the relative contribution of American eel rearing regions to total egg production, based on the assumption that production of silver eels, by number, is proportional to mean freshwater discharge. Everything was standardized to silver eel production for the US eastern seaboard of 1,000. Based on their calculations, the St. Lawrence contributed about 60 percent of total egg production; the Canadian Maritimes contributed about 28 percent, and US eastern seaboard, 13 percent. If the Mississippi Basin was included it was estimated at 17.6 percent of total egg production, the St. Lawrence contributed about 49 percent; the Canadian Maritimes contributed about 23 percent, and US eastern seaboard, 10 percent.

John showed us Table 3 from the report, which depicts migration periods, mean length, and age of female silver eels exiting Canadian freshwater systems.

Table 5 depicted the mean values of major long-term American eel abundance indices in an early period, compared with 1999-2000. The point was to see how eels have changed, comparing 1999-2003, to an early period (1970-1977). The percent change is downward, in almost every case. Two of the indexes were up, one slightly and the other more. The early period year range varies from one region to another. John noted that the COSEWIC is trying to put the declines into context. He didn't believe that one unit would be treated differently than another.

11:53 a.m.: John moved to his second presentation, which would focus on trends over time. John noted they had been interested in, if you assume that harvest in some sense is an index of abundance, what pattern you would see if you look at changes in relative contribution to the overall fecundity of the species, with special reference to the St. Lawrence River-Lake Ontario portion of the stock.

Their objectives were: to examine the commercial harvest of the species across the range by region, emphasizing the past 50 years and the ST. Lawrence River-LO system; use of harvest data to estimate changes in the relative contribution to overall fecundity of the species; and changes in relative contribution of the St. Lawrence River – LO stock and species fecundity.

They used the regions he used in his original presentation. He noted the proportion of females in the harvest had to be taken into account. In the upper St. Lawrence, the proportion is 100 percent females. The size of the eels is dramatically different from one region to another. Fecundity is much higher in the St. Lawrence River system, much more so than in the more southerly regions. In millions of eggs, the range is from nearly 14 million eggs, relative to about 4 million in the central region of the US Atlantic coast. John noted that the commercial harvest was really quite large in the central US states.

John reviewed the relative contribution to the overall fecundity of the species, by region. He noted that the central states appear to be very important, while the southern states don't appear to be very important. John noted that for the St. Lawrence-Lake Ontario, the contribution to fecundity has decreased in steps through time. He noted that he understood the limitations of using harvest data to assess contribution to fecundity. John noted that if the contribution of the St. Lawrence-Lake Ontario was as high as they believed, it is really important.

Paul Angermeier noted the concept of contribution to fecundity was really important. He asked John to clarify the method he used. He asked if the numbers John was using are post-harvest? John noted they didn't know how to get to that answer. He noted they are trying to look at both immigration into the lake, and emigration from the lake. They are using the electrofishing to estimate eel abundance in the lake. They are using Guy's data to measure the eels going out. Paul asked if there was any more harvest after the eels left. John noted for the silver eels, there wasn't any more harvest, for the ones in the lake, there is.

Paul suggested that two maps were needed; one showing how many egg bearers were getting to the Sargasso Sea. John noted the second map was key, and no one knows how to do that one. John agreed that was a critical question.

Joe Hightower asked, going back to the earlier presentation and discussion of discharge, he was trying to remember what the numbers looked like, and the landings associated with them. John noted they were different numbers. Joe noted that harvest compared to discharge, in the US, suggested that perhaps we might be fishing too hard in the US. John agreed that was an important point. He indicated that he had a big concern about the discharge.

Paul asked if discharge was a surrogate for wetted habitat. John confirmed it was. He noted that Martin suggested it was a measure of the attractiveness for habitat. Paul asked if there was any way to validate the relationship. John noted that you could, if you examined things carefully.

Wilson noted that if you are measuring discharge today, you really aren't measuring "attractiveness," because of all the barriers we discussed earlier. The second point is that the landings data from periods other than the very most recent are highly questionable, because there was no mandatory reporting. John agreed and noted that the approach needs a lot of refinement.

Heather wondered about the utility of assessing European data. She asked us to consider the fact that she has to consider the question of listing, rangewide, and John's analysis is just for North America. She noted the species ranges all the way to Brazil.

Rob asked about any provision for designate able units, in the US. She noted we did have a provision for Distinct Population Segments (DPS). Heather noted that Marty could address that for us, if need be. She asked that we take a look at the **definitions** that she had provided to us.

Paul noted that he wanted to volunteer to attend the Brazilian-Caribbean workshop.

John encouraged Heather to decide early on, what units she was going to use in the landscape, for the entire range.

June DeWeese asked John if we know whether historic units, such as Chesapeake Bay, have been mapped out for us?

John was not aware of any. Guy noted that portions of Canada, along the north shore, have been mapped out and the water bodies are smaller and less productive. John agreed with Paul that an important question is: are there some portions of the range producing eels that we aren't covering?

Marci addressed the location of nearby restaurants. There is one in the hotel, which Mike advised is the handiest place to eat. Marci recommended the microbrewery for dinner, and the hotel restaurant for lunch, because of its proximity. Heather suggested we all go to the microbrewery for dinner.

Marci asked us all to return prior to 1:30 p.m.

Heather asked that FWS colleagues please eat lunch with her downstairs in the restaurant, so we could regroup.

12:15-1:30 *Lunch on your own*

1:30-2:00 Stock Assessment & Peer Review presentation (Joe Hightower)

Marci introduced Joe for his presentation. Joe indicated that he would focus on the first two terms of reference, since he felt those were of most interest to us for the Status Review. They were the various models that could be used, as well as the data sets that could have been used.

Joe noted that the data are brought up to date, but are not really unique. Joe noted there are some other fishery-independent data sets, such as the Delaware River survey, that should have been used. It shows a different trend. Some of the data sets show a decline, but others don't. Joe indicated we would look at those. Joe indicated he would review the basis for selection of the data sets, and development of coastwide fishery-independent and fishery-dependent indices. He noted that in his opinion, it is not good to combine all the data sets into one analysis, because trends can be obscured.

Joe noted there are two glass eel time series, from Little Egg Inlet, NJ, and Beaufort Inlet, NC, which don't show particular trends. Joe and the peer reviewers thought this was an important piece of information. Joe noted the Stock Assessment Subcommittee (SAS) noted there wasn't much of a trend between the two site, but the peer reviewers didn't believe that was a problem. The SAS was also concerned about variability, but the peer reviewers didn't believe this was particularly bad, unless the sample size was really small. Joe noted they preferred to have confidence intervals, which weren't displayed on the graphs. The two time series run from 1986 to 2003. Wilson asked Joe if there was any ability to mine those data sets for additional information. Joe stated you could generate variances, but the key point was that the data sets may show there isn't any problem with recruitment. John Casselman suggested there are some other things, such as looking at the size and seasonality of the migrating glass eels. He noted the location of the two sampling sites might be a factor, and that other sites might show a decline. Joe agreed. John asked for clarification of how the comments of the peer reviewers would influence the ASMFC management process. Lydia explained that both reports would go to the American Eel Management Board, and they would assess them and make decisions accordingly.

Marci asked Joe to return to his presentation. Joe noted he didn't mind taking questions as he went. Marci asked him how much time he would need. He noted that he only had nine slides and was already on the third one.

Joe turned to the ASMFC young-of-year surveys that had been initiated only relatively recently. Joe noted the peer reviewers thought these data would be very useful in the future.

Joe reviewed all the datasets that were included in the assessment. Joe noted that one concern the peer reviewers had was that the entire time block is not covered by all the datasets. They averaged over the surveys to generate a single value, so different years are included in the averages, and they were all equally weighted. That didn't seem to be a great idea. Joe noted he didn't have answers, but these were concerns they raised. One weakness is giving equal weight to things that represent different amounts of sampling, and different size watersheds. The solution for that might be to weight based on area.

Joe noted the stock assessment standardized each of the surveys, by dividing the mean by the standard error. In some cases, the mean was based on only three years, while in others it was based on 15 years. The result is biased. They re-did the analysis, and included it as part of the peer-reviewed report. The analysis was done as a general linear model, so it gets rid of one problem, but it still weights every survey individually. The graph Joe showed depicts each individual survey, as well as the overall trend. The overall pattern obtained is about the same, but Joe noted their analysis is much fancier, therefore he would claim it is much better. Joe noted the differences exhibited by the various survey datasets.

For the fishery-dependent data, Joe noted the SAS had used a Z-transformation, which the peer reviewers didn't like. He was skeptical about the usefulness of the analysis. He noted that you have to

decide whether commercial CPUE is a meaningful index of abundance. He doesn't believe it is. He noted that he felt that commercial fisherman want to exceed having their catch be an index of abundance.

Rob McGregor asked Joe for his view of the overall assessment, i.e., what were the key findings? Joe noted he forgot to mention the surplus production model, and would do that also. He thought the assessment brings together a lot of useful information. It doesn't provide an estimate of population, nor does it indicate how much mortality is due to fishing. They tried to use a surplus production model to do the latter, but it didn't work very well. The model couldn't sort out why the various surveys were showing different trends. They couldn't estimate a fishing mortality rate. The quality of the data is pretty poor, in comparison to other ASMFC species. There is very little, other than landings, and people question those. Joe noted that the SAS decided the only things they could do were to look at trends. He noted that one criticism of the assessment is that they came up with a subset of the surveys, to use in the final status, but they didn't use all the datasets, the ones that show other patterns. It is hard to decide what the status of the stock is, because there is no consistent trend. The longer the datasets, the more likely it is to show the downward trend. Joe noted that it was hard to reconcile how Delaware Bay could be different from Chesapeake Bay.

Rob noted several statements from the assessment, including that the decline was substantial, and if it continued, recruitment failure could occur. Joe noted this was based on the observed pattern in the European eel, which was in recruitment failure.

Dave Perkins noted that the two glass eel indices didn't show any trend. Joe noted the two sites are well-separated.

John noted the two datasets are in different locations, but the northern dataset does show some change in size, and timing. Wilson noted that the two sites are in the southern part of the range, so if recruitment is ocean-driven, we would expect to see less change there. John noted the two sites are actually in the central part of the range, rather than the southern (North of the Charleston Bump).

Paul asked if the peer review panel made any recommendations to the SAS. Joe noted that there were things that could be done to establish criteria for using surveys. He noted that a ten-year minimum might be good for including surveys. He noted also that ICES had some unique methods they applied to eels, and they could have been applied here as well. Longer-term, Joe noted since they were university types, they came up with proposed research that could be done. They didn't address the management issues. Lydia concurred that the Board makes those decisions.

Rob McGregor noted he liked Joe's point about needing a minimum length of time series. He suggested that such short time series shouldn't be included.

Joe agreed. He noted the GLM approach they used did still give some weight to all the included surveys.

Rob asked if the prevailing thought was that if recruitment of glass eels was low, they didn't need to migrate as far upstream. Joe thought that was in John's (Casselman) article.

Karin asked to see the last slide again. She noted she was thinking about John's comment earlier, about prices, and asked if it would mimic the pattern that the fishery-dependent data exhibit. John wasn't sure it would.

