TO: Mr. Wesley Patrick, Atlantic Sturgeon Status Review Coordinator
FROM: Dr. David Secor, Professor, Univ. MD Center for Environmental Science
SUBJECT: Review of Draft Status Review of Atlantic Sturgeon (10 October 2006)

10 November 2006

I found the review comprehensive up-to-date and carefully conducted. I have several more significant criticisms that I hope can be addressed in the version related to sub-population designations and the attribution of bycatch analyses to the ASMFC Atlantic sturgeon technical committee, which were not contained in our report and did not represent consensus opinion on best available science by the committee. I've detailed these under the issues you've requested directed responses.

a. Is the species delineation supported by the information presented and currently available?

I concur with the designated distinct population segments. They were well justified on the basis of genetic analyses presented in review. That multiple independent genetic analysis supported the same general delineation strengthened inferences on geographic delineations between populations. The delineation was also justified on the basis of eco-regional locations and likely zoogeographical breaks in the species range.

I did not find the criteria for current spawning status consistently applied. The strongest evidence for reproduction within a river/estuary will be the presence of YOY juveniles, yet frequently yearlings and older juveniles or the capture of a single or few adults was taken as definitive evidence for recent spawning. I think these latter instances should qualify as a "possibly" categorization. This would apply to Penobscot, Pamlico, Cape Fear, Waccamaw, Pee Dee, and Santee systems.

b. In general, does the status review report include and cite the best scientific and commercial information available on the species and threats to it and to its habitat?

I think great lengths have been taken to incorporate relevant up to date literature and consult with knowledgeable scientists. The review is very current. I wish to urge authors to make sure that those scientists providing personal communications are well aware of the information they are providing to this review and they have been appropriately references. My caution stems from references to assessments conducted by an ASMFC-sponsored workshop on sturgeon by-catch in February 2006, where many analyses were presented and vetted, but the working group (comprised primarily of ASMFC Atlantic Sturgeon Technical Committee members) rejected using a previously published bycatch rate matrix (Stein et al. 2004) due to changes in fisheries and issues of over-interpolation in that matrix (by-catch rate x fishery x month). This concern receives only minor treatment in the review and analyses are given in the review that were not accepted by the ASMFC by-catch working group, despite language that seems to indicate that it was. I strongly recommend (here as Chair of that working group and the Technical Committee)

that the review limit its review of current by-catch rates (post-2000) to statements made in our report (attached). To do otherwise is an erroneous and biased reflection of that activity. For instance, nowhere in the report did we give a recent bycatch mortality estimate, which is presented as several hundred sturgeons in the Review. I do not think this necessarily curtails application of the Stein et al matrix to the more recent period, but in so doing, the SRT must accept and give acknowledgement to uncertainties in doing so. Also if individual scientists wish to provide analyses on bycatch, I think this too is OK with respective caveats but the SRT must accurately reflect that this is their work – not the activity of the ASMFC Technical Committee. Our work on bycatch estimates from the NMFS observer dataset remains incomplete – awaiting a more focused workshop, which is planned early next year. I have provided more detailed criticisms on the bycatch section below.

c. Concerning extinction risk analysis, is the methodology used appropriate?

I agree that this can only be done on subjective basis currently, which is unfortunate. In reading through these sections it sometime seems to be voting contest on whether one DPS is categorized x or y. You may wish to reword this to specify whether callsifications are due to uncertainty or lack of consensus. Also in the executive summary, last paragraph, I urge you to be more careful in indicating rationale for DPS classifications for GOM and SA – not clear if it was uncertainty or moderate risk factor that led to classification as not threatened.

d. In general, are the scientific conclusions sound and derived logically from results?

For the most part yes. As indicated above, I think critical assumptions need to by further emphasized in the bycatch section.

e. Where available, are opposing scientific studies or theories acknowledged and discussed?

In the extinction risk analysis I was surprised that water quality and hypoxia were not indicated as stronger factors for Chesapeake DPS given our evidence that these factors limit carrying capacity, and the emphasis these factors received in the Carolina and SA DPS – seems inconsistent and erroneous. I am concerned that this may have been a "votes in the room" artifact rather than a more objective treatment that was curtailed given lack of quantitative framework for the analysis.

