Supply and management of oyster harvests in the Chesapeake Bay: An examination of historical factors and their implications for introduction of non-native oysters and targeted alternatives

Final Peer Review Report and Lead Agency Response

Report Content and Charge:
This report describes the peer review process and presents the Lead Agencies' response to the peer review. Also included are the names of the peer reviewers and their organizational affiliations and a compilation of all the peer review comments on the research project titled, "Supply and management of oyster harvests in the Chesapeake Bay: An examination of historical factors and their implications for introduction of non-native oysters and targeted alternatives," and the responses from the Principal Investigator (PI) to the peer review.

Mr. Robert Wieland, the PI on this project, developed five research reports out of this single research project. These reports were developed as supporting documents for the Programmatic Environmental Impact Statement (EIS) for Evaluating Oyster Restoration Alternatives for the Chesapeake Bay, Including the Use of Native and Non-Native Oysters. The process followed for this peer review is consistent with the peer review plan that was developed by the Lead Agencies for the EIS project. This peer review plan was specifically designed to comply with the December 16, 2005 Office of Management and Budget's Peer Review Guidelines and was accepted by the US Army Corps of Engineers for this purpose.

Study Objectives:
The objectives of this research were to determine harvest and production costs in the Chesapeake Bay oyster fishery and to analyze net returns in the industry over the recent past. These activities were conducted to inform prospects for a public *Crassostrea ariakensis* fishery and alternative management actions specified in the Programmatic Environmental Impact Statement (EIS) for Evaluating Oyster Restoration Alternatives for the Chesapeake Bay, Including the Use of Native and Non-Native Oysters. Research objectives also included examining publicly-funded oyster restoration and management efforts and specific management initiatives and analyzing these for comparisons of net benefits to the proposed action and alternatives presented in the EIS.

The Lead Agencies Review and Response Process:
The PRG received the five research reports, three solely authored by Mr. Robert Wieland, one authored by Mr. David Herberich, a Ph.D. student working with Mr. Wieland, and one report coauthored by Mr. Wieland and Mr. Stephen Kasperski. The first four reports, were submitted to the PRG on January 8, 2008 and the fifth report was submitted to the PRG on August 21, 2008:

1. Wieland, R. (Aug 16, 2006). Operating Costs in the Chesapeake Bay Oyster Fishery. Prepared for the NOAA Chesapeake Bay Program Office Non-native Oyster Research Program.
2. Herberich, D. (Nov 26, 2006). Estimated Return to Harvest due to the Maryland Department of Natural Resources Repletion Activities 1990-2006. Prepared for
3. Wieland, R. (February 20, 2007). Managing Oyster Harvests in Maryland's Chesapeake Bay. Prepared for the NOAA Chesapeake Bay Program Office Non-native Oyster Research Program.

4. Wieland, R. (December 12, 2007). Costs and Returns to Oyster Aquaculture In the Chesapeake Bay. Prepared for the NOAA Chesapeake Bay Program Office Non-native Oyster Research Program.
5. Wieland, R., Kasperski, S. (July 21, 2008). Estimating Net Present Value in the Northern Chesapeake Bay Oyster Fishery. Prepared for the NOAA Chesapeake Bay Program Office Nonnative Oyster Research Program.

The PRG commented on each report separately. PRG report comments for the first four reports were compiled and sent to the PI on March 3, 2008. PRG comments for the fifth report were sent to Mr. Wieland on October 10, 2008. Mr. Wieland and Mr. Herberich prepared revisions and responses to PRG comments received. Mr. Wieland sent revised reports and responses for "Operating Costs in the Chesapeake Bay Oyster Fishery," and "Managing Oyster Harvests in Maryland's Chesapeake Bay," on March 11, 2008. Mr. Wieland revised and submitted "Costs and Returns to Oyster Aquaculture In the Chesapeake Bay," on June 19, 2008 and after consultation with the PRG, re-submitted the report with additional revisions on July 21, 2008. The PRG comments to Mr. Herberich indicated that substantial revision of his report was required. Mr. Herberich responded to the comments on March 13, 2008 reporting that he was unable to address all of the PRG comments, or to significantly edit the report because, in his words, "research interests have led [him] to other areas since writing the report." Mr. Herberich also stated that with the availability of more time and funding, he would have re-run the data analysis using a different method. Mr. Wieland revised the report "Estimated Net Value of the Northern Chesapeake Bay Oyster Fishery" in response to the PRG comments and submitted to the PRG on November 7, 2008.

The PRG found the revisions of the reports and the comments provided by Mr. Wieland to be adequate and did not have additional comments.

The Lead Agencies are satisfied that the key concerns raised by the PRG have been addressed.

The remainder of this report presents the PRG's review comments and the Principal Investigator's Response to the PRG's reviews. The reviews and responses have been organized by report (in the order listed above).

Deposition of Peer Review Group:

Dr. James Anderson (Lead Reviewer)
Department of Environmental and Natural Resource Economics
University of Rhode Island

Dr. Keith Criddle
Fisheries Academic Program
University of Alaska Fairbanks

Dr. Walter Keithly
Department of Environmental Studies
Louisiana State University

Dr. James Opaluch
Department of Environmental & Naturals Resource Economics
University of Rhode Island

Peer Review Group Evaluation

Wieland, Robert (Aug. 16, 2006)
Operating Costs in the Chesapeake Bay Oyster Fishery

Prepared for:
The NOAA Chesapeake Bay Program Office
Nonnative Oyster Research Program

James L. Anderson, Peer Review Group Chair
University of Rhode Island

March 3, 2008

The Peer Review Group has completed its evaluation of the report:

- **Wieland, R. (Aug. 16, 2006). Operating Costs in the Chesapeake Bay Oyster Fishery. Prepared for The NOAA Chesapeake Bay Program Office Non-Native Oyster Research Program.**

The Peer Review Group considered the validity of the research design, the quality of data collection procedures, the robustness of methods employed, the appropriateness of the methods for the hypotheses being tested, the extent to which the conclusions follow from the analysis, and the strengths and limitations of the research project.

The Peer Review Group came to relatively consistent conclusions regarding the report. Overall, the study was evaluated as useful and basically sound. However, some concerns were raised, specifically: a) whether or not the small sample is large enough and whether the sample provides an unbiased representation of the population of oyster fishermen; b) the accuracy of survey responses; and c) the manner in which labor opportunity costs are estimated.

The information regarding estimates for costs of harvesters in Maryland's oyster fishery (including differences in cost for different gear types) is very important in supporting various policy questions regarding management of oyster populations and could be useful in forecasting behavior by harvesters under various policies.

2. Document: Operating Costs in the Chesapeake Bay Oyster Fishery (Wieland)

Reviewer 1 Comments:

General Comments:
This is a relatively well-written document that attempts to estimate operating costs in the Chesapeake Bay oyster fishery. As indicated in the report, estimating these costs is not as straightforward as some may think, and the author does a credible job prorating a number of the fixed costs. However, the small sample size does concern me; particularly when that small sample is then partitioned by gear and high and low producers. Unfortunately, limited information on the population precludes one from examining whether the sample is representative of the population. The information presented in Table 1 would suggest, however, that the number of participants by gear type is known and the discussion, at a minimum, should identify the proportion of population (by gear type) that was sampled.

Specific Comments:

1. p. 3: The statement at the top of this page (i.e., In the 2004 and 2005 seasons, shaft tongs proved too inefficient to cope with reduced oyster abundance" needs some additional explanation. It may be useful to steer the reader to the Table on p. 47 of the "Managing Oyster Harvests in Maryland's Chesapeake Bay" report which illustrates the sharp decline in boat days associated with tonging.
2. p. 4 (3rd paragraph): It is noted that the share in total harvest by power dredging declined in 2006. This may be the case but Figure 1 included data only through 2005.
3. p. 5: While probably correct, the author needs to use some exercise regarding the relationship between efficiency and ability to survive in the fishery. Specifically, while patent tongs may be less efficient then power dredges, the reduced costs associated with the former may more than offset any reduced efficiency. This is exactly what the author attempts to examine in a subsequent section. Also, it may be the case that less efficient gears account for a smaller proportion of landings only because more efficient gears were previously prohibited.
4. p. 6 (3rd paragraph): I think that the statement "the purchase price statistic is not significantly different from zero" should state that the variance in ….
5. p. 6 (last paragraph): some additional explanation needs to be given regarding why a 30-year depreciation schedule with a 25% salvage value is used for highend boats while a different depreciation schedule (and salvage value) is used for low-end boats. I assume there is a rationale for using these two schedules but without additional information, I cannot determine whether the differences are justified.
6. p. 8 (3rd paragraph): Something appears to be wrong with respect to trade association fees. For the costs among 16 non-zero respondents was $182. Therefore, including only five zero respondents would bring the average down to about $140 (not the $40 as indicated in text).
7. I am somewhat concerned in the manner that labor opportunity costs is estimated and this concern may be very relevant given that the daily labor costs account for 50%-60% of

total daily costs. My concerns are two-fold. First, the estimate is based on only five responses. Did the remaining 16 interviewees not answer the question or did they respond “no” to the question pertaining to making money from any other activities not associated with harvests from the Bay? If they responded “no,” then this might suggest that the majority of oystermen do not have $17.30 per hour opportunity costs in which case the average opportunity costs for the industry would be considerably lower. The second concern I have is the assumption that the crew member has the same opportunity cost as the captain/owner (who is the individual who presumably completed the questionnaire). In general, crew members are compensated on a share basis and opportunity costs of the crew members are, I would think, likely to be less than those of the captain. At a minimum, I would suggest that a fuller explanation be given to why only five individuals responded to this question and whether extrapolation to the entire sample based on these five responses is appropriate.

8. It is somewhat disturbing that, with the exception of 2006, costs consistently exceed revenues for certain gear types (Figure 2). If this were the case, one would certainly assume that those gear types would no longer exist in the fishery.
   I am left with the conclusion that historical costs may be overestimated.

Based upon this review, we can now consider the criteria noted above:

- Are the conclusions adequately supported by the evidence, analysis and argument?

See discussion. If the cost estimates, as presented, are correct, then the conclusions are adequately supported by the analysis. As discussed above, however, I do have some concerns regarding the cost estimates. One of the concerns represents the “small” sample size and partition of this small data set by gear type (and high and low producers). Given the information presented in the report, there is no means by which to ascertain whether the sample size is representative of the population. The other issue of concern relates to that of labor costs. A sample not representative of the population and/or improperly specifying labor costs will tend to distort the conclusions. Without additional information, however, the amount (if any) of distortion cannot be determined.

- Are the uncertainties or incompleteness in the evidence explicitly recognized?

The author does recognize the small sample size and incompleteness in the evidence is recognized.

- Are the data and analyses adequately described and well-suited to address the questions being investigated?

See discussion. I have made several suggestions regarding increasing discussion in different sections of the document.

- Is the report fair and impartial in tone?

Yes

Reviewer #2 Comments:

Review notes on Wieland (2006)—Operating Costs in the Chesapeake Bay Oyster Fishery

Evaluation of this paper comes down to questions about whether the sample is large enough and whether the sample provides an unbiased representation of the population of oyster fishermen. Unfortunately, there is ground for concern on both of these questions. Twenty-one (partially) completed surveys is a small sample, but to judge whether the sample is large enough, the reader needs to be provided with information about the magnitude of the population that is being sampled. Unfortunately, the document does not provide an estimate of the magnitude of the population. The smallness of the sample is painfully evident when the data are subdivided into eight gear/capitalization categories (Table 1). While we are not provided with information about the number of observations in each of these categories, the mean number of observations per category is less than three. The survey respondents were not drawn at random from the population of oyster fishermen and the report does not provide evidence to suggest that the sample is characteristic of the population. Consequently it is not clear that it is appropriate to use the survey information as a basis for inferences about the general patterns of variation in costs of operating in the oyster fisher. My concerns about the small number of observations could be alleviated if there were evidence to suggest that this small sample represents a near-census of the population of active fishermen; if the survey comes close to being a census of fishery participants, it would be an appropriate description of the actual cost structure of the current fleet. However, because the information does not provide a basis for estimating economies of scale in operations and because it does not include information on the cost structure of latent capacity, it does not provide a useful basis for projecting changes in the structure or profitability of oyster fishing under conditions characterized by large changes in abundance of oysters or in their ex-vessel prices.

Sections of this report that should be revised include:

1. Include an estimate of the total number of oyster fishermen
2. Table 1 needs to be revised to indicate the number of observations in each of the gear/capitalization categories.
3. If the sample (n=21) is nearly a census, the report should clearly say so and provide supporting information. In addition, the report should provide information about the extent of latent capacity and the magnitude of barriers to entry.
4. If the sample is not an approximate census, the survey should be administered to a random sample of TFL and OYH permit holders.
5. The report should also examine a range of values for the opportunity cost of labor. Imputed labor costs are the largest component of total operating costs and the estimate is based on 5 responses.

Minor editorial notes:

1. The report does not cite papers included in the references (Kennedy et al., Lipton et al., Prochaska et al., and Tarnowski).
2. The report cites Lenihan & Peterson, but does not include it in the references.
3. Page 3, paragraph 4—"divers employee" should be "divers employ"

Reviewer #3 Comments:

Operating Costs in Chesapeake Bay Oyster Fishery
Robert Wieland

This study estimates for costs of harvesters in the State of Maryland's oyster fishery, including differences in cost for different gear types. This information is very important in supporting various policy questions regarding management of oyster populations and could be useful in forecasting behavior by harvesters under various policies.

The study implemented a questionnaire to collect information on fixed and variable costs of oyster harvesters. The sample included of 21 watermen using four different gear types. One significant challenge that was faced is that many harvesters have left the oyster fishery in recent years to do the collapse of the fishery. The study includes a description of the main gear types and the historical composition of the fishery.