Paul followed up on Rob's question. He noted he wasn't a leptocephalus expert, but from his reading, the detraining of leptocephalus larvae wasn't particularly density-dependent. Ken Oliveira agreed with that assessment. Rob noted that when trying to explain to people that knew even less about eels than him, such as ministers, the concept of density-dependence was useful, but he wanted to make sure that was the prevailing thinking.

John Casselman indicated that if density-dependence comes into play, it does so after detraining. John stated that eels piling up below a dam was a demonstration of that phenomenon.

Bob Graham noted that his comment was along the same lines as John's.

Heather asked if there was any glass eel data from Canada, either fishery-dependent, or fishery-independent. John stated they have both, but they are very short-term, maybe ten years of data, and they are in two separate data sets. They don't have any data that could be compared to the two long-term data sets from NJ and NC.

Heather noted John had stated that the Scotia-Fundy area had to be treated differently from the Gulf of St. Lawrence, because the recruitment mechanisms are different. He briefly reviewed the available datasets, one of which is from Brian Jessop's work.

2:00 – 2:45 Discussion

Marci asked that we address the stock assessment questions. **The overarching question** was: What areas of uncertainty have the greatest potential influence on our listing decision? The first sub-question was: **Question 1.** What factors contributed to the reported eel decline in the past two decades? Harvest in the late 1970's? Full effect of the Conowingo Dam? NAOI? Heather noted that some authors believe that fishing and the dam were major factors. Bob Graham noted that we don't have any idea about the timing of spawning by outmigrating females, either.

Tim Brush noted Conowingo was built in 1928, and Holtwood in 1910, a pretty long time ago.

Joe Hightower noted that all the harvest on American eel is pre-spawning, which is the worst possible case for an exploited species. Joe noted the modeling work that has been done suggests that the impact on females is particularly high, since you are fishing them year after year.

Marci asked **Question 2:** Is it likely that the decline will continue? No one had an answer.

Question 3: Have the effects of some contributing factors (e.g. dam construction) been fully realized and hence expected to cause no further decline? Tim Brush noted that there have been no significant numbers of new dams. He felt that the large effect due to damming has been realized, and may be ongoing. With respect to the Susquehanna system, which is about 50 percent of the Bay's input, it would have clearly been important, but other systems, such as the Potomac, and other Virginia rivers, should be providing ongoing recruitment. Bob Graham noted that if you look at the VIMS data, all the rivers show the same trend, but the Rappahannock only has one dam.

John Casselman stated these are pretty overwhelming questions. He asked, are we seeing some dams retrofitted for turbines, that didn't have them before? If that is true, then the impact could increase, and certainly will be ongoing. He noted that Moses Saunders was put in in the late 50s or early 60s. He noted that we are dealing with different species generation times, latitudinally. He noted in the St. Lawrence, 20 years is the generation time, and he felt that it took three generation times to see any effect.

Rob stated that for Lake Ontario, he felt the decline would continue, unless we get a bump in recruitment. He asked; when you get down to the low levels that are present in Lake Ontario and the St. Lawrence, if every mature female wasn't worth a whole lot more? Heather asked if Rob meant rangewide, or Lake Ontario. He meant Lake Ontario.

Karin agreed with Rob that it would take generations for all this to play out. She also stated she felt the impact of dams was under-rated. She noted for MD alone, there are lots of them. Tim Brush noted his statement about the impact having been realized was based on the mid-Atlantic, because most of those dams were built a long time ago.

Joe noted that the peer reviewers wished they had more information about how much of the habitat above the dams really is accessible. He noted they had the Busch et al. report, but that doesn't document how much of the habitat is accessible to some degree. Also, they didn't know to what degree turbine mortality is a factor.

John Casselman noted he felt the declines will continue. He felt that 25-30 percent of overall stock fecundity came from the St. Lawrence, in recent times, and he didn't see any way to "make up" that

fecundity. His feeling is the decline will continue, and be accelerated, due to the loss of the St. Lawrence contribution.

Question 4: Are there new factors, the effects of which have not been fully realized? Heather suggested one might be the new nematode parasite. Wilson asked if viruses might be an issue. Paul noted that invasive exotics are on the up rise, so we have no idea what sorts could pop up that might impact eels. He noted there is a huge amount of uncertainty associated with that issue, but we can be certain there will be more of them.

Rob noted there is a big push for cleaner energy sources, and in Ontario, there is a push for more dams. Heather noted the President made the same commitment in his State of the Union address. Heather noted the President's statement was not definitive enough, but if Ontario had definitive plans, that would be useful information. Marci noted that she had heard the argument more than once that if the resource was already impacted, additional stresses won't hurt them.

Tim Brush made the point that the increase in the cormorant population might be a factor. Wilson noted that one researcher at the AFS meeting in Anchorage had made a presentation, which he thought involved eels, that documented a high rate of predation. John Casselman felt that cormorant predation on eels would be a minor factor, since cormorants preferred schooling species. He did state they had observed eel otoliths in cormorant stomachs.

Catherine suggested that emerging contaminants, as well as global climate change, could be factors in further declines.

Marci turned to **Question 5:** How is recruitment failure defined in the peer-review report? Joe responded there isn't much bumpiness in the trends, they are just going down. Heather noted to her, "recruitment failure" equates to the loss of the species, i.e., extinction from the planet, but that as a fishery term it may have a different meaning. Joe noted the Decker article indicates that the European eel recruitment is below the sustainable level (in a follow-up email Joe provided the following clarification regarding "recruitment failure". My take on recruitment failure is that we hypothesized that a continued decline in yellow eel abundance could result (sooner or later) in a spawning stock that produced very poor recruitment. It would be a similar scenario to what Dekker suggested for the European eel. He has argued that below an apparent threshold spawning stock size, recruitment has been very poor. This makes it difficult to rebuild the stock because the odds of getting a big year class are very low. It doesn't mean that there is no recruitment but just that recruitment is very low in relative terms and doesn't support the stock at its historical level. Recruitment "failure" might be observed when there is a juvenile abundance survey and higher values are observed in the early years, then values close to zero in later years. The Albemarle Sound stocks of blueback herring and striped bass are examples where JAI values were close to zero for a number of years. In the case of striped bass, severe reductions in harvest along with regulation of river flows resulted in a rebuilding of the stock and now strong year classes (high JAI values) occur regularly.). Wilson noted that "sustainable" refers to the "sustainable" fishery (not sustainable population). John Dettmers noted he generally agreed with Wilson's assessment and that often, the effect could continue to snowball downward. Marci asked Heather if she was satisfied. Heather asked Joe to further clarify.

Joe used striped bass as an example. He noted the Roanoke stock of striped bass had exhibited a higher average juvenile abundance index (JAI), but then it went lower almost to zero, but ultimately was restored. Wilson suggested that there was a relationship between recruitment levels, and sustainable fisheries, and ultimate extinction, given that if recruitment was maintained at a low enough level, for a long enough time period, the risk of extinction would be greater.

John Casselman noted that the eel was a panmictic species, and not a local stock, as was striped bass on the Roanoke.

Heather indicated that she would talk to Joe and Lydia about the stock assessment and how it should be interpreted. (in a follow-up email Joe provided the following as a response to **Question 6** - Given the data sources used, is it appropriate to make inferences at the species range-wide, including the marine component, from the stock assessment? In Joe's opinion it is difficult to make range-wide conclusions from the stock assessment because surveys from different areas show different patterns and because the

glass eel surveys do not indicate a recruitment decline for the section of the coast that they cover (U. S. mid-Atlantic). It is very clear that the upper end of the stock's range (St. Lawrence) is in severe decline but that could be due to oceanographic factors. He has a hard time relating the St. Lawrence decline to spawning stock size when the pattern is not consistent rangewide. Regarding the marine component, I think the otolith microchemistry work needs to make a clearer separation between estuarine and true marine distribution. It seemed to me that they were sometimes lumped together. Also, as I mentioned at the meeting, I am skeptical until there are other kinds of data (e.g. surveys) indicating significant eel biomass offshore.)

(Question 8 was sent to Joe in a follow-up email as we were running short on time. Chesapeake Bay shows a 50% decline (94-04) how much of this could be a regional phenomenon given that the harvest here is so great (50% of all commercial landings from 50-00) and targets yellow eels? It seems possible to me. Unfortunately the stock assessment did not make any evaluation of the impacts of fishing. It would be possible (with significant effort) to generate density estimates for the bay and compare those to harvest but it wasn't done in the assessment.)

Heather moved to **Question 9**: Uphoff and Weeder identified overfishing of eels in the Chesapeake Bay, but acknowledged that this was based on a "closed system," how relevant then is the "overfished" statement in a status review given that it is not a closed system? Bob Graham noted he thought that Julie and Jim were just trying to document the approach they had taken in their analysis. Paul noted that perhaps they were trying to say, if the same level of fishing was occurring in Chesapeake Bay, rangewide, then the impacts could be greater. Joe stated that perhaps they were trying to state that some level of escapement is necessary, to allow some contribution to the spawning stock. Kim Damon-Randall stated that is exactly what they were trying to say.

2:45 p.m.: Marci excused us for break

2:45-3:00 *Break (hosted by GLFC)*

3:00 – 3:30 Conceptual Model of Annual Abundance Presentation (Paul Angermeier)

3:00 p.m.: Paul Angermeier noted his objectives were to review the 2005 results of the population dynamics workshop he had convened; review literature for 2006 USFWS workshop relevant to how threats affect eel population dynamics; and, explore utility of conceptual model as tool for organizing scientific knowledge.

Paul noted that he was going to talk about conceptual models, not mathematical ones. He noted the models are useful to summarize data, identify data gaps, look at the entire life cycle, specific life stages, and specific regions; make assumptions, hypothetical relations explicit; suggest new hypotheses; and set the stage for additional studies, both future simulations and field studies.

Paul put the conceptual model for American eel on the screen, which he had sent out to workshop participants prior to his 2005 workshop. He apologized for the busyness of the diagram, and noted it reflected his lack of PowerPoint skills, more than anything else. He explained the diagram. Each stage is affected by growth, mortality and dispersal factors. The capital "L" refers to latitudinal effects. Paul noted the lower half of the diagram exhibited a dual track, depicting the fact that glass eels could either remain in the estuary, or move to freshwater. The original concept was that they would ultimately mature and all charge off together to spawn.

Paul reviewed the questionnaire responses he received from workshop participants. The summary of historical versus current population dynamics: observable declines began 1970-1985; limiting factors have changed substantially for yellows and adult (oceanic) migrants, and perhaps for leptocephali, and freshwater elvers; mortality, including fishing mortality may have changed substantially for nearly all states; dispersal may have changed substantially for all life stages;

Summary of causes of eel decline: Overall abundance is limited by number of yellows and spawners, and perhaps by number of freshwater silvers, leptocephali, and elvers; Growth limits yellows; Dispersal rates may limit number of yellows and spawners; Much uncertainty surrounding mechanisms of eel decline.