Specific Criticisms

P. 1, last sent. Confusing – is it analyses or lack of data that drove a no listing determination for SA and GOM?

p. 8. I don't think information from Penopscot R. supports definitive classification or extant population there. One presumed adult – seems scant evidence. It should fall under "possibly" categorization.

P. 13, line 8. Actually, I thought the mark-recapture assumption was that there was similar mortality between wild and released fish over the time at large. You may wish to double check this assumption with study authors.

P. 14, 3rd paragr. The recent uptick in HR juvenile abundance observed through these monitoring programs deserves additional emphasis in this section as it is referred to later in the Review.

P. 15, 1st paragr. I believe intended word was unequivocal rather than equivocal.

P. 15, 2nd paragr. This decline in DE CPUE during 91-04 is substantial and worrisome. You may wish to reference this later in report in justifying classification for associated DPS.

p. 18, 1st paragraph. exists should be exist.

P. 19-21. Here as in Penobscot, Review should be consistent in application of criteria. The Pamlico, Cape Fear, Winyah Bay, and Santee systems seem definite "possibly" classifications for extant populations given uncertain evidence of YOY presence in recent years. "Possibly" should cue readers to give priority to understanding the true nature of these past sub-populations, much like occurred in the James, where increased science and monitoring in recent years now supports a more definitive classification.

P. 31. You should define subpopulation (carefully) with this first useage.

P. 36, 1st paragr. I do not find this EPA categorization very useful, nor do I suspect other scientists and managers will find it useful. Recommend omitting it from Review – adds little to assessment of these systems.

P. 36-56. This section on habitat information is a bit eclectic in that it focuses on primary threats to each system, but does not provide consistent information across systems. For instance, is DO as much of a threat in St. Lawrence as in Chesapeake? So written, this could be a tedious affair, which suggests tabulation might be good presentation for habitat information. In some ways, habitat issues have been addressed in a very coarse way in Table 18. Still, it might be useful to have table specific to habitat threats and their likely importance across systems associated with this section. One threat that gets very

uneven treatment, which means it probably doesn't get sufficient treatment, is spawning habitat. Many of us feel this is limiting and this comes out nicely in the Chesapeake discussion on p. 46. Still, I would like to know how siltation/sedimentation, changed flow and other likely anthropogenic changes have limited spawning areas historically and in recent times. Indeed, this might deserve a separate column in Table 18 – status of current spawning habitat.

P. 41. Taunton River section. It seems unusual that DO would be unfavorable during spawning season. Usually low DO is significant only during summer and fall months. Review should carefully indicate when low DO occurred in Taunton and whether this was indeed likely to affect spawning (I would guess rather it would affect nursery habitats).

P. 45. First paragr. "furing.."?

P. 51. 2nd paragr. Systems in Canada are larger than Santee Cooper. I believe Santee was historically the 4th largest system on the east coast.

p. 55. Satilla River section. First sentence needs work.

P. 56. Summary. Chesapeake – low DO occurs in shoal waters as well as in important presumed nursery areas for sturgeons (Nikl and Secor 05) and continues into fall.

P. 58, 2nd paragr. last line – which state – NJ or NY?

P. 60. 2nd paragr. line 4 ..monitored trips should be monitored trip. Also, this bycatch rate is highly biased for weakfish-striped bass as it may only represent a single trip where a single sturgeon happened to be captured (note total observed catch weight). This type of bias is a classic sampling error bias and is reflected in that all other bycatch rates fall below 0.03. I think this provides an important opportunity for SRT to explain difficulties inherent in analyzing the NMFS observer data set – that coverage is uneven across fisheries and months and where observation rates are low for particular fisheries – high error rates in either direction are likely.

P. 60, 2nd parag. line 6-7. Please be careful to indicate what these percentages refer to = % of recapture sample of tagged sturgeon. Also, for disclosure sake, sample sizes should be indicated by fishery. In last two sentences of this paragraph, you suddenly switch back to Stein et al. analysis and this should be more carefully noted (citation again given). Also, the period for each of these estimated bycatch numbers (annual or 10 year period?) should be given careful specification.