Important questions could be raised about the sample size, the representative nature of the sample, and the accuracy of survey responses. Figure 1 appears to indicate that there were a large number of participants in recent years, with several hundred participants as recently as 2002. But by 2004, numbers dropped to significantly less than 100, rebounding to nearly 200 participants in 2005. One wonders how that has changed since 2005. If the number of participants continued in the 100's, there would appear to be opportunities to collect more data. Or at a minimum, it would be useful to make efforts to test for the extent to which the sample is representative of the population. Nevertheless, the information could be very valuable, and the limited nature of the responses is understandable given the collapse of the fishery, and the amount of effort required. It might also be useful to carry out sensitivity analyses based on sample responses and perhaps some expert judgment. For example, one might use the lowest and highest cost estimates in the sample to estimate bounds. One also might get input from other knowledgeable individuals as to whether costs seem higher or lower than one might expect, or whether costs seem to be pretty much as expected.

Given the context of the study of an industry suffering collapse, the present study should be viewed as a valuable first step in collecting cost data, rather than a comprehensive data collection effort.

Response to Peer Review Panel Comments:
Robert Wieland

Operating Costs in the Chesapeake Bay Oyster Fishery

General:

The absence of any report of the population size is noted and text is added at the start of page 2 to fix that. The partitioning of the data by gear type was a reasonable grouping of the sample that permitted consideration of costs specific to a given technology. By further grouping the data such that some respondents of a given geartype fall in a high-end category and others in a low-end category, I am incorporating the sample variability into the cost estimates and obtaining reasonable ranges for what those costs might be in the wider population. Because the sample is so small, the extent to which it is representative is dependent on the sample selection. The sample was randomly selected within the constraints of wanting a balanced number of respondents for each gear type. The process by which the sample was chosen is described in the methods section.

Specific comments: (Reviewer 1)

1. I have provided a statistic at the end of the first paragraph on page 3 to help quantify the decline in shaft tongers' relative harvest share over those two years.

2. I have noted that the 2006 data is not shown. I have total harvest by gear type for 2006 (and 7) but not for the over 100 bushel group separately. What I say, although not shown in the figure, is correct.

3. Relative efficiencies of the different geartypes over different abundances would be a useful thing to know. But other than generalities such as the idea that power dredging can work sparse populations better than any sort of tongs, there is not much evidence to cite. (With the exception of Lenihan, 2004.) The point that I wanted to make in the text is that, with continuing declines in abundance, it is increasingly difficult to make a living on oysters without going to more efficient gear. The industry has been accommodated in this by the relaxation of gear restrictions.

4. This paragraph is very important to the estimates developed in the paper. I have amended the text with respect to the problem with the purchase price statistic. But it is in this paragraph that I admit that the calculations that follow cannot be treated as statistically significant estimators of costs for the larger population of harvesters. If I treat the sample as single set encompassing all geartypes, variability is uncomfortably high for most of the estimates. But, when I disaggregate the sample observations along reasonable groupings such as geartype and "high cost/low cost" categories, they provide a basis for estimating costs from those several observations falling in each category. Their representativeness is unknown, but I contend that my method of drawing sample observations was sufficiently random that they do not have any known sampling bias. I have tried not to claim more statistical validity than can be justified for these measures.

5. The two different depreciation schedules for “high cost” and “low cost” boats is predicated on the assumption that high cost boats tend to be newer and “better” and that low cost boats are either old or burdened with some problem that diminished their market price. While the end value and time of depreciation are somewhat arbitrary, I believe that this mirrors what happens in the market for boats. I have added a footnote describing this rationale.

6. The reviewer is correct and I have replaced the wrong number with the correct one ($137). This works through to a $0.97 difference in the total daily summary costs reported in Table 4.

7. I used the opportunity cost of labor as estimated by the reported hourly wage of respondents (only five responded) who gave me information on question 30 on the Survey Questionnaire. This is a defensible measure of expected returns to labor if one abstracts from non-pecuniary benefits to working this fishery. During the period of the survey, the construction and services sectors around Maryland’s Bay were experiencing robust growth and relevant jobs were readily available at $15 to $20/hour. As the reviewer notes, the watermen who I surveyed were largely not taking those jobs, so maybe they have a higher reservation wage. Or, maybe (as suggested in a footnote in the management paper) harvesters derive non-pecuniary benefits from working on the water such that they will accept a lower wage to do that. In which case using returns from alternative employment is less useful for estimating the true variable of interest – the net that they need to earn per day to make working the fishery worthwhile. However, without testing this conjecture, the opportunity cost seems a credible estimator for this variable. But I add a footnote explaining this.

With respect to shares and expected returns for captain and crew, the shares that were described to me were often one third for the boat, one third for the captain and one third for the crew. If it was friends working together, each would often take what they caught and the one who did not own the boat would pay fuel costs. The reported cost shares hold up pretty well when compared against these divisions of harvest revenue.

8. The reviewer is concerned about the sustainability of an industry made up of firms that do not appear to be covering their costs. I agree. It is not sustainable in the long term. But, it can go on for a while, because the boats are there, and there are crabs in the summer and rockfish and perch and other fisheries that can sustain the waterman as the oysters are fished out. Given the overall decline in the fishery over the period, costs in excess of revenues seem to me a very probable circumstance.

(Reviewer 2)

General: This is clearly a cost study based on limited cases that (as above) are representative of the industry only in as much as the sample selection credibly captured the range of costs. I have not claimed anything else. It is not uncommon to estimate costs by way of “case studies” and, given the difficulty in getting watermen to respond to a survey, treating the limited respondents for each geartype as representative “cases” seemed the most practical approach. It is not so elegant and it is only as good as the sum of its judgments, but I have tried to give the reader a basis for judging those judgments.

That the study might not "provide a basis for estimating economies of scale in operations … [nor] include information on the cost structure of latent capacity,…[nor] provide a useful basis for projecting changes in the structure or profitability of oyster fishing under conditions characterized by large changes in abundances…" is a bit more damning. The study secured a set of observations that allow an estimate to be made of harvest costs, based on that sample. The sample was randomly generated, with the exception that I targeted ports where boats of a specific geartype tended to come in, to ensure representation of all gears in the sample.

I am not sure how economies of scale are relevant to an industry that by all reasonable expectations is based on a shrinking resource. But I had hoped that the study results would be adequate to predict changes in structure or profitability under different oyster abundances. I am not sure why the reviewer deems them as not, except for the question of representativeness. Part of the goal in separating each geartype into high cost and low cost segments was to show the range even within the (admittedly small) sample. As I note in the discussion of the sampling frame and methodology, I believe that the sample is representative of both the averages and the variability in the total population. There is no way to prove that, short of sampling a larger population, and that would only be practical with a better resourced study.

Specific comments (Reviewer 2)

1. Done

2. Done

3. N is variable throughout the year. If you divide the number of boat days effort in the 2006 season by some assumed time utilization rate, say, 50 days per season, then there were 174 boat-season-equivalents working the fishery. By that measure the study surveyed over ten percent of the effective population. Assume a higher utilization rate and there are fewer season-equivalents working the fishery. Assume a lower utilization rate and you begin to stretch the definition of "commercial" fishery.

There is overwhelming latent capacity in this fishery. Even though oyster stocks have collapsed one can still keep ones boat afloat during other seasons of the year, working other fisheries. There are regulatory barriers to entry through the limited entry program for OYH licenses. However, these are currently undersubscribed, and many (if not most) harvesters use a TFL to harvest oysters. There are, as noted, over 2,000 TFLs. Only a portion of these are used to catch oysters but all of them could be (with payment of the surcharge and oysters to catch). With respect to non-regulatory barriers to entry, I do not believe that these are all that high, but this was not part of the study.

4. In a random survey of OYH licenses about 2/3 of the people receiving the survey would not be active in the fishery. The ratio of active harvesters to licensed harvesters among TFL holders is then about one half, using the 2006 total harvester numbers. One could limit the survey to active OYH or TFL licenses from DNR records but there are some legal hurdles to clear first. The watermen expect that it is impossible to link harvest information with any given license outside of DNR commercial shellfish recordkeeping. Once that legal hurdle was cleared, you would

need to find enumerators who were able to undertake face to face interviews with willing participants. A large response from either a phone or a mail survey is not likely.

5. The cost analysis was sufficiently defined such that any reader can change the cost of labor if they believe that they have a basis for doing this. Additional text is added to the conclusion pointing this out.

Peer Review Group Evaluation

Herberich, David (November 26, 2006)
Estimated Return to Harvest due to the Maryland Department of Natural Resources Repletion Activities 1990-2006

Prepared for:
The NOAA Chesapeake Bay Program Office
Nonnative Oyster Research Program

James L. Anderson, Peer Review Group Chair
University of Rhode Island

March 3, 2008

The Peer Review Group has completed its evaluation of the report:

- **Herberich, David (Nov. 26, 2006). Estimated Return to Harvest due to the <u>Maryland Department of Natural Resources Repletion Activities 1990-2006</u>**

The Peer Review Group considered the validity of the research design, the quality of data collection procedures, the robustness of methods employed, the appropriateness of the methods for the hypotheses being tested, the extent to which the conclusions follow from the analysis, and the strengths and limitations of the research project.

The Peer Review Group identified several problem areas in this study. The principal concerns are: a) There has been no attempt to standardize man hours by gear type.
Failure to standardize man hours could lead to some estimation problems, the extent of which is unknown. b) It is unclear why the harvest equation includes shell plants in t-1 and t-2. Assuming it takes three-to-four years for cultch to set and the oysters to reach maturity, it would seem that a lag structure on shell plants similar to that for natural spat would be more appropriate than the current model specification. c) Why is the national oyster price considered in the analysis rather than the Maryland annual price? d) We do not agree that "[t]his preliminary analysis lends support to the DNR's repletion effort….
(p. 9)." e) Misspecifications in the model lead to findings that there is a large negative return on investment with respect to shell plants. f) It is unclear why cost should depend upon stock for a given level of effort. Further, it is not clear that the econometric model is of any use in guiding policy.

In summary, the statistical results appear to be contrary to expectations given the system being analyzed, and thus may be an artifact of data. We believe it would be a mistake to use these results to guide policy.

1. **Document: Estimated Return to Harvest due to the Maryland Department of Natural Resources Repletion Activities 1990-2006 (Herberich)**

Reviewer #1 Comments:

General Comments: This is a very well written document which, based on empirical estimation, presents some very interesting and potentially useful results. I have a number of comments/concerns however that are briefly considered below:

1. As discussed in Wieland's paper (Operating Costs in the Chesapeake Bay Oyster Fishery), there are four types of gears currently being employed in the fishery with efficiency varying by gear type. As indicated in his report, furthermore, the proportion of harvest by gear type has changed since the 1990s. In the Herberich report, however, effort is measured only in terms of reported man hours (p.1) with no attempt to standardize man hours by gear type. Failure to standardize man hours could lead to some estimation problems, the extent of which is unknown.
2. While proper model specification can be very problematic in examining a biological system (or almost any system), it is unclear to me why the harvest equation includes shell plants in t-1 and t-2. Assuming it takes three-to-four years for cultch to set and the oyster to reach maturity, it would seem that a lag structure on shell plants similar to that for natural spat would be more appropriate than the current model specification. I do not know if such a change in specification would significantly alter results but I believe some discussion is in order.
   Similarly, the purpose of planting shell in the Chesapeake, I would assume, is to establish/rehabilitate reef. While marketable oysters associated with these shell plants may be forthcoming after three-to-four years, maximum production may not occur for several more years (if properly managed) as the reefs become more fully established. As such, I wonder if a longer lag (including, perhaps, some type of distributed lag model) may be more appropriate for the analysis.
3. I am somewhat troubled by the findings that indicate that repletion efforts have had only a 'minimal" effect on harvest yet a much more significant effect on the effort decision. If I am conceptualizing the issue correctly, the harvest model indicates that if you were to hold effort constant, repletion efforts would have only small positive impacts on harvest. This would suggest that repletion efforts do not significantly influence stock size. Since repletion activities do not significantly influence stock size, could then conclude that repletion activities do not significantly influence profits to the boat or industry. Given this to be the case, why would industry effort be much more responsive to repletion activities? This would certainly imply a large degree of naivety by the oyster fishermen. While there may be some learning curve (i.e., fishermen seeing repletion activities in time period t in area j and thereby expecting higher profits from that area in future time periods) one would think (or hope) that fishermen would quickly "learn" that this is not the case. Overall, while I would tend to agree with the statement given on p. 16 that "oystermen are following where the seed and the shell is being placed in the summer and are oystering in those locations in the winter," one would think that oystermen, over a 16-year period, would learn from previous experience that such behavior does not result in increased profits.

Specific Comments:

1. p. 2 (and elsewhere in report): Why is the national oyster price considered in the analysis rather than the Maryland annual price? Given that Chesapeake oystermen are price takers (footnote 3), I would think the use of Chesapeake price would be more appropriate (particularly if the Chesapeake price differs significantly from the national price). Furthermore, it is unclear to me whether nominal or real dockside price is included in the analysis. From what I can ascertain, current price is used which might tend to yield some misleading results.
2. p. 8: In comparing means, it is somewhat misleading to state that "[t]he top six rows imply that seed and shell planting have a positive affect on harvest in comparison to areas not receiving planting in the same year." According to Wieland **(**Managing Oyster Harvests in Maryland's Chesapeake Bay), repletion activities in year t (by area) are largely based on previous harvests (by area). Hence, I would have been surprised if harvests in time period t in those areas with shell planting and seed in time periods t-1 and t-2 were not higher than in those areas receiving no shell plants. This does not imply, however, that the seed and shell plantings have any affect on harvest. As such, I do not agree that "[t]his preliminary analysis lends support to the DNR's repletion effort…. (p. 9)."
3. p. 10: While I agree that the fit of the regressions is "quite strong," I think it might be instructive to mention that much of the variation in the two regressions appears to be captured by a few "key" variables including effort in the harvest equation and effort_{t-1} in the effort equation.

Based upon this review, we can now consider the criteria noted above:

- Are the conclusions adequately supported by the evidence, analysis and argument?

See discussion. To some extent, I am concerned that possible misspecifications in the model lead to findings that there is a large negative return on investment with respect to shell plants. Modeling of a biological system, however, is complicated and I am not sure that additional investment in time would provide results that are more defensible.

- Are the uncertainties or incompleteness in the evidence explicitly recognized?