Top-priority assumption or uncertainty (model wide): aspects of dispersal; dispersal of yellows and silvers; effects of D on M and G; causes of mortality; mortality, sex determination and distribution; genetic influences on leptocephalus metamorphosis (is larval recruitment a random consequence of panmixia?); contribution of large females to overall recruitment; interactions among D, G, and sex determination; geographic variation in reproductive contribution

Summary of Gaps in knowledge of population dynamics: Distribution and abundance of leptocephali and elvers are poorly understood; Dispersal (all stages) and sex determination are poorly understood; Top-priority gaps to fill include various aspects of dispersal; Consensus on top-priority gaps is weak

Paul showed the revised conceptual model, after review by the workshop participants. He noted that more things were added to the model, making it even more complicated. Paul noted the other thing that came out of the discussion is that the vital processes are rarely measured, and there are multiple plausible relationships between growth, dispersal, mortality and sex determination. Paul noted he was surprised how little we know about sex determination in American eels. There are an awful lot of unknowns about how things really work, that are critical to our understanding.

Paul showed a table of data availability on the various eel life stages, including eggs, leptocephali, elvers, yellows, silvers and spawners. He noted there is a high frequency of "low" categories.

Paul noted he had breakout sessions that focused on various life stages, and asked them to identify factors affecting them and indicate how uncertain they were about their impact. Participants were asked to rate importance, uncertainty, geographic variation and uncertainty. Parameters rated were the number and location of eggs, transport, food availability, predation and contaminants, for leptocephali. All were viewed as of high or medium importance. There was some suggestion that there might be some gradients.

For elver abundance: factors identified were habitat loss, fishing, parasitism, predation (including by exotics) and contaminants. Importance values were from high to medium, uncertainty was from high to medium-low, geographic variation was from E-W, U-D, and F-E.

Joe Hightower asked for leptocephalus, what was meant by the number of eggs. Paul explained that it meant just that, the number of eggs produced, and not the spawning stock biomass (SSB).

For yellow eels: fishing and predation were the two threats identified. Paul noted he was surprised that habitat didn't emerge as a factor.

For silver eels: factors that emerged were barriers, fishing, predation and contaminants. It was noted that all factors operate sex-specifically. There were all sorts of spatial gradients identified. Predation and contaminants were deemed of high uncertainty.

Paul turned to his review of the 2006 workshops literature. He divided his conceptual model into six phases, to show some of the information he pulled from the literature and how it fits into the life cycle.

The first phase is egg to leptocephali, and we know next to nothing about that.

The second phase is leptocephali to elver. There is information suggested that changes in ocean currents, temperature and nutrients may affect survival; they are transported by surface (down to 300 m) currents; change in the Gulf Stream may impair transport, especially to high latitudes; they may be a general population bottleneck; there is greater than 99 percent mortality; growth may be affected by marine "snow;" detrainment is size-dependent.

From elvers to yellows: salinity-regime residency is established within one year of arrival; great variation in timing, extent, duration of inland movement; major declines in abundance observed; low pH induces high mortality; instantaneous daily mortality is about 6-7 percent. Threats include dams, fishing, habitat degradation and loss, and poor water quality. Paul noted that there is debate between those who believe that since some eels never go into freshwaters, inland habitats aren't essential to the survival of the population.

From yellows to silver (general): growth is greater in brackish water than fresh; age varies between brackish and fresh, and south to north; sex determination is density-dependent; inter-habitat shifts increase with age; trends in catch data are mostly negative; most fishing occurs in the estuary; reproductive potential especially is sensitive to fishing. More specifically, females grow larger than males; size, age and the percent female increase with distance inland; density is inversely related to the distance inland; survival is density-dependent; sex ration is female-biased, especially at high latitudes; there is wide variation in duration of the stay in freshwater; short-term movement is limited; pH less than 5.5 may be lethal; condition of individuals not affected by *Anguillicola* infection; and turbine mortality is size/sex – selective.

Paul noted that since there are eels that stay in estuarine waters, the idea of obligate catadromy is turned on its head. He created a new diagram, which is essentially the bottom half of the previous one, that begins with continental elvers, and ultimately leads to silvers starting to migrate. Elvers can decide to stay in marine, brackish or freshwaters. The ones that stay in marine waters go eventually to the silver stage. Estuarine ones can go back and forth. The freshwater ones eventually also transform to silver eels. Latitude seems to be an important correlate of this portion of the life cycle.

Paul turned to knowledge of eel population dynamics. For yellow to silver stages, there are five threats related to all those factors: dams, disease, exotic species, fishing and habitat degradation/loss.

For silvers to the available spawners: in general, fecundity and size increase with latitude; viral infection was lethal in simulated migration; parasite infection reduces swimming speed, endurance, and capacity; catch trends are mostly negative. For freshwater, movement rate is about 380 km downstream/year; mortality in St. Lawrence system during migration: 53% in lower 500 km; turbines > fishing

For available spawners to eggs: the probability of mating may be density-dependent (?); spawning timing and location could be affected by variable ocean currents (?); contaminants concentrate in the gonads; lipophilic contaminants impair gametogenesis, and fecundity (?); and fecundity is a function of size (assumed). The only threat for sure is that of contaminants.

Paul asked can the framework presented above, or another framework, help us organize the vast eel literature in a way that helps us meet this objective. He noted that a key objective was to evaluate and rank the importance of selected threats in regulating American eel distribution and abundance.

Karin noted she has a paper in press, addressing phenotypic variability in three species of eels. She noted by using otolith microchemistry, they found half a dozen typologies. They definitely found latitudinal effects, with more use of brackish and estuarine systems, the further north you go. The most complex typology was one they called nomadic. Heather indicated that she would make copies of the paper for everyone and scheduled Karin to present the next morning.

Catherine noted that including contaminants in the mix might make it even more complicated.

Paul agreed and felt that breaking the diagram into sections helped to make the concepts more manageable.

John Casselman noted that he was interested in the latitudinal effects and how he would deal with that. He asked if Paul planned to draw several lines for freshwater, based on the different subpopulations. Paul noted that was a possibility. He wasn't going to do any more analysis; this was just a way to organize the information. Paul noted that it was distance inland, as well as latitudinal.

Wilson noted that the otolith microchemistry has to be put into the context of the present-day landscape, in which eels can't migrate inland the way they used to do. He wasn't sure how it should be factored in. He noted that two recent studies of striped bass otolith microchemistry also showed multiple (up to four) life history strategies.

3:30 - 5:00 Discussion

Marci asked that we turn to the population dynamics questions. **Question 1:** How important are environmental factors and anthropomorphic factors to recruitment and population dynamics? Dave Perkins noted that it seemed from the first workshop that environmental stochasticity was a big factor in determining population size. Paul asked for clarification. Dave indicated that ocean currents were likely a major factor in determining recruitment. John Dettmers agreed with that statement, noting there is a lot of variability that isn't explained by the numbers of spawners, or eggs. John noted that with global warming, and the change in the [NAO...] he asked John Casselman to comment. John stated it wasn't just temperature, but also changes in ocean currents.

Paul noted that the fact that over 99 percent of the larvae never make it to the next stage, he was guessing that was a density-independent factor, but wasn't sure. Heather noted the 99 percent was a European number which travel much farther, not for American eels. We don't really know the number for American eels.

John noted that if the NAO affects precipitation, that effects discharge. He noted that in some areas, there may have been increases. John noted increases in discharge affect the migration of silvers, with high discharge years increasing the migration. John noted there are years when there were fluctuations in the NAO, and we saw no effect, but that could have been due to recruitment being higher. We have perhaps seen the oceanic effect in recent years, because recruitment is way down.

Heather indicated she wanted to spend more time on question one. Rob McGregor noted he felt there was still confusion regarding the definition of "environmental factors." Dave clarified that originally, they were thinking more of oceanic factors. Heather suggested that we split it into two groups, strictly environmental factors, and anthropogenic ones. John Casselman suggested the former were factors we thought we couldn't affect, but that might not be the case.

Paul noted that recruitment is another term that requires definition. He noted we have to be specific about which phase we are discussing, i.e., from egg to leptocephali, or from leptocephali to glass eel; etc.

Heather asked that we consider first only the phase from eggs to leptocephali. She felt that most of us would agree that if there was only one factor, it would be oceanic conditions. Heather raised the potential of marine snow as a factor. Wilson and Dawn explained what marine snow meant. Paul noted even if that was a factor, ocean currents were still the driver. Catherine felt that contaminants could be a factor, either in the eggs or larvae. She noted that we just don't know for eels, but there is a lot of lab work with other species that shows sublethal effects of contaminants on the eggs. Ken suggested that the impact of contaminants on eggs was an artifact of where the eels lived, so that wasn't necessarily oceanic. Paul noted that some eels never leave marine waters. Dawn noted that Thiamonase could be a factor.

Heather indicated she wanted us to start eliminating things, if they weren't really the main drivers, and began a diagram.

Marty noted that with salmon, increased predation due to reduced prey buffers resulting from changes in the fish communities was a factor. Heather noted we have no evidence of that for eels. Wilson noted it is hard to find leptocephali in stomach comments. Marci noted that if we are going with the most plausible, we are limited to oceanic currents and contaminants.

Heather asked if we needed anything different between leptocephali and glass eels. No one stated anything.

We moved to glass eels to elvers. Paul noted that we needed to decide whether to include glass eels as a life stage separate from leptocephali.

Dave Perkins asked about the relevance of the saltwater phase. Heather noted that she had put three stages, salt, brackish and fresh, but there was in actuality a gradient. Wilson noted that Brian Knights had stated there were eels that were entirely marine, off the coast of England. Karin noted that her paper addressed that issue to a degree. She indicated based on the otolith chemistry, their movements were really a smear. She indicated that it was hard to catch eels in marine habitats.

Paul asked how the eels sorted out, proportionately, across the three habitats. Karin noted you see a lot in brackish water, but that doesn't mean they began there. Some move up into freshwater, then back. Some grade their way up, and then back down. Paul asked if we could bound the numbers by percentages, from Karin's work. She felt you could do that with her data, and thought they had done so in the paper. Heather asked if tomorrow, we could build some of that in, after everyone gets a copy of the paper. Marci asked if Karin could give a presentation. She stated she could, but asked if that meant she had to drink less beer tonight. Marci stated perhaps that meant everyone should drink less beer.

Marci indicated that we would jump to the yellow phase, and postpone any further discussion of the oceanic phase until tomorrow.

Dave Perkins noted that for all the other life stages, the picture was even less clear. He asked if there were any other factors that clearly had a major role. Alastair noted that fishing was a big factor for yellow and silver eels. He noted to his knowledge there were no fisheries for marine American eels. Turbine mortality was included as another.

Paul noted the other side of the egg stage was a big factor, i.e., the number of spawners escaping the continent and leaving to spawn. Paul felt this was for the silver to egg stage. Dave noted that skipped all the other life stages. Heather noted it might be both the number and quality of spawners. Paul noted that we need some measure of how many escape.

Bob asked if it would help to bring up the slides that Paul used, showing the factors and their rankings. Paul noted he didn't want to discount that, but he didn't know that their currency was any more valuable than ours. Bob stated that gave us something to look at, as opposed to taking a shotgun approach.

Mike put up Paul's slide of the leptocephalus abundance factors. Marci asked Paul to explain the two uncertainty columns. He did so. Heather stated that it appeared that oceanic factors were the principle one explaining leptocephalus abundance.