P. 60, 3rd paragr. I do not think it is a proper representation to describe presented research (ppt presentations and distributed preliminary analyses) at the ASMFC workshop as best available science on bycatch. While the general approach of using MPUE can be represented, the specific analyses performed were preliminary and not fully accepted by the Technical Committee. They require more vetting by experts

familiar with the NMFS observer program and the corresponding data set. It is therefore not appropriate to give specific rates of bycatch mortality or absolute bycatch takes (i.e., 460). It is only appropriate to cite material in our consensus report of the February 2006 meeting and these figures occur nowhere in that report (attached).

P. 61. 1st paragraph. The Technical Committee DID NOT conduct or reach consensus on the MPUE approach nor statistics reported here – i.e., individual fishery mortality rates and no. of deaths in specific fisheries. Scientists capable of doing these analyses could be cited as doing these analyses on their own but it is erroneous to cite these statistics as coming from the ASMFC Technical Committee. The issue here is that the Technical Committee conducts and approves best available science through consensus. This process has not yet occurred for these preliminary analyses. It is my strong opinion that the SRT must limit themselves and the Review to the report of the Technical Committee. Therefore, this entire paragraph needs to be redrafted and specific rates and numbers limited to those presented in the bycatch Technical Committee Report. I can see no way to cite the statistics given in this paragraph unless the individual scientist who provided them approves and is willing to be cited.

P. 61, 3rd paragr. There is an issue that should be discussed related to Stein et al. analysis. For a certain period, gill net and trawl fisheries likely *targeted* sturgeon as this fishery was allowed. Thus the very high catches in 1996 are likely not reflective of the current by-catch associated with gill nets because they are not intentionally fished in recent times to intercept sturgeon. This again implies that the Stein et al. bycatch matrix is no longer appropriate to recent fisheries.

P. 62, 2nd paragr. phrase “less effective” probably poor choice as it implies fisheries are targeting sturgeon.

P. 63, 1st paragr. The observation in DE of higher mortality with longer gill net soak times is consistent with the Feb. 2006 ASMFC bycatch workshop finding, which could be reinforced here. Last statement that managers think bycatch is *vastly* underreported requires citation.

P. 63, 2nd paragr. “The ASMFC stock assessment assumed….” Which stock assessment was this? Is this a reference to the bycatch report and if so, I do not believe we reported a coastwide 5% mortality rate in trawl fisheries.

P. 63, 3rd paragr. Here again we see evidence that sturgeons were targeted in 1996 not only in gill net fisheries but trawl fisheries.

P. 64, 4th paragr. Please specify that 3% can be removed *each year*. Also are u (exploitation rate) values estimated from USFWS tagging studies? This should be specified.

P. 64, 5th paragr., P. 65 6th paragr. This is a mix of elements that were not contained in the workshop report and others that were. This paragr. should be redrafted and estimates

re-performed that conform to those assumptions listed in the report. First we used bycatch sink gill net sturgeon take estimates from Stein et al. (not specifying monkfish, not adjusting for recent period), so these values will in fact be higher than 400 used in the Review. Then we applied the 25-100% as indicated and compared it to likely sustainable takes for HR as performed in the Review. To be clear, you should not specify monk fishery here; you should use Stein et al. sturgeon take estimates from sink gill nets. Doing so will modify your interpretations (takes will be higher), but conform with original implications of Review and the ASMFC Techn. Committee report – "by-catch…is of amplitude that would substantially curtail recovery of Hudson River population."

Here and elsewhere I urge the SRT to report findings directly from the Bycatch Workshop Report. We took great care that this was a consensus document. More careful treatment of the report will reduce error in over-extending/interpreting what occurred there, but at the same time strengthen many interpretations made in the Review that were consistent with findings of the Technical Committee.

P. 66, First paragraph. Assumptions noted by ASMFC TC members and other experts should be noted. Principally, many fisheries are poorly represented overall and seasonally in the observer database. As highlighted above this can cause important sampling error and bias in interpolated bycatch rates. In the recent period, the TC noted that observer coverage was un-even across principal fisheries that take sturgeon (monkfish fishery) on inter-annual and seasonal basis, which was a principal reason we did not apply the Stein et al matrix approach or the proposed MPUE approach. I think it should be noted that the SRT is venturing down a path that ASMFC TC committee felt was too uncertain to pursue until a dedicated analysis of bycatch matrix error could be performed. That they did so may still be justified as a first cut, acknowledging uncertainty, etc. but I think for accuracy sake a clear statement about what SRT was prepared to assume relative to ASMFC TC should be made.