The author does explicitly recognize the uncertainties and incompleteness in the evidence and has reminded the reader of shortcomings in the analysis several times throughout the report.

- Are the data and analyses adequately described and well-suited to address the questions being investigated?

The investigator does a very good job at describing the data and analysis. The "general" method developed for analysis (i.e., the incomplete censored panel data) is ideally suited for the analysis though it is my opinion that some aggregation across areas might be in order. This opinion is based on the discussion at the top of p. 7 that “many oyster buyers may tend to note the origin of their oyster purchases with a bias toward local bars, due to a past policy of allocating repletion effort by harvest records.” Aggregation across contiguous/near-by areas may, to some extent, mitigate this issue. I do not know the system well enough, however, to even have a basic understanding as to whether such aggregation is feasible or desirable. According to a paper by Wieland (Managing Oyster Harvests in Maryland’s Chesapeake Bay), Cabraal (1978) included only four regions (Eastern Bay, Tangier Sound, the Bay mainstrain, and the Potomac and Patuxtent rivers) in his analysis. Given the issues regarding “bias” as noted above, I wonder if a similar aggregation might be appropriate. This level of aggregation would almost certainly reduce reporting bias and, hence, likely lead to results that are more defensible.

- Is the report fair and impartial in tone?

Yes

Reviewer #2 Comments:

Review notes on Herberich (2006)—Estimated return to harvest due to the Maryland Department of Natural Resources Repletion Activities 1990-2006

This is a well executed analysis. The statistical model is well-conceived and interpretation of the results is appropriate.

Areas that should be revised include:

1. The report should provide a rationale for using April-September mean dissolved oxygen (DO) rather than the minimum DO or the frequency that critically low DO events were observed.

2. The estimated negative effect of lagged (3-year) DO does not make biological sense. This coefficient and other coefficients on lagged DO should be constrained to be nonnegative. Nevertheless, it is unlikely that constraining these coefficients will lead to substantive changes in other estimated coefficients.

3. The harvest and effort time series should be evaluated for the presence of unit roots.

Minor editorial notes:

1. Kennedy (1981) is missing from the references.

Reviewer #3 Comments:

David Herberich, “Estimated Return to Harvest due to the Maryland Department of Natural Resources Repletion Activities 1990-2006”

This analysis estimates return to harvests from planning seed oyster and oyster shell to estimate the effect of repletion efforts undertaken by the Maryland Department of Natural Resources (DNR). Maryland has moved seed oysters from areas with abundant oyster stocks, and relocated these seed oysters on bars where stocks are sparse and natural spatfall is less frequent. Dredged shell has also been placed on harvest bars throughout Chesapeake bay to replace shell removed by harvesters.

The study uses a typical specification of the relationship between fishing effort and catch, whereby catch in a time period depends upon the level of effort and the stock of oysters. The work separates out the direct effect of repletion program when stocks change holding effort constant, and the indirect effect of the response of effort to changes in stock. The study points out that unless effort is controlled, the repletion efforts are likely to be unsuccessful, since any increase in oyster stocks due to repletion is likely to be offset by increases in fishing effort that bring stocks back down. Thus, repletion efforts must be considered within a large context of managing oyster fishing effort.

Equation (3). It is unclear why cost should depend upon stock for a given level of effort. Cost depends upon the level of fishing effort applied, but it is less obvious why cost will depend upon the stock given the level of fishing effort if effort is properly specified. It is also unclear of the purpose of the variable x, which is described as inputs. Presumably, effort is a single variable that is meant to represent all inputs. And note that the inputs are not included in the resulting equation 4 or 5. Nor does the author maximize profit with respect to the level of inputs, x. I suggest that x be dropped from the analysis, and the fishing effort be described as a summary measure of all inputs applied by watermen. Otherwise, catch will depend on effort, stock and other inputs (x).

Bottom of page 3, last partial paragraph, 4th sentence. It is not simple in this sort of dynamic analysis to use the State of Maryland controls as a proxy for the oyster population. Population is a stock variable that results from a time stream of restoration efforts, natural growth and mortality, including fishing mortality and therefore effort. An appropriate specification would consider all of these elements in constructing a proxy for oyster population. Since these are predetermined variables, it may be feasible to construct a proxy for population, and include the proxy in the econometric analysis. But a lot more thought is needed to be sure that you are doing this correctly. It seems to me that an age class model would work best here, so you capture the effects of growth, mortality and reproduction.

Wouldn’t it make more sense to construct current stock in terms of lags:

$$St = St-1 + G(St-1) + R(t-1)$$

where St is current stock, St-1 is lagged stock, G(St-1) is the growth rate of the stock, net of mortality and R(t-1) is the previous period's restocking efforts. Specifying a simple functional form for growth, one could solve for current stock as a function of the growth and mortality rate, and repletion efforts would enter with some sort of decay function.

This model is based on as assumption that different "cells" have no connections. Although adult oysters are stationary, oyster larvae are mobile; indeed oyster larvae can travel long distances. Thus, adult oyster populations in one area can affect future populations in other area, which is precisely why there are fears with respect to introduction of nonnative oysters. Thus, the econometric model needs to include populations outside of cell i.

Furthermore, it is not clear that the econometric model is of any use in guiding policy. Many of the coefficients are not statistically significant, and some are statistically significant but of the wrong sign or are otherwise inconsistent with expectations. For example, in Table A1, shell lagged one and two periods have positive and statistically significant effect on effort, but shell lagged three periods has a negative and statistically significant effect on effort. Shell has the same pattern in Table A2, but the results are less statistically significant.

A common sense understanding the process would suggest that there is a considerable time lag between dropping shell and the subsequent changes in oyster populations and catch. Dropping shell in the water potentially provides hard bottom upon which spat may successfully set. Logically, one would expect a significant time lag between dropping shell and the eventual development of oyster populations on the shell. One would expect virtually no effect whatsoever in the short term (e.g., one time lag). Perhaps some spat would begin to set on the shell within one year, but they would not yet have had time to reach catchable size.

One would expect that if the practice of dropping shell is successful, the reef might become more effective over time at providing habitat for larvae to set as the reef becomes larger and is comprised of more adult oysters, as opposed to just shell. So if dropping shell is effective, the productivity of the shell should *increase* over a significant time period, and certainly not reach a maximum in one year, then decrease and become negative within three years. This implies one would expect the coefficient on shell lagged one time period to be quantitatively small (probably zero) and statistically insignificant. The coefficients should increase with increasing lags, perhaps eventually reaching a maximum after a much longer time horizon (ten years or more?). This is not consistent with the results of the econometric analysis, which shows the largest effect of shell lagged one year, a smaller effect of shell lagged two years and a *negative* effect of shell lagged three years. This suggests that if you drop shell in one year, it has a large effect on harvest following year. But within this time frame spat would just be beginning to set on the shell, and the oysters would not yet have had time to reach marketable size. And certainly there would be no significant oyster reef established within one year. The results also suggest that shell dropped three years ago have a *negative* effect on harvest.

Three years would (optimistically) be a *minimum* amount of time for shell to have any significant impact on harvest. So the statistical results appear to be contrary to expectations given the system being analyzed.

Similarly, shell lagged one year has the largest effect on effort. For this to be true, harvesters would need to flock out in significant numbers to areas where shell was dropped the previous year, well before one might expect any effect at all on the population, and the oysters would not yet have had time to reach marketable size. The effect on effort lagged two years is smaller, and the effect of shell on effort lagged three years is negative. Once again, three years is probably a minimum amount of time before release of shell would likely have any effect on effort so these results are also contrary to expectations.

Other results are also counter intuitive. For example, DO lagged one or two years has a negative effect on harvest, but DO lagged three years has a positive effect on harvest. Is there any reason for DO to have a negative effect on populations? Why would the sign change on DO for different lags? One might argue that larvae are more sensitive to low oxygen conditions than adults. Therefore, on might expect a larger positive effect of oxygen with longer time lags. But the coefficient should not change sign and become negative.

The equation has catch per unit effort lagged by one period. So catch is explained by current effort and lagged catch per unit effort, among other variables. If one used current effort and *current* CPUE, the catch would be explained exactly by these two variables in the multiplicative form (Catch=Catch per Unit Effort * Effort). Thus, it seems to me to be extraordinarily dangerous to me to put in effort and lagged CPUE as explanatory variables. Any autocorrelation would be problematic for lagged CPUE. Also, if CPUE is relatively stable over time, one would almost certainly expect a misleading correlation between lagged CPUE and current harvest. It is unclear to me why CPUE enters this equation. Why should lagged CPUE determine current harvest after accounting for current effort as a separate explanatory variable?

Thus, the statistical results appear to be contrary to expectations given the system being analyzed, and thus I fear they are an artifact of data. I believe it would be a mistake to use these results to guide policy.

Department of Agricultural
and Resource Economics
University of Maryland
College Park, MD 20742

March 13th, 2008

James L. Anderson
Peer Review Group Chair
University of Rhode Island

Subject: Response to Peer Review Group Evaluation

Sir,

Please find attached my response to your review of *Estimated Return to Harvest due to the Maryland Department of Natural Resources Repletion Activities 1990-2006*. Please let me know of any questions, comments or concerns you may have. Thank you very much for your time.

Sincerely,

David Herberich, MS
PhD candidate
dherberich@arec.umd.edu

Thank you very much for your comments. I appreciate your time as this was a new endeavor for me. Below is a brief response to some of the issues that were raised. I have not been able to address all of the issues at length as my research interests have led me to other areas since writing this report. I made an effort in my report to be as open concerning the analysis as possible because I felt limited by the quality and depth of the data available. Further, I intended the report to closely follow the previous work of Cabraal and Wheaton (1981) and, by doing so, left much room for further research. Looking back on the report, given the time and funding I would re-run this analysis using a simultaneous system of equations for the amount of harvest and effort, given their close relationship, and attempt to use the prior year's shell and seed placement to instrument for the decision of where to place effort.

Responses are given in accordance to the Peer Review Group evaluation:

a) I am in agreement that differentiating catch by gear type would improve the analysis. Differentiation was not done at the time due to a concern that the number of observations in each separate gear type would limit the significance of the results.

b) First, when $seed_{t-4}$ was included, it did not impact the results as mentioned in footnote 34 of the appendix. Second, the specification used was intended to follow that of Cabraal and Wheaton (1981) as closely as possible. Once the results for the shorter lags of seed and shell were found significant, it was not possible to remove them from the estimation without a strong justification. Although I am in agreement that the shorter lags are not likely picking up a biological impact, there may be a behavioral impact as discussed in the paper.

   Although I am adverse to bias the results in this way, I have included a number of alternative specifications on the attached excel spread sheet (ad hoc). I did not include any that remove seed and spat lags of one and two years from the effort equation as I feel these measures do influence effort and thus act as additional instruments. Please do not take this as finalized work, see my opening statement for how I would like to address this situation in full had I time and funding. Further, including longer lags as mentioned in your review would require a much more in depth study of the intent of the repletion efforts over a significant amount of time and whether this intent is honored by local oystermen. Of note in the attached results are the coefficients in the harvest equation remain negative for shell t-3 and t-4, and though the coefficients remain positive for seed t-3 and t-4, they do not gain in significance.

c) The national price of oysters is used to proxy for the local price because the local price is likely endogenous with harvest. This is mentioned briefly in footnote 3.

d) I am not certain that I understand the concern of the statement. The results in Table III are meant as basic results, in the form of comparing means, and are meant to contrast the more in depth analysis. To that end, after suggesting that Table III lends support perhaps your concern lies in that I should have pointed out the contradiction that was about to be presented in the more complete analysis.

e) Bullet (b) should address this to some extent. I do think that, along with running the identification model that I suggest in the opening, additional water quality variables could be included.

f) I felt the need to include stock levels in the representation of costs after considering the dual problem of cost minimization. Intuitively considering the issue from a spatial standpoint may be helpful: allocated with a given level of effort, I assume that oystermen would seek out high stock areas first in order to take advantage of more easily obtainable oysters. This is the concept of seeking oysters at lowest marginal cost, influenced here by stock levels. Stock levels then act in a related yet unknown manner as a shift in technology would: influencing the cost curve and resulting minimum marginal cost point of optimal effort. That being said, I don't believe removing stock from the representation of the cost function would impact the implications of the model presented.

In terms of guiding policy, this report was intended to comment on the effectiveness of Maryland's repletion efforts. If you were to trust the results on a rudimentary level, the takeaway is that repletion efforts are questionable in terms of their impact on stock and interesting in terms of their impact behavior. I do not think that the contrarian results should render this paper useless, but rather, should point to deficiencies in data collection and spur further interest in finding a positive impact of the repletion effort, especially in light of the similarities between the findings of this research and those of the prior work by Cabraal and Wheaton (1981).

3) *Given this to be the case, why would industry effort be much more responsive to repletion activities?* – This is a great question, my impression from the results concurs with your intuition that repletion efforts do not impact harvest greatly from year to year and therefore should not result in a great increase of profits. I believe that oystermen are interested in repletion efforts to keep at least the same level of profits, least price adjusts. That being said, your conceptualization of the model may not quite be correct. If the harvest model was run without the effort model, and the same results were obtained, then your statement concerning holding effort constant would be accurate. However, because seed and shell appear to influence effort, it is necessary to account for this when determining the impact of seed or shell on harvest levels. When accounting for the impacts through effort, there are some positive results (as calculated on the bottom of page 12). Given the larger harvest levels, the repletion efforts may be positively influencing stock levels but this would be more certain through a direct impact on harvest which would rule out the possibility of repletion efforts inflating harvest through shifted effort, not increased stock.

Specific comments:

1) *Page 2:* The endogeneity of price was mentioned briefly above – local price would impact local harvest and vice versa, while the national price is assumed to not be influenced by local harvest. Real price is used when the returns are calculated.