We moved to factors regulating silver abundance. The four factors on the list were barriers, fishing, predation and contaminants. Paul asked if barriers referred only to turbine mortality. Wilson and John thought it was both that factor, and the preclusion of eels migrating further inland and becoming females, since that would ultimately reduce the number of silvers.

Dave Perkins asked Paul if the focus was range-wide. Paul confirmed that was the purpose of the workshop, to think grand.

John Casselman noted that his work suggested [the fishing mortality was about equal to the fishing mortality, for an incoming cohort.] Heather asked the utility representatives if there are areas where we have estimates of both turbine and fishing mortality. Karin wondered if we have data from the Carolinas. Joe advised we didn't have such data.

Catherine asked John Casselman how he was doing his modeling. He noted that he had the eel ladder data for the incoming cohort, and could calculate the exploitation rate in freshwaters for yellow and silver eels, and the turbine mortality has been assessed. He also has estimated the natural mortality, from a cohort that comes in, and allows for 25 percent escapement.

Heather indicated that she felt it was about time to wrap this discussion up, unless someone had anything additional. Rob wondered why fishing wasn't a more important factor for silvers, given that all fishing affects silver abundance. Heather agreed that it made sense to view fishing as a cumulative impact.

Catherine noted where she was getting to is that we can't estimate the effect of parasites and contaminants on the migrating silvers. We could lose a large percentage of the emigrated silvers, and not know it. She noted that we might know the number at the dam, but we might lose additional ones after that time. John Casselman agreed. He noted that they have estimated escapement as 27 percent. He noted the modeling is not simple. They thought that the yellow eel exploitation was only 3-4 percent, but when you add that up over time, it becomes significant.

Marci noted that we have a recommendation to adjourn for the day.

Wilson asked if the group wanted to see the elver data from below Roanoke Rapids Dam. They did. The two figures were projected and Bob Graham explained the results. Below the dam, captures of eels were very high during peaks of flow, both in the spring and fall, during full moon. Bob noted they are also still catching elvers at temperatures of 7 C.

The meeting adjourned about 4:40 p.m..

The FWS, NMFS and USGS staff met until 5:30 p.m. to discuss where we might go from this point onward and how best to use tomorrow's time.

Dinner on your own

Thursday 2nd

8:00 A.M.: Marci indicated that she wanted to introduce Lydia Munger, who wasn't here when we did introductions yesterday. Lydia introduced herself to the group.

8:00 – 8:30 Contaminants Presentation (Amy Roe)

Amy indicated that she would be giving a review of the literature review she did for American eel. She indicated she would discuss contaminants, geographic regions and impacts. She reviewed approximately 170 peer-reviewed journal articles, federal and state agency reports, and academic research. Once she received the data, she put them in a file by geographic region.

Contaminants of concern are those that are persistent, bioaccumulate and can have impacts. The most commonly reported substances were PCBs, dioxins, furans, pesticides such as DDT, Mirex, Chlordane, Dieldrin, Endrin and heavy metals such as mercury.

Amy reviewed the geographic scope of the data. Most of it came from the Lake Ontario region of Canada and the US. Data varied by rivers and sampling stations, time period, and tissue samples. Amy noted that she had been reluctant to specify a trend, but further analysis did indicate that PCB concentrations have generally declined over time in some of the geographic regions such as the St Lawrence River and in the Hudson River, NY. The concentrations of contaminants present were generally similar to those found in other fish species and those concentrations have been associated with impacts to growth, reproduction, and immune health.

With regard to impacts, eel mortalities have occurred in lab tests and in the wild, for other eel species. Amy gave some specific examples from the St. Lawrence River estuary, and from Europe.

The major concern is that contaminants will bioaccumulate in the lipid-rich tissues. They can mobilize during migration, when the fat reserves are used, and cause impacts to the individual animal and reproduction. Amy found one preliminary study in Europe which dosed eels with low and high doses of PCB, induced migration of 900 km, and found decreased egg quality and that embryonic development ceased.

What we currently know is that the preliminary European study indicated these PCB impacts, but there is no direct evidence that contaminants have contributed to the recent observed declines.

Amy noted that what we don't know is much broader. We don't know the population-wide contaminant load. There has been a decline in the contaminant (PCB) load in eels from the Hudson River near Catskill, NY.

Amy advised that is the current state of contaminants knowledge regarding American eels. Amy noted that the draft document she gave to Heather contains a table with all the contaminants. Heather indicated that she had provided that, as well as the papers Amy had found, on the CD provided to participants.

Marci asked if there were questions for Amy.

Paul Angermeier noted he appreciated the fact that some nasty substances were declining, but for every one that does that, there are probably five new ones. He asked Amy if she could address what the general trend is for fishes, in freshwater. Amy noted that she wasn't a fish person, but perhaps someone else in the room could answer that question.

Jim Snyder asked if mercury had shown any declines. Amy couldn't say due to the lack of continuous mercury data in the various geographic regions.

Tim Brush asked if the PCB declines in the Hudson have gone below the threshold of concern. Amy indicated it depends. She noted that the impacts vary with species, and noted that eggs and larvae are generally most sensitive, and no one has been able to test those for the eel.

Tim noted the level of PCB appeared to have decreased to about 2 ppm, and asked if that was below the threshold. Amy asked Catherine to comment. Catherine noted that it was hard to reach any conclusions about wild fish, because of their exposure to multiple substances, some of which are highly persistent. She noted that it is hard to isolate impacts from just one substance. She noted that some studies are showing that levels that produce effects are much lower than we thought.

Peter Hodson noted that we perhaps can use surrogate species in some cases. He noted some studies done with other fish. Another problem he noted was that with a decreasing concentration of some chemicals, and attempting to assess impacts to mature adults who might have had a different exposure during their life, is difficult.

Marci asked Peter to introduce himself. He did so. He noted he did some work on American eels in the 1990s and hasn't done much since.

Peter noted that there are a lot of studies on the effects of chemicals on juveniles and adults, but the thing that really counts is the impact on reproduction. He noted that very few people in the world are capable of creating the early life stages of eels.

Karin introduced into the record a final report, by Dave Secor, Joel Baker et al., titled Ecology and Contamination of the Hudson River and American eel. Karin noted that it contains a lot of the details that we want. It has information on demographics, PCB relationships in the Hudson, and linkages to the sediments. Karin noted that Dawn found this, but she didn't know how.

Len noted that the Sloan study also showed that in a number of sites, while PCB values were low, around 2, near the federal limits, there was a lot of variability depending on the individual eels and where they were sampled. A lot of them were a lot higher in concentration.

Catherine noted that it is important to add together all the congeners that are present in a given estuary, to get a sense of the total impact. She noted these could vary from estuary to estuary. Also, she noted that there are some studies for salmon, showing that very short exposure in the early life stages can have significant impacts. The salmon appeared fine, but when they went to sea, they had problems making the transition. Also, Atrazine, and Diazonon have been shown to have major impacts, in some cases for salmon, but for catadromous species such as the eel, we simply don't know. She will provide some publications to us. For eels, in the St. Lawrence River, there are lots of agricultural compounds, and also a huge sewage treatment plant. The Chinese came to visit the sewage plant. It gathers waste from a huge area, and barely treats it. It is already having impacts 50 km upstream and downstream of the discharge. Catherine noted that we have no idea what impact this might have on migrating eels. She suggested that we can't ignore the potential for these kinds of effect.

8:30 – 9:45 Contaminants Session (Lead – Heather)

8:25 a.m.: Marci moved to the contaminants questions, which she noted were only one page and suggested that Heather must have been running out of steam.

The overarching question was: Is the long-term persistence of the American eel population in whole or in part threatened by contaminants.

Wilson noted his perception was that some of the persistent compounds that we have banned in the US, are still being used in Central and South America, so the impacts might be greater there.

Peter noted his belief that given the impacts to lake trout, and the persistence of the chemicals in the environment, it would be difficult to dismiss eels from the list of fishes impacted.

Heather indicated if she understood Catherine correctly, the stage on which we should be focusing was the leptocephalus stage. Catherine indicated they were likely most sensitive, but noted that the yellow and silver stages, when maturation was occurring, were also very important.

Karin asked if the actual silvering phase mobilized anything. Catherine indicated she didn't see anything on that in particular. She noted there is a new study for Arctic char. She stated that effects have now been shown in adults of that species, on the endocrine system, from PCBs, as well as immunosuppression. With that new information, we shouldn't just focus on the early life stages. The work could more easily be repeated for eels, using the silver stage. With different species, the effects vary, due to their physiology.

Marci asked that we move to number one and its five subquestions: What are the current hypotheses about the impact of contaminants on the following: reproductive success; abnormalities in leptocephali; migration; availability of prey items; and synergistic effects with diseases.

Peter suggested that we would expect to see embryonic and larval toxicity, but it is all speculative for eel.

Marci asked Catherine to send the papers to which she had referred, to Heather.

Catherine indicated she would do so. She noted that another important factor related to migration was the energetic expenditure required for migration. If the energetic requirements are increased by parasites, or environmental stress such as increased temperature, the energy budget could change, and cause the mobilization of chemicals in the fat reserves to exceed some minimal threshold below which no impact might occur. Catherine noted that if you introduce a virus, then induce some immunosuppressive impact, that can be really bad.

Heather asked Amy about the European paper (van den Thillart et al. 2005), and energetics. She asked Amy if the eels with the contaminant loads showed any differences. Amy indicated that during swim trials, the eels with contaminant loads and control eels exhibited a difference in oxygen consumption that continued to increase as the distance increased. Heather noted the work wasn't done with the eels under pressure as they would have been in the sea, although the papers were done under pressure (joke).

Heather noted that eels seem so different from other species, what sort of caveats should we emplace? For example, many of the studies employed salmon. How much can we apply this to eels. Catherine stated that we should look at the eel. Peter noted that he had no idea where eel would fall in the range of observed impacts. Heather clarified that getting some lake trout information might be useful. Peter confirmed it would.

Ken noted that eels are great accumulators, because they live for such a long time and don't reproduce until late in life. Kent noted that someone he knows had a living eel that had the highest-ever recorded PCB level. He noted there were some good studies on PCBs and eels, back in the 1970s.

Karin asked if there were any studies on eels and PAHs. Catherine indicated they had looked in the St. Lawrence, and reported in 1997 that they found evidence that some of the silver eels had been exposed to PAH, due to the fact that they exhibited characteristic lesions. They didn't see advanced lesions like the ones found in bullheads. She indicated that they need to look at the larger eels in Lake Ontario, to see if they can find similar cancerous lesions. She indicated that you might be able to detect eels that are not maturing, due to such impacts.

John Casselman asked if Catherine could do examinations of frozen material. She indicated that fresh material was required.

Joe Hightower asked if different ages of eels could be sampled, such as yellows, and silvers on their way out, and assess the levels of contaminants, and presume that silvers with lower levels might be the ones that survived, and the others didn't make it. Catherine noted that eels where they grow are usually quite sedentary, so you have to go to where they live and look at the older eels.

Peter noted the cancer is primarily evident in older animals, so the issue is whether you are incurring non-fishing mortality beyond the level of natural mortality. John Casselman noted he thought they might be able to do that with the eel ladder data.