P. 66 3rd paragr. What does changes in biota refer to? Do you mean changes in targeted fisheries, fishery behavior? Also the Review cites 158 captured Winyah sturgeon as evidence of conservative, but this statistic is over 10 years old. Could not populations/fisheries have changed so that this statistic is no longer pertinent?

P. 67. 1st paragr. From scientific point of view, I cannot support application of state bycatch rates to all fisheries. This is far too uncertain. Also, as 0% mortality is applied to these fisheries, what is the real point here? I think its best to work with greater certainty in computing defensible bycatch numbers and assume that this represents an underestimate.

P. 67. Is this Goosefish rate (should be consistent – goosefish or monkfish) correct? Seems too high and too invariant given data I've seen from NEFO database. Would advise rechecking this figure.

P. 68, 1st paragr. Again 50% mortality in monkfish fishery seems too high, except for where soaks are longer than 48 hours.

P. 68, 2nd paragr. Do not cite sturgeon mortality figure from ASMFC TC – we did not reach consensus nor provide such an estimate.

P. 68, 3rd paragr. Despite my objections to how our ASMFC bycatch workshop activities and report were incorporated into the Review, from a scientific perspective I do not find the range of possible deaths per year outside a realistic frame.

P. 75. 6th paragr. Subjective wording – poorest survival observed as 87% - suggest lowest survival….

P. 79, 3rd paragr. last line. I don't see why a change in size structure represents a recovering population. It could indicate a past successful year-class moving through the population, or perhaps an absence of recent strong year-classes. I think stronger evidence for any recovery comes from HR monitoring data and comparisons of recent adult abundances to those 1-20 years ago in the Atlamaha.

P. 79, 4th paragr. Do not give 400-500 death estimate attributed to ASMFC TC. Not in report; not an accepted figure. SRT estimate of byctach mortality should be conditioned here on some of the principal uncertainties in this estimate.

P. 80, 4th paragr. Statement on shortnose sturgeon in 55-60 km section of HR seems strange. Is this important – seems emphasis should be that shortnose in one study occurred at a certain depth range. This statement seems to imply very limited horizontal distribution as well, which I don't think was the intent.

P. 81. In discussions on predators, I think it's important to recognize that large mouth bass and channel catfish are both introduced and abundant large predators to Atlantic coastal estuaries, that could potentially prey and compete with young sturgeons.

P. 84. There is recent work that shows that the *Pfiesteria* was mis-identified and that it was other HABs that likely affected some fish and caused fish kills. Also, hypoxia and eutrophication can lead to fish kills that were historically attributed to HABs. In that sturgeon kills have never been linked to HABs limits relevance further. Alternatively, a general and limited discussion on HAB's and fish kills might be relevant in giving some context to lack of observed effects on sturgeons to date.

P. 90. I think EPA Chesapeake Bay Program water quality criteria are worth discussing here as an example of where water quality criteria were specified to be protective of sturgeons. Original water quality criteria were c. 2 ppm, but these were increased to 3.5 and higher to take into consideration sturgeon's higher DO requirements. I have attached a slide that shows these criteria now in force.

P. 92. I think VA listed Atlantic sturgeon as endangered in the 1970s. You should check on this.

P. 98. Insert sturgeon after Siberian.

P. 101., last paragr. See my earlier comment on this mark recapture experiment. Is this the correct assumption?

P. 108. Here but not in Table 18, Toxics seems to be over-emphasized. There is scant evidence that contaminants are curtailing recovery. By dint of their marine migrations sturgeons can effectively depurate and growth-dilute contaminants that are accumulated through their use of estuarine ecosystems. On the other hand, there is very solid reason to expect that water quality – DO and temperature - are critically important and that is not well reflected in these priorities.

P. 113. 2nd paragr. As indicated earlier, I do not find this evidence of recent reproduction in Penobscot compelling.

P. 115. I am puzzled that water quality issues did not rank higher in Chesapeake. This is inconsistent with quite a bit of scientific evidence to the contrary. Reasons should be provided for this inconsistency between science and SRT opinion.