2) *Page 8:* I may not be following this concern referencing Table II as it is not clear to me that areas treated with repletion in the two prior years should lead to larger harvests given our agreement that the past two years of seed and shell should not have a large biological impact on harvest levels. Further, I do not follow your conclusion based on Wieland's argument. For those areas which may or may not have had lower harvests in *t-1* and/or *t-2*, table II effectively looks at harvest in *t+2* (or similar) which receives seed or shell placement in year *t*. I don't believe that any conclusive follows from Wieland's argument concerning how the likelihood of being treated in year *t*, given harvest levels in *t-1* or *t-2*, impacts harvest in *t+2*. I suppose an argument could follow if a correlation between low harvest levels and setting of spat was known to exist. However, I am unaware of such a correlation.
3) *Page 10:* Acknowledged.

Again, I think you very much for this exchange. I am not certain in what direction this paper will go but your comments are greatly appreciated. I hope my responses have addressed some of your concerns but please let me know if otherwise.

Peer Review Group Evaluation

Wieland, Robert (Feb. 20, 2007)
Managing Oyster Harvests in Maryland's Chesapeake Bay

Prepared for:
The NOAA Chesapeake Bay Program Office
Nonnative Oyster Research Program

James L. Anderson, Peer Review Group Chair
University of Rhode Island

March 3, 2008

The Peer Review Group has completed its evaluation of the report:

- **Wieland, R. (Feb. 20, 2007). Managing Oyster Harvests in Maryland's Chesapeake Bay. Prepared for The NOAA Chesapeake Bay Program Office Non-Native Oyster Research Program.**

The Peer Review Group considered the validity of the research design, the quality of data collection procedures, the robustness of methods employed, the appropriateness of the methods for the hypotheses being tested, the extent to which the conclusions follow from the analysis, and the strengths and limitations of the research project.

The Peer Review Group came to relatively consistent conclusions regarding the report. Overall, the study was evaluated as useful and basically sound. In summary, it: a) is a good overview of the behavioral and economic implications of common-pool resource regimes as they can be expected to play-out under a "successful" introduction of *C. ariakensis;* b) does a good job in communicating the message that the attempts to revitalize oyster harvests are likely to be fruitless unless they address the common property problem; c) suggests that the results of Herberich (2006) are not reliable in terms of being contrary to the known structure of the problem (e.g., placing shell increases catch and effort in the following year).

3. Document: Managing Oyster Harvests in Maryland's Chesapeake Bay (Wieland)

Reviewer #1 Comments:

This document examines the complexities associated with attempting to effectively manage Maryland's Chesapeake Bay oyster fishery. While there are a number of specific points that are debatable at the margin (e.g., the definition of a common property resource) none of the points would change the conclusions that are arrived at in the report. One minor comment is that the tone of the report seems to imply that overfishing was, until recently (i.e., until MSX and Dermo decimated the stocks) the primary culprit with respect to the decline in stock. In fact, the stock may not have been overfished from a biological sense (i.e., fishing beyond MSY) but, instead, excessive effort resulted in a reduction in carrying capacity (see, for example, the Summary Section presented in pp.
19-20). Regardless, the fishery was overfished from an economic perspective (i.e, the dissipation of rents).

Specific Comments:

a. p. 16: I would use some caution in interpreting the information presented in Figure 7. Specifically, catch limits can certainly constrain the most efficient producers in the industry which would tend to reduce the ratio of CPUE to Catch Limits unless the reduction in CPUE among the efficient firms (i.e., those impacted by the regulation) is offset by an increase in CPUE among the less efficient firms.

b. p. 18: Can the information presented in Table 1 (i.e., OYH licenses) also be presented for the TFL license? This would present a more complete picture, in my opinion.

c. pp. 31-33 (Modeling Recent Repletion Performance); See my comments on the Herberich manuscript. I might mention, however, that the Caabral (1978) analysis indicated that shell plants had a larger impact than seed plants on commercial production. Herberich found the opposite. Can this be explained? Finally, and perhaps most relevant, return on investment is a relatively weak measure of whether the government should subsidize repletion/restoration activities. One could look at government expenditures as simply a transfer payment (to achieve some societal goal). The issue then becomes one of whether benefits to society from this transfer exceed the costs (in terms of resources being utilized). Benefits would include consumer surplus (probably limited given alternative oyster sources), producer surplus (probably insignificant in the long run due to the open access nature of the fishery), and benefits outside the market (potentially large but unknown). It is these that need to be considered in relation to costs which are likely to differ from expenditures.

d. pp. 37-41: I just want to say that I agree entirely with the author regarding the stated functional objective for mangers in Maryland's oyster fishery as well as the discussion in Section IV.C (Changes for sustainable benefits from the oyster fishery).

Based upon this review, we can now consider the criteria noted above:

- Are the conclusions adequately supported by the evidence, analysis and argument?

Yes. Based on sound economic theory and a review of management practices, the author was able to adequately support his conclusions. It is my belief that the conclusions are valid and defensible.

- Are the uncertainties or incompleteness in the evidence explicitly recognized?

Yes.

- Are the data and analyses adequately described and well-suited to address the questions being investigated?

Yes

- Is the report fair and impartial in tone?

Yes. While some of the discussion may appear critical of the current management policy, I believe that the information provided in the report provides a strong foundation for the discussion.

Reviewer #2 Comments:

Review notes on Wieland (2007)—Managing Oyster Harvests in Maryland's Chesapeake Bay

This paper provides a good overview of the behavioral and economic implications of common-pool resource regimes as they can be expected to play-out under a "successful" introduction of *C. ariakensis*. The conclusion that the common-pool regime reduces the likelihood of a "successful" introduction is well-supported in economic theory and case history in other fisheries. The conclusion that a successful introduction is unlikely to result in large increases in the profitability of harvesting operations is also well-supported in economic theory and case history in other fisheries. Based on the reasonableness of these two conclusions, it is also reasonable to accept the conclusion that:

> Without a change in the regulations addressing oyster bottom and the creation of credible enforcement of those regulations, it does not appear likely that oyster culture will be taken up on a commercial scale in Maryland.

The missing punch-line is that introduction of *C. ariakensis* cannot be justified in terms of the net present value of benefits to the commercial fishery unless the institutional structure of the fishery is modified to eliminate the common-pool dilemma and even then, it is not certain that a successful introduction will lead to significant commercial benefits under current market conditions. .

Sections of this report that should be revised include:

1. page 38—The discussion of oyster markets and prices should reflect concerns that were raised in our reviews of Lipton et al. (2006).
2. page 38—The discussion of harvest costs should reflect concerns that are raised in our review of Wieland (2006).
3. page 40—The assumption that production costs in aquaculture are uniformly greater that production costs in capture fisheries is supported in other aquaculture and capture fisheries. For example, salmon aquaculture operations have consistently produced salmon at prices that are below the operating costs of the capture fishery. While it is likely that the capture fishery could be reorganized to eliminate common-pool tomfoolery and thereby reduce operating costs, failure to rationalize the management of capture fisheries has resulted in ongoing financial distress and loss of market share. In the case of shrimp production, not only are costs in the capture fishery inflated by ill-designed management institutions, but there are practical constraints related to the harvesting technology that have limited the ability of capture fisheries to match the production costs of aquaculture. These examples do not ensure that oyster aquaculture would be economically viable in Maryland, but whether it is viable or not may not depend on the presence or absence of subsidies for the capture fishery.

Minor editorial notes:

1. page 13—The reference to International Pacific Halibut Commission, 1997, is missing.
2. page 32—The text refers to a row labeled "combined" in Table 4, but there is no such label in Table 4.
3.

Reviewer #3 Comments:

Managing Oyster Harvests in Maryland's Chesapeake Bay
Robert Wieland

This report provides a history of the State of Maryland's efforts to manage oyster stocks in Chesapeake bay. The report also reviews several earlier studies that use bioeconomic models to analyze the Maryland oyster fishery

The report provides a nice discussion of the management problems that arise due to utilization of a shared resource. It does a good job in communicating the message that that attempts to revitalize oyster harvests are likely to be fruitless unless they address the common property problem. Although it has been made repeatedly within a vast literature, it is essential that policymakers hear this message, and a repeating it in this context is essential.

Page 31-33. I agree with the discussion that the results of Herberich (2006) are not reliable in terms of being contrary to the known structure of the problem (e.g., placing shell increases catch and effort in the following year). The Herberich results are sufficiently unreliable that I believe they should not be used as guidance for policy. I fear that repeating the results here could suggest that the results are reliable, despite several caveat's to the contrary.

Pages 37-38. I agree that the state of Maryland has adopted objectives other than economic efficiency in managing oyster fisheries. However, it is questionable of whether these actions are in the public interest, or whether they result from rent seeking. One must be very careful in accepting "revealed preference" of public agencies at face value as merely an indication of multiple social objectives. Otherwise, all sorts of self-serving manipulations of government action can be justified—even bribery at the extreme.
Having said that, there is clearly an argument for public support for increasing oyster populations—but not necessarily to support harvest. Clearly, oysters populations provide a variety of ecosystem services, including filtering suspended solids from the water column, creation of structural habitat, possibly contributing to denitrification, etc. It is less obvious that society as a whole benefits from subsidizing oyster harvest, particularly in the long run.

Response to Peer Review Comments:
Robert Wieland

Managing Oyster Harvests in Maryland's Chesapeake Bay

General: With respect to any implication the paper makes that overfishing has been a problem to the present, I am not sure why or when overfishing stopped. Regardless of whether or not it was the cause of the stock collapse, retaining the same economic incentives (or, removing constraints to more efficient harvest) would lead one to expect the same sort of rent-dissipating behavior to obtain after a stock collapse as before. Given the sunk costs and seasonal effects, the rate of overfishing might be expected (in the short term) to accelerate in the face of diminished oyster stocks.

With respect to the distinction between MSY at historical levels of habitat versus the degraded levels that now obtain, this point is well taken. But, to the extent that the EIS is addressing an outcome based on historical levels of habitat, reductions from that MSY are what is being considered here.

Specific Points (reviewer 1):

(a) The reviewer is correct in his/her description of Figure 7 in the general case. However, considering CPUE efficiencies across gear types (a more significant distinction than across harvesters within a gear type) only the divers' ratio of CPUE to catch limits rises above 60 percent in any consistent way (for three years around the turn of the 20^{th} century). I have added text and a footnote noting this. However, these are oysters and they are not going anywhere so it does seem likely that what the more efficient harvester does not get, the less efficient harvester will take. Everyone knows where everyone else is working, generally. In the paper, these factors are not discussed in part because I did not break out the discussion by gear type. I stand by the contention that daily catch limits are not, in general, binding constraints on annual oyster harvesting effort.

(b) I do not have the data necessary to include TFLs in such a breakdown. There are over 2000 TFLs and only a portion of them pay the surcharge or work the fishery. Still, I agree that it would be more complete to include them.

(c) There are several reasons why seed planting may have looked more productive than shell planting under our analysis versus Cabraal's. Kimmel and Newell (2007, "The Influence of Climate Variation on Eastern Oyster Juvenile Abundance in the Chesapeake Bay") show that the relationship between the previous year's harvest (a proxy for stock) and recruitment changed in the mid-1980s (as disease became more widespread and virulent). They suggest that this link between harvest (stock size) and recruitment changed because of the large loss of spawners. The argument would be that spat set (recruitment) declined because spawners declined and this made shell less productive.

The second reason is pervasive across a lot of issues and has to do with the accuracy of the harvest data. There was some incentive up to the early 90s to report harvest as coming from your basin, because repletion activities were allocated according to productive success (as revealed by harvests). Even after that allocation strategy was adjusted, it is still necessary to id a bar when buy-tickets are filled out and there is anecdotal evidence (from my earlier harvest cost survey) that buy tickets are often not filled out when the harvest is conveyed to the buyer. My guess is that this is why shell placed in the current year generates harvest returns. The person completing the source bar section on the buy-ticket asks, where was DNR working this year? And reports that the oysters came from there. But this is just conjecture and beyond the scope of the study to confirm. The point is made in both Herberich's paper and this one that the harvest data are not very reliable.

With respect to "return on [public sector] investment" from repletion/restoration activities, I believe that this is useful focus for attention, though I agree that this may be complicated by external benefits and or costs. An interesting test of value within the industry would be what payment watermen would be willing to accept as compensation for the state ceasing its replenishment efforts.

Specific Points (reviewer 2):

1. I am not sure what concerns were raised with respect to oyster markets and prices in Lipton et al. However, my discussion of their findings is just to say that, especially given the shift to power dredging, the prices that they predict could still motivate harvesting at a rent dissipating level. It seems a bit academic, however, given the low likelihood of achieving such stock levels.

2. I have added text noting that these cost estimates are based on a very small sample of harvesters and that they could therefore be biased in an unknown direction.

3. I think there is a typo in this comment and that the reviewer meant to say that the assumption of uniformly higher costs in aquaculture is <u>not</u> supported by data from other fisheries. If so, the point is well taken. I have substituted "often" for "typically" in the text. The main point, that subsidies in the public oyster fishery undermine incentives to develop an aquaculture industry remains valid, I think. A secondary objective of this discussion was targeted at the level of analysis such as Paynter (2007, "A 10-Year Review of Maryland's Hatchery-based Oyster Restoration Program") where returns to the managed reserves is treated as only net of harvest costs, excluding public restoration costs.

Peer Review Group Evaluation

Wieland, Robert (Dec. 12, 2007)
Costs and Returns to Oyster Aquaculture in the Chesapeake Bay

Prepared for:
The NOAA Chesapeake Bay Program Office
Nonnative Oyster Research Program

James L. Anderson, Peer Review Group Chair
University of Rhode Island

March 3, 2008

The Peer Review Group has completed its evaluation of the report:

- **Wieland, R. (Dec. 12, 2007). Costs and Returns to Oyster Aquaculture in the Chesapeake Bay. Prepared for The NOAA Chesapeake Bay Program Office Non-Native Oyster Research Program.**

The Peer Review Group considered the validity of the research design, the quality of data collection procedures, the robustness of methods employed, the appropriateness of the methods for the hypotheses being tested, the extent to which the conclusions follow from the analysis, and the strengths and limitations of the research project.