Peter noted the real question is what proportion of the population is exposed to the extent that it would induce cancer?

Catherine noted that they could differentiate between eels from downstream areas, which had fewer lesions, and those from the Great Lakes which had more.

John Casselman asked if you would expect to see dead eels from this source of mortality? Catherine indicated no, you would just see one at a time dying. She indicated that there is evidence of exposure to PAH, and consequent immunosuppression, so we should investigate this issue. She suggested that the fisheries community should investigate the differences between older eels and younger ones.

Dave Perkins asked if we would be able to see any effects realized on offspring subsequent to the leptocephalus stage. Peter indicated the effects in salmon usually occur before swim-up. The exposure has to happen during the embryonic stage. If they begin to feed, they may overcome any ill effects.

Dave indicated that given our inability to study a lot of these impacts, it appears very important to keep doing the glass eel indices. He noted the stock assessment peer review had suggested that the indices were really important. Joe noted there are only two sites, on the whole east coast. With decent sample sizes, you should pick up any impact.

John Casselman noted the timing and size changed in the NJ survey, and that might be a subtle indication of some changes.

Heather asked if the southern site was north of the Charleston Bump. Wilson advised it was. Heather asked if that meant that the larvae at both sites were delivered by the Gulf Stream. Wilson indicated that was his presumption. He noted the Beaufort site was inside of Beaufort Inlet, and a good ways north of the Charleston Bump.

Marci asked about availability of prey items. Peter addressed the situation in the Great Lakes. He noted that there have been some shifts there, but they weren't due to contaminants.

Catherine noted that if you lose anti-oxidant vitamins, that can cause problems. If a contaminant is broken down, or metabolized, in the body; sometimes the breakdown product is more toxic (an oxidized form) than the original contaminant. These oxidized contaminants can lead to DNA damage. Anti-oxidant vitamins prevent the formation of harmful oxidized contaminants.

Peter noted that the Great Lakes salmonid reproductive story was originally thought to be just a contaminant story, but now, the theory is that the adults become deficient in thiamin, and it isn't an issue for them, but it adversely affects the eggs. So, what used to be thought a simple story is now becoming much more complex.

Rob McGregor noted that all this stuff is cumulative, the contaminants as well as the introduced species.

John Casselman concurred that there have been major food web changes, especially given the changes in alewife abundance over time, which really complicates any sort of analysis.

Marci asked Catherine if she cared to repeat her comments about the synergistic effects with diseases. Catherine noted the problem arises when you mobilize contaminants during migration to use your energy reserves. The immune system is compromised, so if parasites and/or disease enter the picture, the eel's

health can be significantly impaired. The other point we have discussed is that eels exposed to PCB and/or PAH, in the Great Lakes, could die prematurely and not contribute to reproduction at all.

Heather noted we had briefly discussed viral and bacterial infections present in eels, which might not manifest themselves until the circumstances were appropriate. Catherine indicated that she would provide Heather with literature on those topics, including the aquaculture literature, addressing eels maintained in captivity. Catherine thought there isn't much on diseases of eels in the wild. Heather indicated they hadn't yet spent much time looking for literature on diseases in eels. That is why she asked yesterday about spending time on that. She presumed that anywhere in the range, the eels might be carrying a contaminant load, which could increase their susceptibility to disease. Catherine noted that most fish carry a load of microbes, which can manifest themselves under the right conditions. Catherine noted there are pathogens concentrated in some of the farming operations, so there is potential for exposure to new threats, more so if the immune system is compromised. There may not be any problem in a normal eel, but with immunosuppressed ones, it would be problematic. Catherine gave us an example that had occurred in seals, where they were immunosuppressed due to PCB contamination.

Karin indicated she found an article in German, on the Internet, that discusses the synergism between contaminants and disease. She noted that she was fascinated by John and Joe's ideas yesterday about trying to map out reproductive potential, and suggested that we could perhaps map out areas of higher stress.

John Casselman noted they rarely see dead eels in the lakes, but there was a substantial die-off in the past. As a result of modeling, they now believe it was 2-3 million eels, likely due to some epizootic. He indicated that VHS is now documented in Lake Ontario, and it could affect eels, but of course there are very few in the lake now. That should be taken into account in trying to explain all of this. John noted that he has a large archive of frozen eels, at 20 degrees below, and offered that up as a resource for anyone who might want to use them.

Marie Maltese noted there is a lot of export of eels to Asia for captive breeding, and wondered if there is a body of literature there on disease, because of the farming there.

Peter noted that we need some kind of cumulative risk assessment, which takes into account all of these factors.

Wilson noted, to Karin's suggestion about mapping areas for stress, that Fred Holland with SCDNR had done some work along those lines, with red drum, comparing stress hormones in the blood of fish from urbanized, versus more rural, estuaries, and that might serve as a model for what Karin was suggesting. He noted that this was more a recommendation for future research, than useful in the present analysis.

Catherine noted that ill animals frequently don't move around as much, so it makes it harder to assess the state of things... [there was more but I missed it].

Marci referred us to question 2: For each region (north to south) how many years do males and females spend in their freshwater and estuarine habitats? Heather noted that this question related to the potential for contaminant uptake.

Heather asked participants to address that question. John Casselman stated that for the Canadian areas, in terms of females, the average generation time was 20 years. Some studies show less, but John believes that was due to the aging method used. For males, he had no idea.

Heather asked Ken if he knew what it was for Maine. Ken indicated he would have to look that up. Ken noted that he had a very nice dataset, because Brian Jessop has found all the errors in his published papers, and helped him prepare a table of ages. Ken noted that there is a nice correlation for males, of age with latitude. He had the table with him. He will provide a copy to Mike, to put on his paper.

John Casselman noted that he would advise Heather to put 15 years up on the easel for males, and he would check on that. Ken indicated that he would pull his table and help fill out the easel table.

Marci moved us to question 3: What are the home ranges for males and females (and does home range vary for northern and southern eels)? She asked Amy to explain why this was important. Amy explained that eels that moved around a lot more, were more likely to pick something up. Heather noted that migrating females that have ceased to eat might not pick anything up, but asked if they could pick it up through their skin. Catherine stated they could pick things up through their gills. Heather asked about females that might delay their migration upon hitting a barrier. Ken stated they don't feed.

John Casselman noted they ball up in the wintertime, and in the mud. He stated they used to move down the river in balls, and he has seen them in gill nets, in balls of 40-50 eels. He stated the balling in the mud was mainly a winter phenomenon. Bob Graham suggested that in southern estuaries, exposure to mud was more of a year-round condition, so eels taking up residence in a contaminated area would have a higher risk of contamination.

Marci asked the second part of the question first, which is do the home ranges vary. John Casselman noted the home range, based on tagging studies they have done, may vary as a function of density. He indicated that he might be able to calculate some estimates.

Paul noted that in his opinion, home range was more likely to be a function of density, and spatial issues, that weren't particularly related to any north-south gradient.

Marty asked if the population was better off, with a smaller number of eels exposed to higher contaminant loads, or with a larger number of eels, exposed to lower loads? [Check with Marty to make sure I captured his question correctly.] Peter indicated that the home range issue was definitely relevant.

Peter noted that rivers coming out of northern Quebec were much less contaminated ...he referenced a paper published in 1994. Rob asked if there was any indication that the decline in Lake Ontario was related to contaminants? Peter noted the decline in contaminant concentration was concurrent with the eel decline. He noted there are questions regarding the residence time of the eels. The temporal trends in contaminants need to be better related to the temporal trends in eels, so we can better understand the relationship.

Marci asked Heather if she had what she needed for contaminants? Heather indicated that her head started to spin any time we discussed contaminants. Catherine agreed it was complicated, due to the threshold level situation.

Ken indicated that he could put his age table up on the screen, if we wanted. Marci assisted him in doing so. Ken showed us the table that he and Brian Jessop had compiled, looking at eel age and growth. Ken noted that he was still updating the table, and hoped to include Len's work soon. He noted that with the addition of more points, the female regression is beginning to show a trend. Wilson noted that Ken's table would be a great addition to the chapter on American eel being written by Jennifer Lowery, for the ASMFC Diadromous Species Baseline Habitat Source Document.

John Casselman cautioned that some of the studies may not have used currently accepted methodologies. John noted that he could supply more current values for the St. Lawrence. Ken noted that not many of the studies have both age and length data. Peter asked if the relationship for females was in fact linear, or curvilinear, since he perceived a break in the curve. Ken thought the apparent break was due to his sample size being larger from some areas (Maine) with a fairly narrow latitudinal band.

Karin asked when eels could be sexed? Ken addressed the question [I missed the details of his response.]

Karin noted that maleness could develop rather quickly. Ken agreed that was the case.

Tim Brush asked what latitude 32 was? Ken advised that was Georgia.

Marty asked what the advantage was of differences in growth with latitude. Ken noted for males, the size at maturity is rather fixed. For females, the situation is fuzzier, although the more data he adds, the situation appears to get clearer.

Ken noted that he couldn't get NSF to buy that argument. He noted that he has always wanted to work in the Finger Lakes, because eels from multiple sources wind up there, and he really wanted to examine the size issue. Ken agreed that it made sense that if you are further away from the spawning area, larger size would be advantageous, but he hasn't seen any data to support that hypothesis.

Heather asked if it was correct that females were more likely to be further inland. Ken noted while that was often the case, he didn't agree that it was true all the time.

The question was asked about endocrine-disrupting compounds and interruption of the migratory cycle. Ken noted that was his most recent NSF rejection. He was more interested in the habitat issues, and stress relationships.

Peter indicated their studies do show that pharmaceuticals do have impacts. Bob Graham noted there are recent studies from the Potomac documenting sex reversals in smallmouth bass, and the hypothesis is that it was due to pharmaceuticals. Heather asked Peter to repeat his comment. He stated the number of studies was very small, and in most cases they were done below sewage treatment plants.

Joe Hightower expressed skepticism about the usefulness of home range estimates. He suggested that Karin's paper, that showed all these variable life strategies, led him to believe that home range was not a particularly relevant factor.

Marci noted again that Karin's paper was available in the back of the room. Heather noted that the list of the participants from the first workshop was also available. She asked all FWS and NMFS staff to meet with her during the break.

Marci asked that we add five minutes to the break time.

9:38 a.m.: The group broke.

9:45 - 10:00 Break (hosted by GLFC)

Population Dynamics

Presentation: Karin Limburg – Diversity of habitat use in anguillacolid eels

Marci reconvened and noted that we were adjusting the agenda a bit, to provide Karin 45 minutes to review her research. She asked that questions other than those for clarification purposes be held to the end of the presentation.

Karin showed us a print-out of Paul's conceptual model, noting that eels use a wide variety of habitats. She indicated she would review the basics of otolith studies, and give us a Swedish example, then make some comparisons between three anguillid species (the paper she provided to us).

Karin reviewed the otolith and told us about their location, characteristics, and composition (aragonite on a protein matrix). She noted that she would focus on strontium (Sr) in this talk, but noted that other compounds could be assessed as well, such as trace metals. Sr is particularly useful in tracking diadromy. Usually, Sr is higher in concentration in seawater, than in fresh. Microanalytic techniques are used to measure the concentrations in otoliths. Karin noted that analysis and graphing of the resultant concentrations reveals a "jump" when a fish moves to the sea. She noted that she has done work in Sweden, where she worked on the development of better methodology.

