

Comments by John Waldman on Atlantic Sturgeon Status Review Report 10/31/06

All in all, I found this to be a carefully considered, detailed, and useful report. I especially enjoyed the comprehensive gathering of disparate sources of survey results and anecdotal observations.

I thought the report does incorporate the best possible information on status and threats, the extinction risk analysis is defensible, and that the conclusions are (mostly) sound.

I do have issues with the DPS analysis. The first is that I believe the concept itself is flawed. That is the criteria for balancing population discreteness versus significance are not clear enough.

What we have here is recognition that Atlantic sturgeon stocks are genetically distinct at the population level: each geographically separate river's population is discrete from every other. But then some soft ideas on ecology are introduced on pages 27 and 28. For instance, if the fall lines of rivers vary greatly latitudinally, then why isn't it that the physical regime itself and not behavior accounts for differences in spawning locations? Also, it's entirely expected that broad habitat classifications would coordinate well with genetic groupings because habitat is related to biogeography. Rivers within a biogeographic zone should be more similar to each other ecologically and habitat-wise than outside the zone, and populations cluster genetically because biogeography constrains them, so we have logical circularity here. My feeling is that such arguments are thus rendered trivial.

Similarly, on page 28, it is stated that DPS criteria requires further proof that genetic differences are significant relative to the species' fitness as a whole. But the rejoinder is a non sequitur—I don't see how classification accuracies have anything to do with this concept.

Later, on page 111, the proposed DPS scheme is filtered through a matrix of four criteria. But if the DPS segments are at a higher level than a single population, then there is confoundment with the status of single populations. The single populations appear to qualify as DPS based on #3, that they represent a genetically unique population. And also, for #4, that if lost, they would result in the loss of spatial structure within the DPS.

What the DPS scheme is doing is recognizing uniqueness at the population level but then drawing the segments one or more levels higher in the tree that estimates relationships among populations. So what does it mean for the DPS as a whole if one population is robust and one is facing extinction? Is it still a useful concept? I don't think so.

I hate to be so negative on this because I really like the report as a compendium, and I think the problem is with the concept, not with the authors' treatment.

Specific comments:

p.4, middle paragraph, should state that subadult and adults *may* travel widely, it's not a given that all individuals do so. Also, the term "stream" doesn't really apply to sturgeon. Same paragraph—didn't Bain find that some adults linger in the Hudson after spawning?

p.5, the statement that juveniles greater than age-0 does not provide evidence of spawning may be too stringent; it may be that age-1 also is a reliable indicator.

But this brings up a peeve of mine. Fisheries scientists generally consider juveniles of fishes to be age-0, yet this report and most sturgeon papers consider juveniles to be equivalent to subadults. The problem is they use the terms rather interchangeably, which confuses readers who are used to thinking of juveniles as the year class preceding yearlings. This started with Dovel, I believe, and remains a problem with sturgeon science.

p.6, I'm having trouble accepting the 300 per year figure. Yes, we know the Hudson and Altamaha stocks are relatively robust, but I'm not sure the Kennebec/Androscoggin and ACE Basin South Carolina populations aren't of the same magnitude.

p.12, rivers are out of sequence, Thames is north of CT River.

p.15, Are carcasses of large individuals really reported annually? Implies that it occurs without fail.

p.16, Waldman et al. (1996) also showed mtDNA evidence of a remnant Delaware River population (but it was equivocal). Concerning King's paper, you might provide a caveat that they treated Delaware samples as if they form a discrete stock, which is unlikely.

Use different subheadings for states above that of individual rivers.

p.26, for the "fall spawning run," is there documentation of release of gametes in autumn or is this just a movement upriver in preparation for spring spawning?

p.80, the competitor species seem pulled out of a hat. Cunner compete with Atlantic sturgeon?

Finally, the Science paper by Arne Ludwig et al. on Atlantic sturgeon being found in Europe should be referenced somewhere.