The Peer Review Group came to relatively consistent conclusions regarding the report. Overall, the study was evaluated as useful and basically sound. The principal concerns regarding this study are that: a) the costs estimates for cage and float aquaculture are each based on very few interviews, and the study does not present information to suggest that the production costs of the interviewed firms are characteristic of the industry as a whole, and b) the cost estimates do not include capital costs associated with support vessels, docks, and other land-based facilities or for annual costs associated with dock fees, storage fees, or insurance. The panel also suggests that the report could be strengthened with some sensitivity analysis and some discussion on non-market returns. Several minor items were also identified.

1. Document: Cost and Returns to Oyster Aquaculture in the Chesapeake Bay (Wieland)

Reviewer #1 Comments:

General Comments:

1, I would suggest that some sensitivity analyses be conducted with respect to (a) the assumed number of oysters per bushel and (b) grow-out period. It appears that changes in either of these two factors may significantly alter results and the reader should be aware of the changes in net returns that would be forthcoming with respect to changes in assumptions relative to these two factors.

2. While I appreciate the discussion in the “Institutional and Regulatory Issues” section of the report, it is very short in nature and should probably be expanded. One major issue that could be more fully addressed reflects a more detailed explanation for the observed differences between Virginia and Maryland with respect to emphasis placed on aquaculture activities.

Specific Comments:

1. (On Bottom Cages): It might be insightful to note why hourly labor costs of $8.00 are assumed (Table 1). While the author appears to have some basis for other costs used in the budget (based upon a limited number of field observations), no explanation is provided for the assumption that labor will be $8.00. Similarly, an explanation of an assumed sales price of $35 per bushel (p. 6) is in order. According to other studies, the
2006 dockside price was only $30 per bushel and this figure was approximately one-third higher than in previous years (I assume the increase reflects the reduction in harvest in the Gulf Region after the 2005 hurricanes). The relatively high assumed price per bushel
(i.e., $35) may reflect more standardization in size and/or shape but some explanation is warranted (Note: footnote 7 may provide some indication but it is not clear).

2. p. 11 (Table 4): last column should read “Costs/harvest Returns” rather than “Harvest Returns/Costs.”

3. p. 16 (1st line): Clarification is needed for “a private return of 35percent and better is possible.” Is this return on investment? Is so, it sounds exceedingly high.

4.p. 18 (conclusion #2): The word “hinder” may be preferable to “conspire.”

Based upon this review, we can now consider the criteria noted above:

- Are the conclusions adequately supported by the evidence, analysis and argument?

In general, conclusions with respect to net returns associated with different systems (Onbottom Cages, Floats, and Oysters on bottom, spat-on shell) are adequately supported by evidence (though on a very limited number of existing operations with respect to Onbottom cages and Floats). While very limited in nature, the conclusions regarding the paucity of aquaculture activities in Maryland vis-à-vis Virginia due to institutional constraints also meritorious, in my opionion.

- Are the uncertainties or incompleteness in the evidence explicitly recognized?

Yes. The author recognizes that the budgets are based a very limited amount of information and explicitly warns investors to conduct some additional research before investing in aquaculture operations. The author also attempts to provide low and high estimates of costs based on alternative cost assumptions. I would encourage some additional sensitivity analysis (as discussed above) in order to more fully consider how changes in the assumed number of oysters per bushel and/or grow-out time alters the estimated net returns.

- Are the data and analyses adequately described and well-suited to address the questions being investigated?

Yes, though limited.

- Is the report fair and impartial in tone?

Yes.

Reviewer #2 Comments:

Review notes on Wieland (2007-12)—Costs and Returns to Oyster Aquaculture in the Chesapeake Bay

Despite shortcomings noted below, this paper provides a reasonable discussion of the costs and returns to oyster aquaculture in the Chesapeake Bay. The first of the principal shortcomings of this study are that the costs estimates for cage and float aquaculture are each based on interviews with only two producers and the study does not present information to suggest that the production costs of the interviewed firms are characteristic of the industry as a whole. The second principal shortcoming is that the cost estimates do not include capital costs associated with support vessels, docks, and other land-based facilities or for annual costs associated with dock fees, storage fees, or insurance. The report would have been strengthened by inclusion of a discussion of the expected variation in costs and returns over a range of mortality rates, e.g., 25-50%.

Sections of this report that should be revised include:

1. The report needs to provide (in an appendix) a short description of the characteristics of the 4 firms interviewed and a discussion of how these firms compare to other firms engaged in cage and float based aquaculture. How does their size and predilection for innovation compare with other firms?Etc.
2. The report should include estimates of the capital costs associated with support vessels, docks, and other land-based facilities and the annual costs associated with dock fees, storage fees, and insurance.
3. Page 9—the cost estimate for high-end (equip) triploid operations is lower than the cost estimate for low-end (equip) triploid operations. If this is a simple typo, it should be corrected. If this is a correct result of the enterprise budget model, it requires an explanation.

Minor editorial notes:

1. Page 8—there is a missing word, “for”, following the reference to footnote 12.
2. Page 9—Footnote 13, the reference to Lipton and Kirkley (1994) is not included in the references cited.
3. Page 11—Table 4. The header “harvest returns/costs” should be “total costs/harvest returns”
4. Page 11—Footnote 16. Wieland (2006) is not included in the references cited.
5. Page 11—Footnote 17 refers to Lenihan. Is this the same as Lenihan and Peterson (2004) included in the references cited?

Reviewer #3 Comments:

Costs and Returns to Oyster Aquaculture In the Chesapeake Bay
Robert Wieland

This study examines productivity and costs of confined oyster aquaculture operations in Virginia. The paper considers two confined aquaculture technologies: cages on bottom and floats. Contained aquaculture is shown to be economically viable at an assumed mortality rate of 33 percent, with an estimated rate of return ranging from 66 percent for floating operations to 30 percent for cages. The report also discusses several obstacles to contained oyster aquaculture in Maryland, and discusses ways in which these obstacles could be resolved.

While the work discussed in this paper is not particularly novel, the approach is solid and appears to contain very useful information that is essential in assessing a host of policy issues. I have no expertise to assess the specifics of the report. I have some concern, however, that the report is limited to interviews of four operators. Nevertheless, despite this limitation, if accurate and reasonably representative, this information could be extremely valuable in guiding policy in the Chesapeake.

There may be an error in the middle of pay 9, where the low end equipment cost for triploid oysters is higher than the high end cost ($62,628 vs. $59,659). Are these numbers reversed?

The authors might note that shellfish production in Chesapeake bay, including confined aquaculture operations, could provide highly valued ecological services. In particular, shellfish are filter feeders that remove suspended solids from the water column. If these activities are done at a large enough scale, this can improve water clarity and promote denitrification through deposition of organic material to the benthos. These services can contribute to improving habitat, for example, by improving conditions for sea grasses. Thus, successful shellfish operations can provide important social benefits, outside of benefits associated with extraction of shellfish for food. These non-market benefits afforded by oysters might justify public expenditures for re-establishing oyster populations in Chesapeake bay. At the same time, these external benefits might provide a rationale for restricting catch below some bio-economic maximum economic yield. That is, oyster harvest could imply a social cost due to the associated reduction in ecological services, this imply an external cost to harvest that policymakers should consider in establishing limits on catch. While these issues are not central to this paper, they might be worth mentioning.

Response to Peer Review Comments:
Robert Wieland

Costs and Returns to Oyster Aquaculture in the Chesapeake Bay

General: With respect to representativeness, the study's cost estimates for float aquaculture in Maryland is practically a census. There is one additional operator who started up in 2007 and another float operation in the coastal Bays, but the two enterprises assessed in this paper are the bulk of Maryland's contained aquaculture production. In Virginia, there are many but an unknown number more contained aquaculture operations than the two sampled. I add the following text to the introduction (pg 1) to address the concern:

These are developed from structured interviews with entrepreneurs who have produced oysters for market using the relevant technology. In Maryland, the two producers who cooperated were, by best available estimates, the entire active industry in Maryland's portion of the Chesapeake Bay at the time of the study. While there are more producers using contained aquaculture in Virginia than the two interviewed, it is unknown how many more. Contained aquaculture in the Chesapeake Bay is an emerging industry and, while the firms who cooperated for this study are thought to be representative with respect to technology and costs, it is less clear how representative they will be of longer term costs and technology as experience accrues and as innovation drives down costs.

With respect the capital costs of support vessels, docks and land-based facilities, I agree that this leaves the enterprise budgets incomplete with respect to total costs. It is a simplification, however, and I have made clear to the reader that I am excluding those costs for the contained aquaculture operations and including them for the on-bottom efforts.

The practical reason for ignoring boat and dock costs for contained aquaculture was that the type of boat used for current operations varied drastically among them. The floats in Maryland were being done in one case largely without a boat (but, with a boathouse where the floating upweller sat) and in the other case with a small skiff. In Virginia a newer Carolina Skiff and an old shallow draft work boat were being used. All of the contained aquacuture operations had a dock, however. And all but one had a boathouse.

Access to a private dock (meeting the needs of contained oyster aquaculture) is very difficult to price. Access to public docks is probably not adequate to the needs of oyster aquaculture (although investments that might make them adequate are not out of the question). Given the size of the industry and the infancy of the technology (in the Chesapeake Bay context) I am not confident that any estimate I might make for either a boat or a dock is going to be representative of what emerges from this start. Probably, having a boat and a dock or convenient access to the same will be binding constraints for early entrants. But there is no readily-accessible value to predict for those costs.

With respect to on-bottom aquaculture, the whole point to that is having men in boats working the bottom, and access, boat and time costs, are the only private costs born by those who work that fishery. Still, I agree that this might be made more explicit with particular regard to comparing returns between the two different activities and add the following subsection at the end of the Enterprise Budgets section:

D. Relative Costs and Returns

By any basic financial measure, the modeled returns of contained aquaculture exceed those estimated for on-bottom aquaculture by a large margin. However, as these are different processes subject to different institutional arrangements, costs are treated differently between the two sets and it is meaningful to consider these differences when comparing net returns. Moreover, because contained oyster aquaculture as practiced in the Chesapeake Bay is a technology still in its infancy, there is limited scope for using historical financial information for determining optimal production volumes at given levels of investment.

By choosing an end product of one million oysters for contained aquacultural production, the modeled estimate does not necessarily capture the efficiency price of production for contained aquaculture. It is likely, in fact, that such capital investments as upwellers, shaker tables and sorters are under-utilized with a one million oyster cohort. On the other hand, docks and on-shore facilities were not accounted in the estimate of contained aquaculture costs and these could be substantial. Some of the returns, net of the costs accounted in model would, in a more complete analysis, be applied to an annualized cost for such facilities.

The estimate of contained aquaculture also did not account boat costs, except for the gaff and winch as described. A boat was not essential for float aquaculture, but helpful. A boat was necessary for off-bottom cages, but at the modeled level of production, the time employment of a boat is not likely to be a major cost factor (as shown, below).

With respect to on-bottom aquaculture targeting shaft tong or diving harvest methods, a boat is an essential part of the harvest process. Moreover, because of the common property aspect of oysters in Maryland's managed reserves, harvest efficiency on any given site will range from high (at the start of the season) to low (as the site is worked-out). The method used for estimating harvesting costs on the four managed reserves used total effort, which captured this declining harvest productivity as a simple average. Clearly, with more efficient gear, these harvest costs could be reduced. But that is not the practice currently engaged.

With respect to production costs for on-bottom oysters, it is not clear that the full costs of production are captured in our use of a fixed price of $10,000 per million spat-on-shell oysters[1]. Moreover, that cost estimate does not capture major administrative, monitoring and management costs associated with the managed reserves. These costs are likely

[1] Horn Point Hatchery is both a production facility and a research facility and allocation of costs across these different uses is difficult.

substantial, and the current methodology, although new and evolving, is not subject to competitive pressures. These factors add to the difficulty of pricing those aspects of on-bottom production.

Bearing these caveats in mind, we can consider several alternative scenarios for both methods, using the cost factors described in the previous subsections. **Table 5** examines the costs of producing a larger cohort of oysters with off-bottom cages, using fixed equipment costs (e.g., one upweller, one shaker and one sorter). All other costs are assumed to increase by the same factor as the increase of the cohort so that to produce four times as many oysters requires four times as much labor, cages, starting stock, etc. As expected, holding some of the capital costs constant leads to not only higher returns but higher rates of return for all of the four starting stock/labor estimate scenarios.

Table 5: Off-Bottom Cages Estimated Returns from Larger Cohorts

	Two Million Cohort		Four Million Cohort	
	Cost	Net Return	Cost	Net Return
Triploid				
low-end labor/high end capital	$99,607	$75,393	$195,332	$154,668
high-end labor/low-end capital	$123,924	$51,076	$246,592	$103,408
Diploid				
low-end labor/high end capital	$110,309	$64,691	$215,443	$134,557
high-end labor/low-end capital	$130,732	$44,268	$259,789	$90,211

Table 5 maintains the mortality expectation of one third the starting stock. As calculated, total costs are not very sensitive to higher mortality factors, because mortality enters only as a need to start with a greater number of spat in order to ultimately fill an expected number of cages. Obviously, multiplying variable costs by factors greater than one increases total costs. Returns, however, increase faster. What is not known is where the practical limit is for annual throughput for the nursery and other equipment. Higher levels of throughput than 1 million are probable for commercial operations, but nursery equipment will be a likely bottleneck at some (unknown) point.

The net returns from Table 5 require one and one half and two years, respectively, for triploid and diploid starting stock. As noted, those estimates do not include costs such as land-based facilities and boats. A rough estimation of boat costs can be generated by factoring the number of boat-days required to move cages by an estimated boat-day cost. In the production scenario described for off-bottom cages, the total number of placements and retrievals for cages bearing the one million oysters is 1,665. If a boat can place and retrieve 25 cages per day, using the same workboat and fuel cost used for shaft tongers, 33 boat days at a cost of $55/day gives a total boat cost of $1,832 per one million oysters. This number can be factored by the number of oysters produced, such that 2 million oysters require $3,663 in boat costs and 4 million oysters imply $7,326.