Karin turned to the eel work. A study in which she was involved asked whether stocked eels were actually contributing to the spawning stock, or were just put and take? One hypothesis was that the stocked eels don't swim in, so they can't smell their way out. Another study stated that stocked eels didn't contribute at all to the spawning stock.

The study area was Sweden and the Baltic Sea. Silver eels were used. Karin showed us an example otolith from a silver eel. It was striking how diverse eel habitat use was found to be. One was 100 percent marine; another began marine and moved to brackish; another moved back and forth to brackish; another was complex; and finally, Karin defined "S" categories representing eels stocked at various locations and sizes.

Karin asked what do differential fat reserves imply for migrating to the Sargasso Sea, 6,000 km away from the Baltic? She noted that analysis showed that the natural catadromous eels have the energy to make the trip, but the stocked ones didn't. She noted the marine ones didn't appear to either, but that this was skewed by one very skinny marine eel.

Karin turned to the paper she had provided to us, which compares the European, American and Japanese eels. She noted at a recent conference, when comparing notes with other researchers, lots of similarities were discovered. Karin showed us graphs of patterns from eels from different locations. She noted the term they have adopted for use to describe the life history patterns of some eels, is "nomadic," which she believes is very applicable. The Sr values between 2 and 4 indicate brackish residence, while lower values indicate freshwater, and higher values marine residency. Karin showed us the patterns used by one European researcher. Karin noted the patterns displayed by the three species are not exactly the same, but are similar.

With regard to how long they remain in one habitat, they may move after five-six years. She noted with reference to Wilson's earlier comments that these patterns reflect current conditions, with regard to barriers and so forth.

Karin noted that if we had 20 such studies, we might be able to make more sense out of things, but the major point is, eels move around. There does appear to be some latitudinal gradient, with the southern eels spending more time in freshwater. Karin noted that there was a paper in Science that speculated that catadromy and diadromy evolved in response to resource use, and that might in fact be a factor. She suggested that the utility of this work is to assess how much time eels spend in various habitats.

Joe noted that it appeared to him that the fraction of time spent in various habitats was a reflection of where the eel sampling occurred, and therefore was subject to bias. He wasn't sure how you could design an unbiased study. Karin stated that there was quite a bit of variation. Joe agreed with that, but noted that he didn't believe you could take the percentages literally.

Karin noted that one author did sample a lake, and a brackish system. John concluded that it was a sampling thing again, then. Karin indicated that she was beginning more work on the Hudson, but she is limited to using Sr. She noted others are moving more to "fingerprinting" approaches, sampling other compounds as well.

Karin asked Len to address that question. Len indicated that they haven't addressed the issue yet.

John Casselman noted he found it very useful to see the Sr changing. John noted it seemed to him that it would be beneficial to measure the absolute abundance of eels in estuaries.

Heather asked Karin if she had a chance to review the Cairns paper (Lamson et al, in review), in the population dynamics section on the CD, yet? Karin had not.

Peter asked if anyone had used this technique yet to assess the exposure to contamination. Karin indicated that Dave Secor had done such work on striped bass.

John Casselman noted he had failed to provide a paper he did, in 1982, on Sr:Ca ratios in the St. Lawrence, but would do so.

Karin noted that the instrumentation for doing some of the more sophisticated analyses was not very readily available.

Len noted that Cairns 1984 paper, might be worthwhile exploring with regard to the relation between eel movements and barriers. Also, finding out how small systems change could be important. He noted that Morrison and Secor found that eels that move had slower growth rates than those that spend more time in brackish water. Eels upstream might be growing faster, but we need to know if barriers impact their movement back to brackish waters prior to emigration.

Dave Perkins asked Karin for more information on the sampling details. She indicated she couldn't provide those for the other studies. In her study, they collected silvers on their way to the Sargasso, and essentially asked, "What did you do?"

John Casselman noted he found it remarkable that the growth rate was increased in the estuary and ocean. He noted the quality of the streams, from a productivity perspective, is poor. Karin agreed that the mesohaline portion of the Hudson estuary is far more productive than the tributaries.

10:30 a.m.: Heather noted she was going to change gears. She noted that this Status Review was being conducted differently than others in the past. Rather than waiting until the end to bring in experts, we have brought them in at the beginning to help us do the information collection and analysis. At some point, we have to draw the line on further information collection. Heater indicated she was asking us to help make sense of the information gathered. She indicated that she was asking Marty to share with us how we deal with information.

Marty noted he wanted to give us an idea of what we would see in the Status Review and Findings documents. He noted we have traveled a lot of paths, and on some of them, we have thrown up our hands. On other paths, we have raised a lot of concerns. For those about which we can't say much definitively, we won't deal with them much in the Status Review document. All of them will be considered to some extent. This is not to say, they aren't important. Our purpose is different. We have to provide information to document whether listing is warranted, or not warranted. We don't want to suggest that our ultimate document is going to be a transcript, or a dusting off of Wilson's notes.

With regard to the things which we don't give much treatment to, versus those we do, it comes down to how much uncertainty there is associated with it. For us to give a lot of treatment to something, there has to be a lot of significance associated with it, and less uncertainty. It will be between the two extremes, where we spend most of our time in the document, with those things (with a reasonable amount) of significance, and moderate uncertainty.

Rob noted he had asked the question at the last workshop, as to whether we have to look at the entire range of the species, or portions.

Marty noted we are presently looking at the entire range. The only way we could do otherwise is to designate DPS, and he wasn't sure a discussion of that would be productive. Dave Perkins noted that there are two categories, threatened and endangered, and both have risks associated with them. Marty noted "threatened" had a lower associated probability of extinction associated with it.

Rob asked Marty to address the DPS issue. Marty noted we have policy for that, from 1996. He noted that we have to demonstrate that a population is discrete, and significant. He noted that for the eel, we couldn't get past the discreteness criterion. The population has to be breeding discretely, but not necessarily 100 percent. With eel, that criterion isn't met, since they all breed at the same place. If we got new information showing some kind of separation during breeding, we could reconsider that issue.

Heather noted that "threatened" has to be throughout all, or a significant portion of, a species range. **See definitions.** Marty noted that this has been the subject of recent lawsuits. He indicated that FWS has traditionally spent little time addressing this, given that if a species was at risk in a significant portion of its range, the criterion was met. He noted that we had usually lost in court, but recently we won a case. We will have to see how that plays out. Marty noted the most recent ruling seems to be contradictory to some in the past. It is kind of a messy issue now, but we seem to be getting some traction with the courts regarding the way we have interpreted things.

Peter asked if it was a problem that a species range might lay outside the US. Marty noted that wasn't a consideration. He noted that an international boundary could be used to establish discreteness, such as the Canadian border for grizzly bears, for example, but there had to be management distinctions.

John Casselman asked if resource use was taken into consideration. He noted he had no fear of extinction, but noted that our ability to use them as a resource could be completely lost. Marty noted that we looked only at the likelihood of extinction, with regard to listing.

John Casselman noted that the eel could decline to the point where the fishermen could go extinct. Heather noted that there was potential to discuss the loss of the fishery in a Status Review document (as part of the purpose of the status review in this instance is to inform ASMFC), but not in the Finding which will be published in the Federal Register.

Marty noted that economic factors could not be considered (during a listing determination), and that the ESA doesn't require us to establish a level that would allow economic recovery.

Rob McGregor noted that to him, the question about a significant portion of the range is valid. Heather noted we would spend some time on that issue today.

Karin noted even if they retract to the marine range, but we really don't know what the value or use is of the continental populations, and we need to pay attention to that.

Someone asked if the precautionary approach came into play. Marty advised that if we are truly on the fence, we are required to err on the side of the species, and the courts have acknowledged that. It comes down to how well we describe all the uncertainties, and all the risks, so that we clearly know when we are on the fence. Just because there is uncertainty, and because any species can go extinct if there are enough threats, we can't carry that too far.

Paul asked if it would be worthwhile to describe how the input provided by this group will influence what subsequently happens. Marty noted this is the most important piece, because the law requires that we make our decisions on the best available science, and that we gather it from the experts. He noted that we typically don't have these sorts of meetings, but we decided to do so because this species is so complicated, and we needed help. The role of participants is to help us gather the information and make sense of it. Heather will take the first step toward compiling the information.

Heather asked Paul if that answered his question. She noted that she couldn't say exactly how the process would go, from this point onward, because she will have to sit down with our Assistant Regional Directors. She noted that we are overdue with our Finding. She noted that we could provide the information, and uncertainty, to our managers, if we come out of here with our heads still spinning. A recommendation is made to the Director, and he will decide if it is carried forward. She noted the old way was for the biologist to make a recommendation and send it up, but today, the recommendation is sent to the managers, and the decisions are made by managers, after debate or risk assessment. Marty noted that today, in a lot of cases, either the species is in crisis mode, or the petition is frivolous. With this species, we are still scratching our heads, and it may be late in the process before there is consensus in FWS/NMFS with regard to what action should be taken.

Peter noted with all the uncertainty, he felt that research should proceed, especially if the decision is not to list. He wondered what the recycle time would be. Heather indicated that it depends. Marty noted that someone could send in a new petition the next day but it is new information that would determine when we revisit. Peter asked what sorts of resources are available to initiate needed research. Kim Damon-Randall noted the approach they take in NMFS, where different pots of funding are available to do work on species of concern. She noted if the decision is made not to list eel, NMFS may designate it as a species of concern, and try to get some work done. Heather noted that FWS doesn't have that category anymore. Marty noted that a decision not to list the species doesn't mean that everything is fine. It should be clear that the threats remain. He noted that it should inform the research priorities when we complete our documentation.

Paul asked Kim who decides in NMFS about special concern status. Kim indicated it was a rather new process in NMFS, and criteria for designation haven't been established yet. She felt that it was probable that this species would wind up on the list, due to all the threats and uncertainties. She noted there is a solicitation out now, for \$500,000, to do work along these lines. Marty noted that FWS doesn't have such a list, but many of our field offices maintain informal lists. He noted the states have just completed their Wildlife Action Plans, and that process involved getting better coordination among the states in dealing with some of the wider-ranging species.

John Casselman asked if there was any mechanism here to recommend the integration of US and Canadian research. Heather indicated the Status Review could promote that. Marty noted there was an

agreement, established between the US and Canada, to coordinate on imperiled species, or species at risk. He wasn't sure where that process was, but when he worked in headquarters, he met with them. So, there was that coordination, and infrequent meetings. John asked if the species had to be listed to trigger the agreement. Marty advised it didn't.

10:00 – 11:45 Population Dynamics Session (Lead – Heather and Dave)

Heather indicated that what she would like to do in the 45 minutes prior to lunch is to talk about the possible significance to a rangewide listing, of the upper St. Lawrence and Lake Ontario - the information that John discussed yesterday.

Heather indicated that she was looking to Dave and others to drive the population dynamics discussion. Dave noted the broad question was captured in the second **Question 3 c)**: What is the importance of the Upper St. Lawrence River/Lake Ontario (USLRLO) to the overall population dynamics of the species? Would the loss of adults from USLRLO lead to extinction of the species? John Casselman noted that it comes down to how valid you believe the harvest data are. If the data along the US east coast aren't dependable, it throws doubt on the analysis. John noted they believe they know some things about the St. Lawrence system. It is not just because it is their data. They know the harvest is reported. It is a bit different in the Gulf of St. Lawrence. If you look at what has transpired over time there, the declines are not as dramatic, maybe 30 percent, versus up to 90 percent in the lakes. The declines in the US appeared to be about the same as in the Maritimes.

Dave asked folks around the table to comment on the US harvest data. He noted the peer reviewers had suggested the harvest data shouldn't be viewed as a surrogate for population abundance, but it could be. Dave asked Joe to comment. Joe noted that their comments reflected uncertainty. He agreed with John that harvest could be reflected of population abundance, but noted that he was confident that actual landings in the US were higher than reported. He wasn't sure how the market affected things, either.

John Casselman noted they had recently looked at some Florida data, which were very interesting. The Atlantic and Gulf landings seemed to have some very interesting information in them. He noted there are little data from the southern part of the range, but when you do examine what is there, there are some declines which appear to mirror what they have observed in Ontario. He noted that FAO data for Mexico show a similar decline. He is trying to determine from FAO how valid the data are. He noted that the central states do not show similar declines.

Dave asked John to comment again on the use of discharge as a surrogate for habitat. John noted that when he used the landings data, from the 50s and 60s, the landings data suggested the same percentage as the discharge data do. He acknowledged that perhaps he was grasping at straws.

John Dettmers noted that the data John Casselman presented convinces him that the reproductive output from that area is significant. The declines from Moses Saunders also are real. All the measure track together. It convinces him that a) there is a problem, and b) there is a problem down the road. He indicated that eels that used to run up the Mississippi may represent another significant loss from the population, and asked if we wanted to do it again in the St. Lawrence.

Paul noted that he concurred with the assessment that there is a problem in the St. Lawrence, and that it contributes significantly to the population. However, he didn't believe that we have a case for biological extinction. The species is widely abundant, uses lots of different habitats. The only way he would see otherwise is if there is some social interaction in the Sargasso of which we are unaware.

Rob asked about extinction locally. Paul acknowledged that could certainly happen.

Peter noted, given the role that eels play in the system, there are probably a lot of impacts of which we aren't aware. He stated that if listing it would help restore it, he would support that action. Paul noted that he didn't disagree. Peter asked, how do you use the ESA to further that? Paul asked, is the ESA the appropriate tool? He didn't believe it was.

John Casselman noted that in the 1600s, eels were massively abundant in the shallows. For all intents and purposes, we have lost recruitment of the species to those habitats.

Heather agreed, and noted the example of the buffalo, which has a hugely constricted range, but isn't in danger of extinction. Heather noted that as scientists, we would all like to see the species back. But, she noted the sideboards of the ESA are rather narrow. Heather noted the wolf did get listed, but is now being delisted, but only in a tiny portion of its historic range.

Karin noted she would like to hear some discussion of the relationship between the ESA and international measures such as CITES.

Marie noted FWS was considering proposing listing of the American eel under Appendix Three, which would give the least amount of protection, requiring only a certificate of origin. That would allow FWS to track what is being exported, which we at present don't have a good handle on. Eels can be shipped out as "seafood," or "fish." If we see large numbers being exported, then we can propose it for listing under Appendix Two, which gives a higher measure of protection. It requires documentation of legal acquisition, plus the applicant has to indicate that any animals were taken in an area not detrimental to the survival of the species. Marie analyzes non-detriment documents at present. She indicated that after the first workshop, she returned to her supervisor and suggested, given that a new conference of the parties is coming up in 2007, that we recommend Appendix 2, rather than Appendix 3, for all anquillids. She noted that proposal met with mixed reviews. It is unlikely that we would be able to get a listing for all anquillids through. Recently, they have been discussing an Appendix 2 listing for American eel only. She has been discussing with Rob and Alastair, whether Canada would be willing to co-sponsor a proposal, that would use a lot of this information. She indicated that she would likely be contacting a lot of us for support. Any proposal would have to pass by a two-thirds majority of the 162 parties.

Karin asked how that related to ESA. Marie indicated they weren't related, they were two separate entities. Karin asked if a CITES listing would color the ESA deliberations.

Heather noted that if unregulated harvest was viewed as a threat, CITES or ASMFC regulations could come into play.

Marie noted that any CITES package would come into play in a longer time frame than the ESA Status Review. Marie noted that a listing would result in compilation of trade data, which would be put into a database that we could access.

Rob indicated he was confused about one thing, which was the significant portion of the range issue. Heather stated that it wouldn't be listed just in that portion of the range, but rather throughout. He asked if such a listing had been done.

Marci noted that the eastern cougar was listed nationally, despite the fact that there were only two populations left, in FL and in the NW.

Heather explained what would have to happen for us to list the species, based on the declines in the Great Lakes. She stated that if we could show that all the females came from the Great Lakes, then it might be listable. Marty noted that you still have to show that the species is at risk of extinction throughout the range.

Rob noted that we are spanning 17 different jurisdictions. He felt that such listing was the only way to generate any coordinated action.

Dave Perkins noted that ICES served as one possible venue for generating coordinated action. He noted that for Atlantic salmon, that body has worked. Rob noted they had been approached, but weren't interested.

Peter suggested that if more North Americans attended and participated in ICES, they might have more interest.

John Casselman noted that ICES invited North Americans to their eel working group, thirty years ago.

Marie noted that an ESA listing, would trump CITES, and that is one place where they do dovetail.

Dave Perkins noted that Paul had kind of started us in one direction, by talking about the eel's wide plasticity, and so forth. He asked if there was any disagreement with Paul's opinion. Dave asked John Casselman if he had heard correctly that he wasn't too worried about biological extinction. John confirmed that was the case.

Ken noted that the New Zealand fishery was a good example of what could happen, if there is something happening to reduce the density of females, and a fishery is prosecuted on the females. Heather asked if, as density decreases, and you actually get more females, but you have a threat operating more on females, will eventually the population crash?

Peter indicated that if he interpreted what Ken was saying, you wouldn't see the alteration. In fact, the population was really declining.

Paul wanted to offer a caveat to what he said before. He didn't want this assessment to state there are no problems with eels. His statements were really based on the uncertainties that we have discussed in the past couple of days. He noted there are data that can be analyzed to give us a better picture. Some good simulation studies can give us more insight. We know that the European eel is down to one percent of its historic levels. Paul wasn't sure, but he guessed that the American eel might be down to ten percent. He noted that we don't yet know what the threshold level is that would lead to extinction.

John Casselman indicated, given his statement about the likelihood of extinction, and the fact that eels have a marine stage that will give them some protection, we seem to be concerned about females. He noted that we don't know as much about males. Some work they are doing now suggests that the quality of the males and their sperm is in fact very important. We didn't think before that it was.

Tim Brush noted that with regard to Ken's comments about the New Zealand eels, it is good they have some compensatory mechanisms.

Karin noted that Sweden red-listed the eel in the spring of 2005, and it was possible to do so, because there is no longer a requirement by IUCN that the species has to reproduce in the country of origin. She read other criteria, which recruitment has to decline by a certain percentage.

Joe indicated that he agreed with Paul that the species didn't merit listing, because of the broad plasticity. The one place where that isn't true is with their reproduction. If something happened in the ocean to change the current patterns, we could lose the species. Also, if the parasite adversely affects the females leading to significantly reduced output - that is an issue. He did agree that more needs to be done in the fishery management realm to control harvest.

Heather re-stated for the record Joe's comments to be sure she understood them. She noted that we don't know now for sure that the parasites are a real issue, but it could result in a Type II error.

Peter noted that we appear to be facing extirpation in part of the species' range, and he could accept that, if there were no further declines. He wondered if the threshold shouldn't be the total number of individuals, but rather the rate at which it is declining.

Joe noted he thought there is something in the ESA specifying that extinction be imminent. Marty indicated there is nothing in the law, but we apply what conservation biologists use, which is to look at the risk of extinction during the next 100 years. He noted that he had recently reviewed an analysis that used a 200-year time period. He noted that we could list something that was becoming so abundant that it was outgrowing its habitat, but it was at risk, because the analysis is really based on threats.

11:45 a.m.: Marci recommended that we break for lunch. Heather asked that FWS folks and NMFS eat lunch together.

11:45-1:00 *Lunch on your own*

1:00-2:30 Population Dynamics continued

Please Note: The following session had some specific objectives which should be reviewed to provide context for the expert's answers. Our objectives:

1. Verify that the participants understand the species level analysis and whether there were things to take into account when talking about species level extinction versus regional or local extirpation.
2. Ferret out significant issues that may be underlying the species status that we failed to adequately address in the workshops, and
3. Identify the most significant uncertainties and the possible ramifications of those uncertainties to the FWS/NMFS decision.

To meet these objectives we kept the question simple and focused on risk. We did not ask these experts to identify if the species was threatened or endangered under the ESA, as that is specifically an agency decision. Each invited expert was asked for their individual response, no attempt was made to reach a consensus. We found this exercise useful in that no new vulnerabilities or resiliencies of the species were presented, and no new uncertainties or threats were revealed.

Marci asked the **Overarching Question** to each participant individually, which is, do you believe the species could go extinct?

John Casselman stated he didn't believe it could, but he qualified that by saying that given that we know so little about the reproductive end of the life cycle, learning more about that could cause him to change his opinion.

Karin felt it could go extinct on the North American continent, given all the threats it faces.

Guy felt that it could go extinct regionally.

Catherine felt that a spiraling effect could be created at some point, especially given the uncertainties, to plunge the species into further decline. She felt there was a good likelihood of local extinction.

John Casselman noted that he was responding to the question about the extinction of the species as a whole, but he believed that local extinctions were already occurring.

Bob Graham didn't believe that species extinction was likely, because of the wide range and broad habitat use of the species. Marci asked Bob if he believed that local or regional extinctions could occur. Bob stated that after attending this workshop, he did.

Ken Oliveira noted he didn't believe it could go extinct, but he qualified that by saying that there could be some threshold, some minimum number of spawners, in the Sargasso Sea, that we might fall below and not know it, that could change things dramatically.

Peter noted that he didn't think that local extinctions were a problem, in that they are already a fact. He noted that it was predicted in 1984 that the Lake Ontario stock would decline, and predicted for SLR in the 90s, which it did – a very rapid decline.

Len stated that he felt, due to the unknowns, there was a good possibility of extinction.

Rob felt that regional extinction at the range extremities was likely, and viewed that as a grave warning. We might have caught it in time, if we take action now. Rob believes that we are on track for bigger problems.

Paul Angermeier indicated he concurred with much of what has already been said. He didn't believe that rangewide extinction was imminent, but it was approaching some unknown level at which it will be endangered, but we don't know when that might be.

Alastair asked Marci to repeat the question. She did so; asking if in his opinion, the species was likely to go extinct. He indicated he was hoping that wasn't what she was asking. He concurred that local extinctions were likely. Throughout the range, things are less certain. To exercise caution, we need to take action across the range. Can we conclude that the species will go extinct in the next ten years, or in

x number of eel life cycles? He didn't believe we could, but looking at what had happened in Lake Ontario, the only way to address the decline was to take broader scale action.