For off-bottom cages at higher levels of throughput, there is clearly significant scope for paying facility, capital and management costs. Similar returns are generated by higher production volumes using floats (not shown). Given the negative estimated returns to on-bottom aquaculture, it may be more useful to ask, what would need to change to bring those negative returns positive?

Hatchery produced disease-free spat-on-shell is the largest visible portion of production costs for on-bottom aquaculture. Because of the large volume of material that is used for spat-on-shell, it does not seem likely that those production costs can be significantly reduced. However, a potentially larger production cost is mortality and unharvested oysters; which is only indirectly visible in the cost estimate. If triploid oysters were used for on-bottom aquaculture, an additional year of mortality could be avoided and a larger stock would be available for harvest. Similarly, if dredges were used to harvest those oysters, a larger share of the available stock could be brought to market. The gains from both of these practices could conceivably be great enough to generate positive returns for on-bottom aquaculture.

Alternatively, perhaps positive commercial returns are the incorrect metric for evaluating these alternatives. If external benefits (i.e., environmental benefits or the social utility generated by having men using tongs to harvest oysters) are included in the analysis, perhaps these would favor on-bottom aquaculture. Evaluating those possibilities is beyond the scope of the present paper.

[end of inserted text]

Specific Comments:

1. Labor cost estimates are explained in a (new) footnote on page 6. The $35 price is a standard market price in VA for aquacultured oysters and has to do with the larger number of aquacultured oysters that fit in a bushel and their shape and quality. Conversions between numbers and bushels of on-bottom oysters are done at a rate of 350 oysters to a bushel. Aquacultured oysters are assumed to be 400 to a bushel.

2. Fixed.

3. These returns are net of estimated costs (as discussed), and do not include some other costs that we know are there but which we find difficult to estimate (i.e., facility rent, boats, management, capital). They are not net returns on investment in any complete sense.

Reviewer #2:

Comment 1: See my inserted text discussed in the first paragraph of this response.

Comment 2: I hope that I have addressed these concerns in the added subsection under the discussion of relative returns under enterprise budgets.

Comment 3: I have revised my terminology to describe these scenarios more completely as: high-end labor/low-end equipment, and low-end labor/high-end equipment. In the case noted by the reviewer, I had in fact reversed the scenarios and their cost estimates, but in the diploid case, the low-end labor scenario is more costly than the high-end labor scenario because of the difference in equipment costs.

Uncommented: I have made the major change of revising the name of the cages from "on-bottom cages" to "off-bottom cages". Several other folks have found my earlier terminology confusing.

Minor editorial comments are addressed – thank you.

Peer Review Group Evaluation

Wieland, R. and Kasperski, S. (July 21, 2007)
Estimating Net Present Value in the Northern Chesapeake Bay Oyster Fishery

The NOAA Chesapeake Bay Program Office
Nonnative Oyster Research Program

James L. Anderson
Peer Review Group Chair
University of Rhode Island

October 9, 2008

The Peer Review Group has completed its evaluation of the report: "Estimating Net Present Value in the Northern Chesapeake Bay Oyster Fishery" by R. Wieland and S. Kasperski. The reviewers have identified several areas where the report could be improved. However, the report provides useful information for policy discussions.

Addressing these issues (identified below) would provide additional credibility to the report. Given that the model discussed in the paper could be used extensively in the management process, we encourage the authors to consider these comments.

Reviewer I

As clearly stated in the report, the analysis is conducted with very limited and, at times, tenuous information (both data and results associated with other studies). Any use of the current model in the policy arena should recognize these limitations and weight the results accordingly.

While the current study is based on limited information, in general the analysis appears to be economically sound and relatively thorough. I am unable to state whether the analysis/results are credible, however, due to some confusion/concerns I have with the model. These are briefly outlined below.

Concerns:

1. The carrying capacity of market sized oysters in the upper bay (i.e., Maryland) is given as 5.089 billion. This estimate is based on Jordan and Coakley (2004). As the authors recognize, the carrying capacity is highly dependent on substrate and will generally increase as the amount of substrate increases. It is unclear to me whether the 5.089 billion estimate is associated with the current amount of substrate or that amount available at some previous point in time. Since the carrying capacity (k) is an important determinant in both the growth equation and the cost equation, it would be useful if the authors would provide some elaboration with respect to carrying capacity. Specifically, I have a couple of concerns. First, if the estimate is related to the current period, what evidence is there that it could support any large increase in oyster population (e.g.,, that which is predicted under a harvesting moratorium). Along a similar line of reasoning, is there any evidence that the amount of suitable substrate will significantly increase as the population of oysters increases (I know there is some discussion in the paper about attachment of cultch to live oysters but will this be sufficient)? A second concern relates to the fact that carrying capacity relates only to market sized oysters. One would expect that the carrying capacity of market sized oysters is to some extent a function of the quantity of non-market sized oysters. I would imagine that as the volume of market sized oysters increases, one would anticipate a concomitant increase in the volume of non-market sized oysters. Is this considered in the model? In my opinion, results would be much more

credible if the authors could clearly demonstrate the linkage between carrying capacity and population and/or *vice versa*.

2. The report states that the truncation of the distribution to be no greater than the current real price of $30 per bushel "allows prices to vary year to year randomly, and also allows the price to decrease with increases in supply (p. 16)." I understand how prices are allowed to vary randomly (with an upper value of $30 per bushel, but I do not see how the model will allow price to decrease with increases in supply. Perhaps this can be better explained by the authors. This issue is very important given that the model predicts that annual harvests under a more reasonable management regime (i.e., the optimization model) can be ten times the current harvest (i.e., Figure 2).

3. The cost estimate is somewhat disturbing to me. Specifically, while the left hand cost equation (p. 17) appears reasonable, I am somewhat skeptical that the assumption that "operators expect to harvest 50 bushels per day over a season of 100 days" is reasonable. I note in footnote 19 that this figure is derived from a New Jersey Delaware Bay study and wonder if it is applicable to Maryland. Perhaps the authors can elaborate.

4. A constant catchability coefficient (q) is assumed in the model (p. 16). Can the authors provide some evidence that this assumption is reasonable for this industry?

5. Finally, I applaud the authors for examining the sensitivity of the model results to the intrinsic growth rate (r). I think it might be useful if sensitivity analyses are also conducted with respect to some of the other key parameters. Key parameters would be the carrying capacity and the discount rate. With respect to the discount rate, it is my understanding that federal guidelines now suggest the use of two discount rates when the analysis is conducted in real terms (as is the situation in this study). The suggested rates are 2% and 7%. While the 7% might appear high, it might be realistic in this situation given the large amount of risk/uncertainty.

Reviewer II

Here are some concerns:

1. The assumed linear relationship between stock growth and harvests, (section 2.3): The assumed linearity almost certainly overstates the magnitude of stock increases in the absence of harvesting. Moreover, the author fails to provide any empirical evidence to suggest that such a relationship exists, other than a belated acknowledgment that no such

relationship has been evident in Delaware where a moratorium was in place for a short period.

2. The assumption of positive net revenues in what has been and likely will continue to be a regulated open access fishery. Without this assumption, there is no commercial fishery value to current or future harvests.

3. The assumption that price is independent of quantity landed at all possible harvest levels.

4. The foundation for estimates of the assumed value of nitrogen fixation is not substantiated.

Reviewer III

The paper explores the net present value of oyster stocks, including both commercial and environmental benefits under different management scenarios. The paper uses an age class model and a specification for environmental benefits from denitrification. The specification for denitrification is *ad hoc*, but monetary values are calibrated to the costs of removing nutrients by wastewater treatment facilities, based on research by Newell et al 2004. The specification is reflects diminishing marginal nutrient removal per oyster. The model also does not consider the full suite of ecological services of oysters, that includes habitat, possible protection of shoreline erosion, carbon sequestration, and removal of suspected solids thereby potentially enhancing eelgrass growth. Thus, the model is better thought of as an illustrative application, rather than an actual policy analysis.

The paper looks at sensitivity of the results to alternative intrinsic growth rates and examines the implications of a moratorium on catch as a resource enhancement program.

One problem with the analysis of moratorium is the possibility of irreversibilities not being considered in the model. For example, if there is a total moratorium on catch for long time periods. For example, having no catch for several decades would result in a total loss of infrastructure, including both human and physical capital. It is not likely that this industry would simply bounce back following a moratorium 20 to 50 years. Furthermore, the model is subject to the usual difficulties of forecasting structural changes 50 years into the future. For example, climate change effects associated with increase water temperature and sea level rise could have considerable effects on both the biological and economic models, such that entirely new sets of parameters would be needed. As indicated above, problem is not specific to this work, but is equally applicable to any work that attempts to forecast decades into the future.

Peer Review Group Evaluation

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Addressing these issues (identified below) would provide additional credibility to the report. Given that the model discussed in the paper could be used extensively in the management process, we encourage the authors to consider these comments.

Reviewer I

As clearly stated in the report, the analysis is conducted with very limited and, at times, tenuous information (both data and results associated with other studies). Any use of the current model in the policy arena should recognize these limitations and weight the results accordingly.

We have endeavored to be explicit with respect to the limitations of our data and we make the point in the introduction that our estimated growth rate awaits a more rigorous estimate by the biologists and demographers. However, we stand by the optimization model developed to utilize this data. If it is at all consequential, the model should provide additional incentive to acquire a widely-accepted growth rate for eastern oysters under current conditions of scarcity (scarce stock, scarce substrate, scarce oxygen, etc) and disease. It is difficult to conceive any growth rate that would both predict positive NPV of oyster stocks under current policy and justify public investments in the resource but not show a more seriously effort-limiting management policy to be rationally preferred in terms of NPV.

While the current study is based on limited information, in general the analysis appears to be economically sound and relatively thorough. I am unable to state whether the analysis/results are credible, however, due to some confusion/concerns I have with the model. These are briefly outlined below.

Concerns:

1. The carrying capacity of market sized oysters in the upper bay (i.e., Maryland) is given as 5.089 billion. This estimate is based on Jordan and Coakley (2004). As the authors recognize, the carrying capacity is highly dependent on substrate and will generally increase as the amount of substrate increases. It is unclear to me whether the 5.089 billion estimate is associated with the current amount of substrate or that amount available at some previous point in time. Since the carrying capacity (k) is an important determinant in both the growth equation and the cost equation, it would be useful if the authors would provide some elaboration with respect to carrying capacity. Specifically, I have a couple of concerns. First, if the estimate is related to the current period, what evidence is there that it could support any large increase in oyster population (e.g.,, that which is predicted under a harvesting moratorium).

Jordan and Coakley arrive at the 5089 million estimate of the carrying capacity by multiplying the charted areas in the Bay which are suitable for oyster habitat times their presumed density of market sized oysters at carrying capacity. The Jordan and Coakley 2004 estimate of the suitable habitat area comes from Maryland Bay Bottom Survey, which indicated that only 10% of nominal oyster habitat actually supported oyster populations (Smith et. al. 2001)[1]. To get their carrying capacity estimate, they assumed that 10% of cultch areas supported 10 market sized oysters per m^2, and 10% of sand and cultch and mud and cultch areas supported 3 market sized oysters per m^2. We consider this a fairly conservative estimate of mean oyster densities at carrying capacity. Additionally, we have included a sensitivity test of our results to a reduction in carrying capacity, and it has little effect on the optimal harvest rate. It does affect the net present value estimates as the NPV declines with a smaller carrying capacity because density dependence sets in with a lower stock, and the stock cannot become as large – leading to lower harvests after the stock has rebounded. See Section 3.7 (included below) for the complete accounting of this sensitivity test.

Along a similar line of reasoning, is there any evidence that the amount of suitable substrate will significantly increase as the population of oysters increases (I know there is some discussion in the paper about attachment of cultch to live oysters but will this be sufficient)?

Given the derivation for the carrying capacity (above), we do not need to assume that the suitable habitat area increases over this period. The assumption that we make here is that the charted areas which were deemed suitable for habitat in Jordan and Coakley in 2004 (from the MBBS in 2001) are still suitable for oyster habitat in 2008. We assume the carrying capacity of 5089 million oysters is fixed for all years, and does not vary according to the current population of oysters. We do not assume that as the population of oysters increases, additional areas which were not deemed suitable in 2004 are suitable habitat in the future thereby increasing the carrying capacity of the MD portion of the bay. Similarly, we do not assume that in the early years of the model where stock have yet to increase substantially, that the suitable areas without oysters become silted over and become unsuitable reducing the carrying capacity of the MD portion of the bay. See Section 3.7 below for our sensitivity test of the effect of reducing the carrying capacity.

A second concern relates to the fact that carrying capacity relates only to market sized oysters. One would expect that the carrying capacity of market sized

[1] Smith, G.F., K.N. Greenhawk, and D.G. Bruce, et al. 2001. A digital representation of the Maryland oyster habitat and associated bottom types in the Chesapeake Bay. *Journal of Shellfish Research* 20(1), p. 197-206.

oysters is to some extent a function of the quantity of non-market sized oysters. I would imagine that as the volume of market sized oysters increases, one would anticipate a concomitant increase in the volume of non-market sized oysters. Is this considered in the model? In my opinion, results would be much more credible if the authors could clearly demonstrate the linkage between carrying capacity and population and/or *vice versa*.

We addressed the concerns about the carrying capacity in relation to current oyster population levels above. The role of oyster recruitment and the role of non-market sized oysters in the model deserves some consideration. There is very little visible correlation between oyster stock size and recruitment in the Chesapeake Bay. However, all the demographic models to date assume a positive relationship between numbers of oysters (spawners) and stock growth. The difference between our simple accounting model and the demographic model is that the demographic model requires estimates for growth and mortality on top of their recruitment (spat-set) estimates. They then have to predict what the weather is going to be in the future. We simply use population estimates paired with harvest rates over the past 14 years to develop our estimate of oyster stocks' inherent growth rate. We explicitly assume that repletion activities and the weather will be similar over our time horizon. It is a decidedly blunt instrument and it would be great to have a longer time series, but these are the best data available to us.

2. The report states that the truncation of the distribution to be no greater than the current real price of $30 per bushel "allows prices to vary year to year randomly, and also allows the price to decrease with increases in supply (p. 16)." I understand how prices are allowed to vary randomly (with an upper value of $30 per bushel, but I do not see how the model will allow price to decrease with increases in supply. Perhaps this can be better explained by the authors.