John Dettmers asked, how many different ways can we say similar things? Heather asked him to do it for her. He stated the likelihood that the species would go extinct throughout its range was quite low. However, the likelihood of range contraction and local extinction was high. He noted that we don't know much about the ocean phase of the life cycle, and uncertainties associated with them are great. He felt we couldn't rule out the possibility of extinction.

Joe felt the possibility of extinction was low. But, he noted that he had asked about the time frame, and that is important. He noted that we have no basis for predicting the likelihood of the nematode knocking out reproduction. For him personally, if these trends continue for five to ten years, especially if other surveys such as the Delaware one start to show the same trend, then listing would be warranted. Joe noted there is a nice correlation of the Moses Saunders index with the NAO, and if that changed so that you should be getting recruitment, but it didn't happen, that would be a real red flag for him.

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Uncertainties – although not listed in order of importance, these uncertainties were thought to be the most critical in understanding the status of the species range-wide.

- Spawning requirements, including behavior and whether there are mating thresholds, and the impact of *A. crassus* and contaminants on spawning success.
- Rate of decline
- Cumulative impacts
- Oceanic life stages and the influence of oceanic conditions on transport and spawning.
- Marine subset, including the percent of population.
- Escapement levels

Heather noted that she wanted to make sure that she went to the right data. With regard to the rate of decline, the rate was certainly significant at Moses Saunders, but the two US glass eel time series aren't showing the same decline. She asked how Joe would characterize the observed rate of decline in yellow eels. Joe stated that was for the three rivers sampled by VIMS, and while it was less severe than the Moses Saunders database, they are going down at about the same rate. Joe referenced a table in the back, where one of the panel people ran a regression on them, and the rate is about the same. There were also a bunch of surveys that didn't show a significant trend.

Heather clarified that on a rangewide basis, the signal is mixed, but for those systems showing a trend, the decline rates are similar.

John Casselman noted the assessment had to be tempered by the gear being used. If the observational variability is low, you can place more faith in them. John noted that the observational variability at Moses Saunders was about 16 percent, whereas with trawls, it is about 75 percent. Also, logged data will give you a different slope. That is why the ladder data show a steeper slope. The sample size was much smaller in the trawls, and now they don't catch any eels at all in the trawls. John Casselman asked Heather if that was what she wanted.

Heather indicated she was trying to get at the rate of decline issue. She acknowledged that the rate of decline at the ladder was significant, but wondered how that compared with the other declines.

John Casselman noted that the trends may vary, with different size classes. He noted the VIMS data show a steeper decline, in certain size classes. John Casselman stated that the length of the dataset also has to be considered. People are looking at different lengths of time series and reaching different conclusions. The farther back you are; the better off you are. Long-term datasets will give you much more insight.

Dave Perkins agreed that long-term data are very useful. He noted the datasets for the NAO, and noted that the decline is somewhat predictable, based on the cycle of the oscillation. He asked John if the decline is what you would expect, based on the NAO cycle. John noted Joe's comments about trying to analyze the departure from normalcy. He noted that someone else, Decker, suggested recently that there is a decoupling between the NAO, and recruitment of the European eel. John suggested that we should look more closely at the European eel, because it is responding in similar ways. John indicated that if we are successful in getting some restoration, we will see it earlier in the glass eel monitoring along the southern Atlantic coast.

Heather wondered what level of caution she should put on using the European eel as a surrogate, on coupling the two species too strongly.

John Casselman noted that the similarities are rather remarkable. He noted that because of currents, the European species may take longer to get there, but there is remarkable synchrony.

1:34 p.m.: Heather asked with regard to the glass eel data, are there any time series for Maine, for example? There aren't any. Heather asked if there were any glass eel sampling in Canada. John advised there was one fishery-independent program, that was started, then stopped. There is urgent discussion that it should be picked up immediately. It is in Nova Scotia.

Karin noted that NYDEC is running one, in conjunction with ASMFC, and thought that was going on coastwide. Heather noted it is, but it is rather short-term.

Marci noted that Ken was out of the room when she asked the question about any glass eel time series in Maine, so Heather should ask the question again. Heather did so. Ken indicated that the state was doing some work, Gail Wipelhauser. Joe thought that was the ASMFC sampling.

Wilson noted that some of the Long-Term Estuary studies funded by NSF might be a useful source of data. He knew there was one at Belle Baruch, at North Inlet, in SC. Karin knew there was another one at Plum Island Sound, in Mass. Heather asked Wilson and Karin to see if they could get contacts for those programs and determine if they might have any long-term glass eel data.

Marci turned back to the easel notes that Heather had begun earlier.

John Casselman suggested it might be productive to ask about other time series that have documented elvers, or young yellow eels.

Heather suggested that we perhaps didn't need the glass eel stage for this discussion.

So, the chart was changed to assess the most important factors affecting the transition from elvers to yellow eels. Paul thought fishing was one. John Casselman stated barriers. Kim asked Paul if fishing was a present threat, given that the glass eel fisheries have largely been eliminated, except for Maine. Lydia commented that the incentive for the fishery varies with market demand. Joe noted that the people involved in the fishery stated that the fishery was really variable; also, he noted that ASMFC doesn't allow any relaxation of the regulations presently in effect, so there won't likely be any substantial elver fishery any time soon. Ken noted that the price was high last season, so the effort may increase this year. Heather concluded there is still a price-driven fishery in Maine, albeit small. Heather asked John Casselman about the Canadian elver fisheries. John stated the argument is that those fisheries are conducted on surplus production, since those elvers are going into acidic tributaries and would not likely survive. Peter noted that habitat degradation was a factor. Rob asked about the bait fishery. Lydia noted the US bait fishery uses small yellow eels, which are captured fairly well in the commercial statistics. All the states have a six-inch size limit in effect. There may be a cash, side market. Heather clarified that the yellow eel fishery, using pots, would capture the bait eel fishery.

John Casselman asked about changes in productivity, or food web changes, that would result from urbanization. Heather asked if that would fall under habitat quality. John stated there was a physical component, but there was also a biological one. As long as it was captured there, he was comfortable. Heather indicated that in making this list, she was just trying to formulate a more focused list of important parameters.

Peter asked about changes in hydrologic regimes associated with hydropower dams. Heather indicated that changes in stream flows had been discussed in the ASMFC documents as a concern, but she hadn't been able to find much information on it. She indicated that one conservation group had raised that as an issue.

June DeWeese noted that we do know that the manipulation of the lake levels and the St. Lawrence is affecting estuarine habitat quality, and asked what the impact of that is on eel habitat? John Casselman noted we know that temperature over the last 50 years has changed, and that will definitely affect things. Things with regard to flow are less clear, and much, more complicated. John indicated the order in which issues were being listed on the board were likely a reflection of the uncertainty we feel regarding each. He noted that global climate change is definitely important. Peter noted that there would be two different

life stages affected - the leptocephali, and the yellow to silver transition. Heather added climate change to the list.

Heather reviewed the lists on the wall. She noted that we had listed contaminants as a factor regarding eggs, even though we don't have definitive data. Same is true for leptocephalus larvae and the NAO. Marci asked if there was anything to add. Peter suggested that the nutritional hypothesis needed to be added. Karin thought we didn't put that in yesterday, because we felt it was pretty speculative. Heather indicated that this list was for her to make sure that she spends her time on the most plausible issues, not the more speculative ones.

Heather continued her review. Regional fishing is an issue. Habitat quality is an issue, including low pH. She indicated she would have to do some searching in the literature to capture what we wanted. Barriers are part of that, but also are a little different, since it prevents access and causes fragmentation. John Casselman felt barriers were a major issue, related to the majority of habitat loss. Joe indicated he felt that fishing and barriers were the two primary threats. [Heather clarified that she was saying that barriers...] She indicated that she would like to talk today with those folks who might have given some more consideration to the barrier issue. Heather recorded that barriers came into play with regard to movement, disruption and habitat disruption. Joe noted that Guy's paper addressed the impact of barriers on eels also in the downstream direction. Heather indicated that in the paper, she would be clear that only some barriers, and not all, have turbines.

Paul noted that on the barrier chart, it is not simply a matter of lost access, but there are also all sorts of consequences as a result of the changes in eel density, with regard to sex determination and so forth. There are all sorts of indirect effects.

Len agreed with Paul completely, and took it one step further. There are broad ecosystem changes with these dams as well, that no one discusses with regard to eels, but could play a very important role and have a direct impact on them.

John Casselman noted the concentrations of eels below barriers have increased fishing pressures in some cases.

Kim noted that we should add increased predation as well, due to the below-barrier concentrations. Someone stated it was cormorants. Wilson noted it was avian, reptilian and mammalian predators. Len noted that it was cannibalism also.

Heather moved to the yellow-silver transition and reviewed the factors we had listed. Escapement and quality of the spawners was an issue. Fishing on silvers, along with turbine mortality, are likely the two biggest factors. Catherine noted that contaminants from wastewater discharges was an issue. Peter noted that water degradation in ports was an issue. Peter noted that Lake Ontario was less productive than it used to be, so the rate of change to silvers was lower than it would be, because the growth rate is slower.

Heather indicated that she had parasites on the list as well.

Marci asked for anything else. There were no other suggestions. Marci indicated that we had now captured all the major threats at each life stage. Rob noted that all these happen before any eel spawns once, and noted it was rather amazing that there are any left at all.

Heather suggested that the list of threats could serve as a useful guide to restoration. You do wonder how the eels get out of the freshwater system at all. She noted it is a good thing that we have estuarine and marine habitats. Rob asked about the 80 percent loss figure for American eel. Heather noted that came from the Busch et al. work, which was never intended for American eel, and there are better methods available now to make new estimates.

Catherine asked about exotic species, such as the zebra mussel, and the Chinese mitten crab. Are they important enough to mention? Heather wondered if we should put that under habitat quality. Catherine suggested that if the crabs became very dense, they could pose a problem. The second problem she wanted to raise is that injury could be a factor. Heather noted that she had just written it on the list. She

noted that we had discussed at the last workshop the fact that the injury rate, short of mortality, was also needed. Heather agreed it was pretty significant at facilities with turbines, but noted again that not all barriers have turbines. The injury and mortality rates vary depending upon the type of turbines and facility design.

Catherine asked about diseases. Heather indicated she would add viruses.

Peter suggested that we should add our own ignorance as a major threat factor. Heather agreed and noted she was attributing that list addition to him.

2:13 p.m.: Marci asked Heather if there was anything else. Heather indicated that we had gotten a lot from everyone. She noted that she had everyone's addresses and would send them the summary, although there might be something posted for the public on the website. Heather indicated that she might ask Wilson to send his notes out to the group, so they could help fill in any blanks.

Marci noted that Mike Twohey had checked with the front desk, and if a group wanted to go to Niagara, they would arrange for a taxi van, to take a group there. Heather suggested that if there are only a few people who want to go, they should talk to Joe, because he really wants to go.

2:17 p.m.: workshop was adjourned.

Heather Bell – Coordinator – American eel status review

Mike Twohey – Workshop coordinator

Marci Caplis – Facilitator

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