   The model does truncate the distribution of the price at $30/bushel, but there is no inverse demand function for oysters in the model, so the price could randomly decrease with increases in supply, or it could increase (although not above $30/bushel) with increases in supply. This was misleading, and has been fixed in the revised draft.

   This issue is very important given that the model predicts that annual harvests under a more reasonable management regime (i.e., the optimization model) can be ten times the current harvest (i.e., Figure 2).

   While harvests may increase ten-fold under this management scenario, current harvests are so low that even this ten-fold increase will likely have little effect on the price that watermen receive for their oysters. The average harvest between 2005 and 2007 harvest was 39.17 million oysters, which at 300 oysters per bushel is only 130,666 bushels. In the no moratorium scenario (figure 2), we predict oyster harvests in 70 years to be around 160 million oysters which equates to .533 million bushels of oysters. In 2003, US production of oysters was 2,800 million bushels (Lipton, Kirkley, and Murray, 2006)[2]. The largest single year harvest that we predict in the optimization model is only 1.333 million bushels which occurs in 5057, the first year after a 50 year moratorium. Additionally, regional MD and VA processed oyster production comes from oysters harvested in other states, with the majority coming from the Gulf of Mexico (Murray, 2002; Lipton, 2008)[3]. Given that the substantial production of oysters from the Gulf act as a near perfect substitute for oysters from the Chesapeake, so we think we can safely ignore any price effects from our model's small predicted increase in production relative to total U.S. production of oysters.

3. The cost estimate is somewhat disturbing to me. Specifically, while the left hand cost equation (p. 17) appears reasonable, I am somewhat skeptical that the assumption that "operators expect to harvest 50 bushels per day over a season of 100 days" is reasonable. I note in footnote 19 that this figure is derived from a New Jersey Delaware Bay study and wonder if it is applicable to Maryland. Perhaps the authors can elaborate.

    We use a more efficient harvest rate than is currently found in the Bay because we assume a preference for efficiency among managers and harvesters. Over the past ten years inefficient tongers have shifted to dredging because they cannot make a living tonging and because dredging has recently become permitted. If stocks were higher, 50 bushels per day is not an unreasonable catch expectation, as shown in the Delaware Bay data. The boats are basically the same and it is the same oyster. Of course there would need to be fewer boats. An important factor in the currently low daily catch rates in Maryland is the tremendous surplus capacity in the fishery. We assume that both managers and harvesters would like to fix that. We will make this assumption more explicit.

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[2] Lipton, D., J. Kirkley, and T. Murray (2006). A Background Economic Analysis for the Programmatic Environmental Impact Statement Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*. Final Report to the Maryland Department of Natural Resources.

[3] Murray, T.J. (2002). Making a case for the oysters: Putting ecological benefits into economic terms. Virginia Marine Resource Bulletin 34(2):7-10.
Lipton, Douglas (2008). Final Draft Economic Analysis for Oyster Restoration Alternatives. Appendix D1: Economic Analysis of the EIS Regarding the Restoration of the Chesapeake Bay Oyster Fishery Using the Non-Native Oyster, *Crassostrea ariakensis*.

4. A constant catchability coefficient (q) is assumed in the model (p. 16). Can the authors provide some evidence that this assumption is reasonable for this industry?

   The main argument for a constant catchability coefficient is that oysters are a sedentary species, and the only thing stopping all of them from being harvested in a given year is watermen finding them. Each waterman should be able to cover some fixed area each day/year. Assuming that the watermen are using dredges, and there is no daily catch limit, as long as they can cover their operating costs, they should dredge the same amount of area on any given day. If oysters are spread randomly out over a given space, and since the area a waterman can dredge is limited with some upper bound on the amount of time they can spend on the water, we assume that they will be able to find some fairly constant (we assume completely constant) percentage of the oysters in that space. Additionally, this assumption allows us to get just one optimal harvest rate per moratorium year, which then allows us to compare harvest rates across different moratorium lengths and parameter scenarios.

5. Finally, I applaud the authors for examining the sensitivity of the model results to the intrinsic growth rate (r). I think it might be useful if sensitivity analyses are also conducted with respect to some of the other key parameters. Key parameters would be the carrying capacity and the discount rate. With respect to the discount rate, it is my understanding that federal guidelines now suggest the use of two discount rates when the analysis is conducted in real terms (as is the situation in this study). The suggested rates are 2% and 7%. While the 7% might appear high, it might be realistic in this situation given the large amount of risk/uncertainty.

   We heeded this advice. Section 3.7 addresses a decrease in the carrying capacity, and Section 3.8 addresses the sensitivity of the results to discount rates of 2% and 7%. We have included both sections below. Quickly, the discount rate does have a mild impact on harvest rates, and a substantial impact on the NPV estimates, but the general conclusions of the model remain the same – a harvest moratorium of decent length will increase the NPV of this fishery and harvest rates should be much lower than current rates.

Reviewer II

Here are some concerns:

1. The assumed linear relationship between stock growth and harvests, (section 2.3): The assumed linearity almost certainly overstates the magnitude of stock increases in the

absence of harvesting. Moreover, the author fails to provide any empirical evidence to suggest that such a relationship exists, other than a belated acknowledgment that no such relationship has been evident in Delaware where a moratorium was in place for a short period.

In response to overstating the magnitude of stock increases in the absence of harvesting, with the stock at such a depleted state, the growth might in fact be linear from this starting point. The linear relationship between stocks and growth shown in Table 3 is used only to derive an intrinsic growth rate of the stock (to be used in the logistic growth function in section 3). This relationship is likely linear near the origin (which approximates current conditions). Therefore, to the extent that the data compiled by MDNR is credible, and to the extent that oyster population growth follows a logistic pattern, our estimate of the intrinsic growth rate is an underestimate since we are fitting the linear relationship at a stock that is (hopefully) greater than the minimum viable population. We appreciate the concern relating to our calculation of the intrinsic growth rate, which is precisely why we present several sensitivity tests around our estimate. As all of the tested growth rates generally lead to the same model conclusions, we feel the model is fairly robust regardless of any errors in our calculation of the intrinsic growth rate.

With respect to the reviewer's interpretation of our position on the effectiveness of New Jersey's oyster management, it seems clear to us that limiting harvest through annual harvest allotments has been co-incident with reasonably stable oyster stocks, even without the substantial shell repletion and seeding efforts that have been employed in Maryland's Chesapeake Bay and even without the great volume of suitable oyster bottom in areas with different average salinity levels that exists in Maryland's Chesapeake Bay. If the reviewer is arguing that New Jersey's harvest restrictions have had no effect on oyster stocks over time, he should support that argument. The references cited (Powell and others) support our interpretation of the Delaware Bay oyster fishery data.

2. The assumption of positive net revenues in what has been and likely will continue to be a regulated open access fishery. Without this assumption, there is no commercial fishery value to current or future harvests.

We assume that positive net revenues are about finished in the Chesapeake Bay oyster fishery. That is why a continuation of current policy has such a low NPV. Under a moratorium or otherwise more effectively managed fishery, we assume that harvesters will utilize their resources in such a way as to make money from their allocation at some point in the future. But it is a little misleading to refer to the Maryland's oyster fishery as a regulated open-access fishery. None of all the regulations bearing on the fishery singly or in combination provides a binding constraint to total harvests, except to the extent that gear restrictions impose economic inefficiencies. Economic returns to harvest effort limit harvests, which is for all intents and purposes the same outcome delivered by an open-access fishery.

3. The assumption that price is independent of quantity landed at all possible harvest levels.
We do not assume that price is independent of quantity marketed at all possible harvest levels, but rather at all relevant harvest levels predicted by our model. Our estimated inherent stock growth rate, slowed by stochastic factors (occasional high mortality events) in the optimization model, never leads to harvest rates that are likely to have a large effect on national oyster production. See our response to reviewer 1.

4. The foundation for estimates of the assumed value of nitrogen fixation is not substantiated.

The reference to Newell and others is our substantiation of this value. The biology of this ecosystem service is the focus of that paper (actually a book chapter). As noted in the acknowledgements to Newell and others, the first author of the current paper provided background material for their valuation of oysters' denitrification services. This valuation was based on economic findings derived from a Bay-wide Use Attainability Analysis. Given a total estimated nutrient reduction requirement for the Bay (to obtain mandated uses) and an estimate of the cost of achieving that reduction, the average cost worked out to about $24/kg. This was treated as the nutrient reduction value of a kilogram of denitrification services in Newell and others. We will endeavor to make this more explicit in the paper.

Reviewer III

The paper explores the net present value of oyster stocks, including both commercial and environmental benefits under different management scenarios. The paper uses an age class model and a specification for environmental benefits from denitrification. The specification for denitrification is *ad hoc*, but monetary values are calibrated to the costs of removing nutrients by wastewater treatment facilities, based on research by Newell et al 2004. The specification is reflects diminishing marginal nutrient removal per oyster. The model also does not consider the full suite of ecological services of oysters, that includes habitat, possible protection of shoreline erosion, carbon sequestration, and removal of suspected solids thereby potentially enhancing eelgrass growth. Thus, the model is better thought of as an illustrative application, rather than an actual policy analysis.

We acknowledge in the paper that our measure of environmental value is partial. Other than (Hicks, Haab, and Lipton, 2004), there is little ecosystem services valuation research focusing on oyster stocks in the Chesapeake Bay. Newell provided a dramatic estimate for oyster's filtering services, but this has never been assessed with respect to its value to humans. We believe that our discussion of the value of oysters' denitrification services provides a useful example of the effect of ecosystem service values on decisions about harvests and NPV, but we do not suggest that we have captured a complete measure of oysters' ecosystem service value. We will endeavor to make this more explicit. Monetary values are not calibrated by point source load reduction costs but by the requirement that nutrients in the water-column be reduced to more serviceable levels and estimates of how much that might cost.

The paper looks at sensitivity of the results to alternative intrinsic growth rates and examines the implications of a moratorium on catch as a resource enhancement program.

One problem with the analysis of moratorium is the possibility of irreversibilities not being considered in the model. For example, if there is a total moratorium on catch for long time periods. For example, having no catch for several decades would result in a total loss of infrastructure, including both human and physical capital.

There will be significant losses for investors who are currently vested in harvesting and packing oysters. Although to the extent that the processors in the region currently receive the majority of their oysters from states outside of MD and VA[4], the effects of the moratorium on this industry will be mitigated. But it is not clear why the returns to a well managed fishery would not generate additional investment if oyster stocks rebound, no matter how far that is into the future. Oysters are a valuable resource, easily captured and fairly easy to process and market. It is not clear where the irreversibilities are in this.

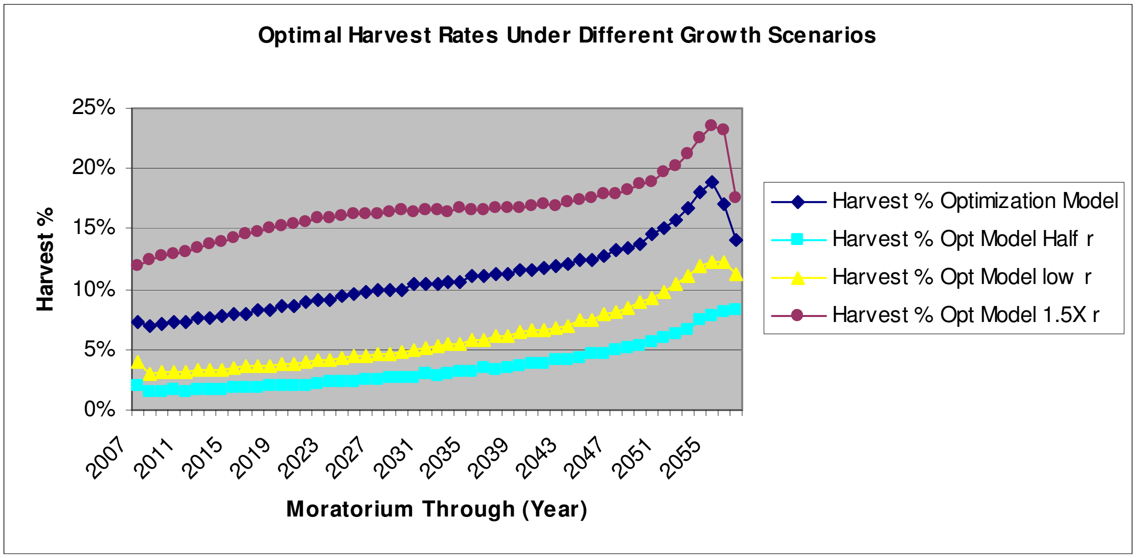
It is not likely that this industry would simply bounce back following a moratorium 20 to 50 years. Furthermore, the model is subject to the usual difficulties of forecasting structural changes 50 years into the future. For example, climate change effects associated with increase water temperature and sea level rise could have considerable effects on both the biological and economic models, such that entirely new sets of parameters would be needed. As indicated above, problem is not specific to this work, but is equally applicable to any work that attempts to forecast decades into the future.

The reviewer is correct that basing our analysis on what happened over the past 14 years means that the farther we get from the present, the less accurate our estimates probably will be. However, we believe the long-term optimization model that is developed in our paper to be a reasonable basis for choosing among policies, if maximum NPV is the management goal.

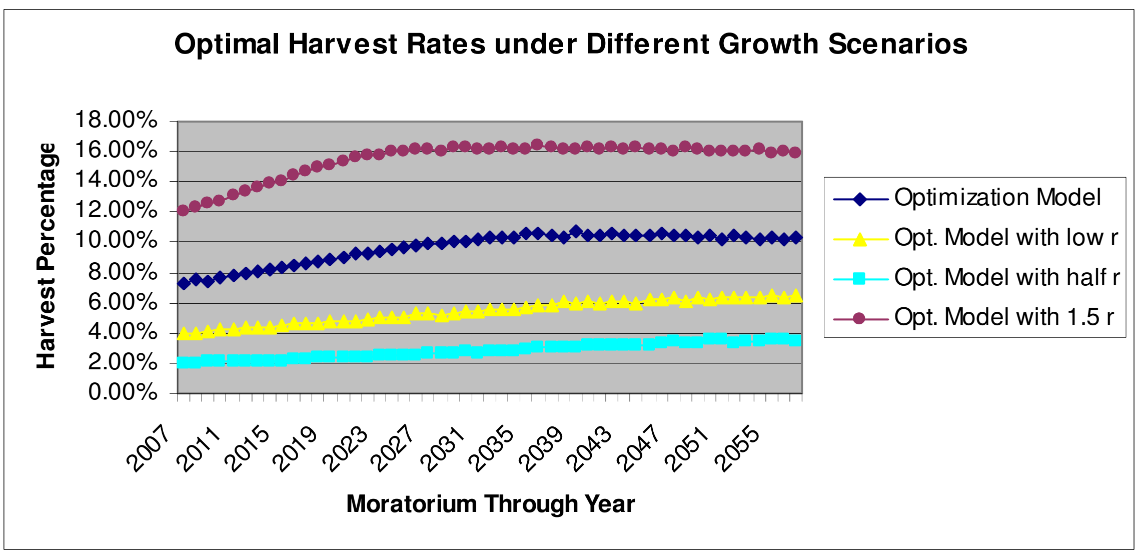
[4] See Murray 2002 and Lipton 2008, citation provided in the revised draft.

Revisions to the Draft

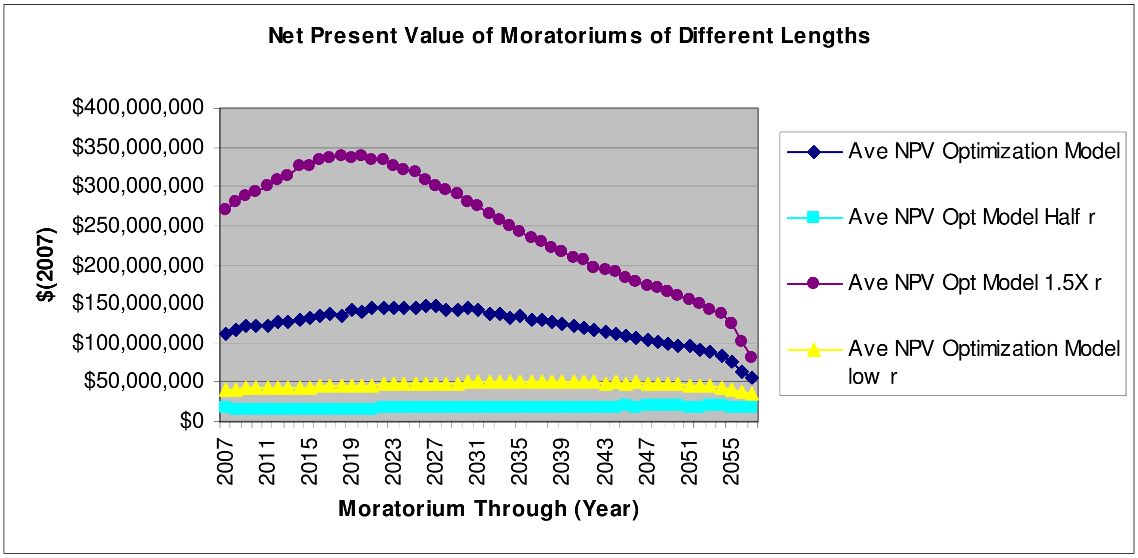
There was a small error in the optimization model which determined the steady state stock levels. Rather than take the previous stock level, they were taking the stock level from 50 years before (2057) which was a remnant from when the model was only run for 50 years. As a result, the harvest rates were erratic around moratoriums lasting until nearly 2057. This is most easily referenced in regards to Figure 6, which includes all of the model runs which were subject to this error. Here is the old Figure 6:



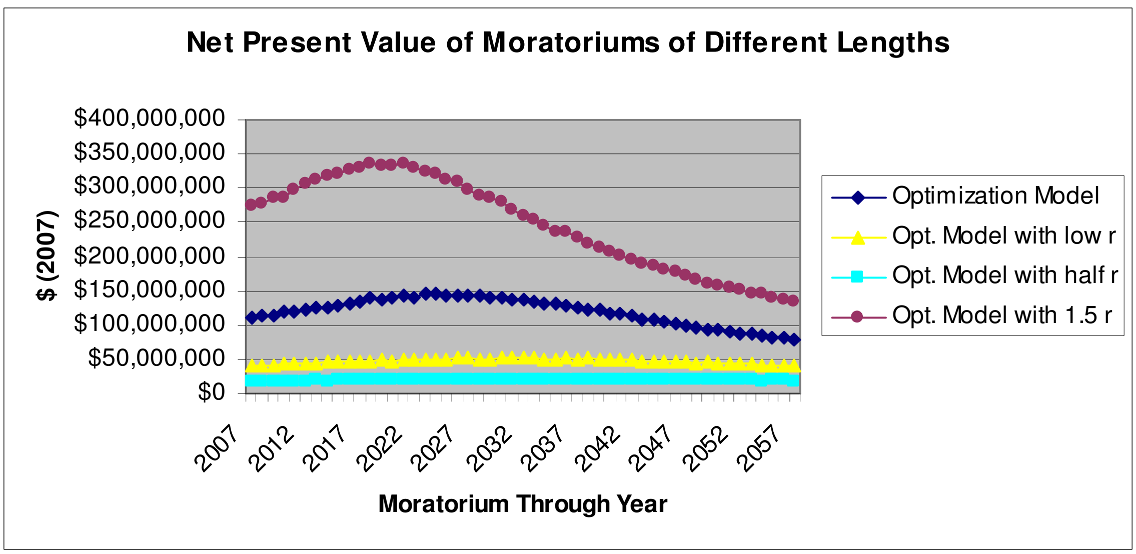
Notice the large increase in harvest rate in moratoriums that end before 2057 and the sharp drop as we get closer to the 2057 moratorium. There was not really an explanation for this phenomenon, and gladly because it was a result of an error in our code. The new Figure 6 looks like:



Since the models were rerun (and the original optimization model needed to be rerun), Figure 7 changes as well. The old Figure 7 was



And the new Figure 7 is



There were very few changes in the NPV estimates aside from the last moratorium scenarios that we ran.

However, since the original optimization model had to be rerun, all of the figures in the study had to be replaced, but with very little change in interpretation as shown by figure 7.

The most significant revisions came as a result of the suggestion of reviewer 1. We reran the model with a lower estimate of the carrying capacity, as well as separate runs for a lower and a higher discount rate. Those different sensitivity tests are included below in sections 3.7 and 3.8 respectively. While we were rerunning the model, it seemed logical to include a scenario in which we combined a number of the bad scenarios together and see how the optimal harvests changed. We called this the pessimistic scenario, and it runs the model using the low growth estimate, the low carrying capacity estimate, and the

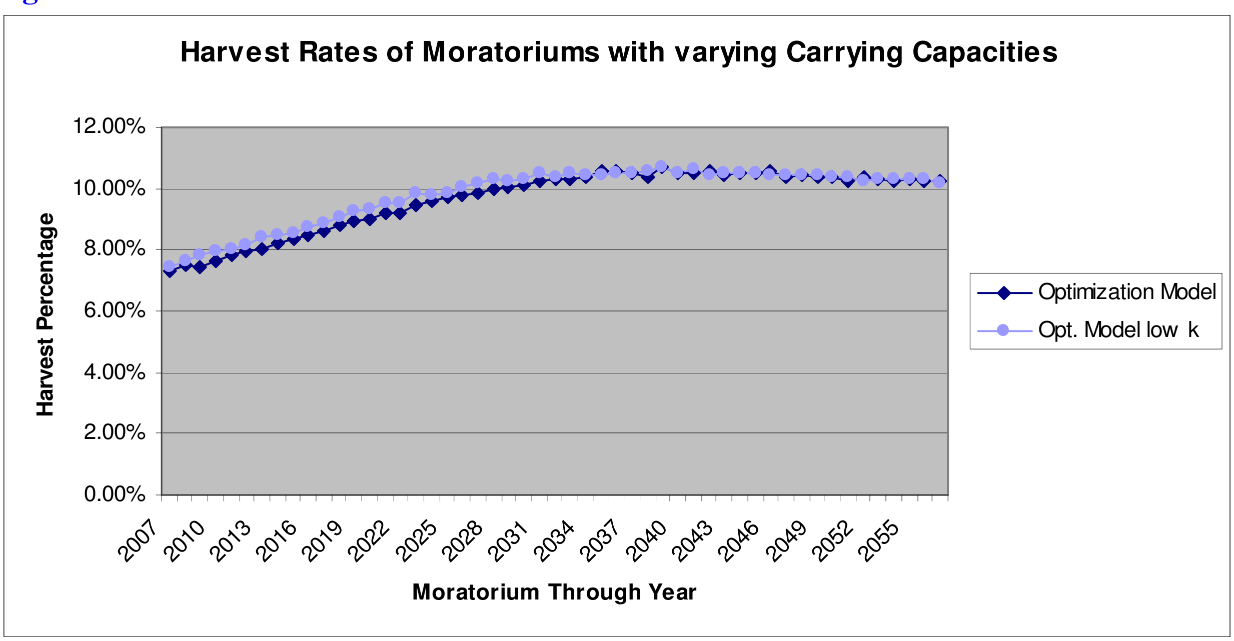
.07 discount rate. The pessimistic scenario is included below as section 3.9. None of the additional sensitivity tests included in the revised draft result in any changes to the general conclusions of the model. Despite the different parameters that we used in the sensitivity tests, we always reached the general conclusion that a harvest moratorium of decent length will increase the NPV of this fishery and harvest rates should be much lower than current rates.

3.7 Sensitivity to the Carrying Capacity (k)

As a result of current and potential future habitat degradation, it is possible that we have overestimated the carrying capacity of market sized oysters in the northern Chesapeake. To test the sensitivity of our results to this parameter, we reduce our estimate of the carrying capacity by over 25% (k=3,678.9 million oysters) and rerun the model. This estimate is the sum of the carrying capacity of the low, medium, and high salinity zones in the Maryland portion of the Chesapeake Bay from Jordan and Coakley (2004). This estimate is lower than the total Maryland carrying capacity because they lacked salinity data for all areas with oyster habitat.

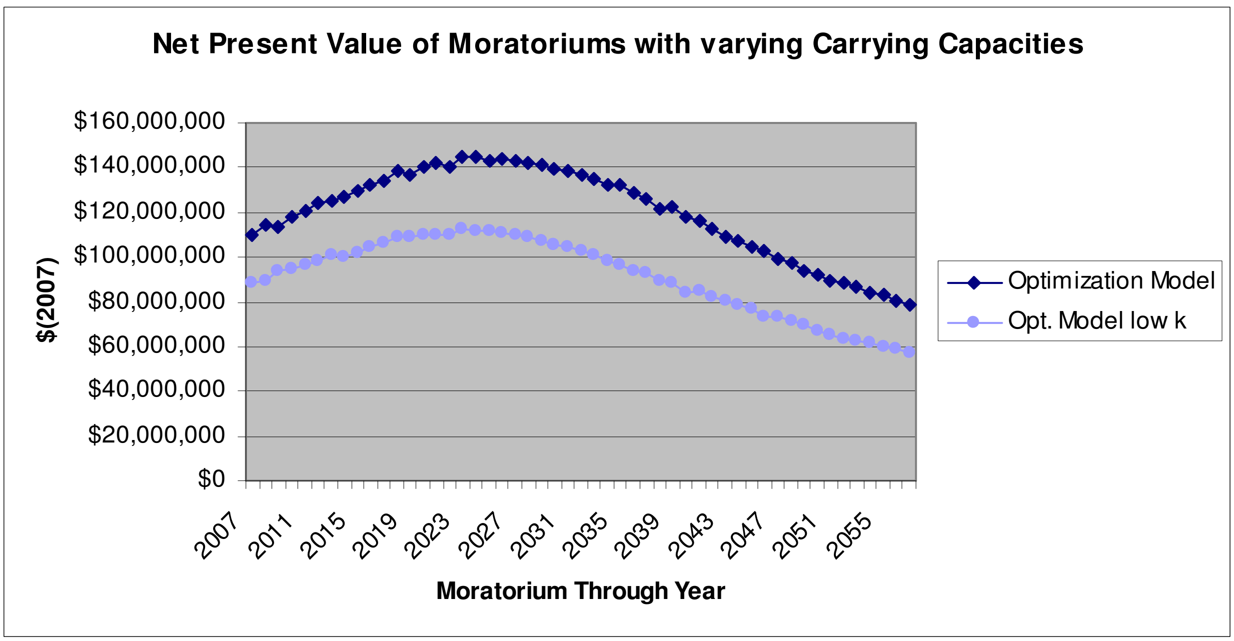
Despite the lower carrying capacity, the optimal harvest rate with a lower carrying capacity is nearly identical to the optimal harvest rate under the original optimization model for the moratorium lengths shown in Figure 8. Despite being slightly higher in the short moratorium years, the difference in harvest rates is never greater than half of one percentage point. This suggests that the carrying capacity estimate does not have a large impact on the choice of optimal harvest rate in the model.

Figure 8



The lower carrying capacity estimate also does not materially change the predictions of the optimization model in terms of which moratorium length maximizes the net present value of the oyster fishery. The optimal moratorium decreases from 2024 in the original optimization model (k = 5,089.2) to 2023 in the model with a lower carrying capacity (k = 3,678.9). The net present value estimates are presented in Figure 9. The net present value of the oyster fishery with the smaller carrying capacity is lower for all moratorium lengths for two reasons. The first reason is that the population can not grow as large, and therefore leads to smaller harvests at the same harvest rate. Secondly, density dependence in the stock begins with a lower stock, which means that the net growth of the stock, and therefore harvest, is lower. Therefore, while the value of the resource changes, changes in the carrying capacity estimate does not cause large changes in other predictions of the model.

Figure 9

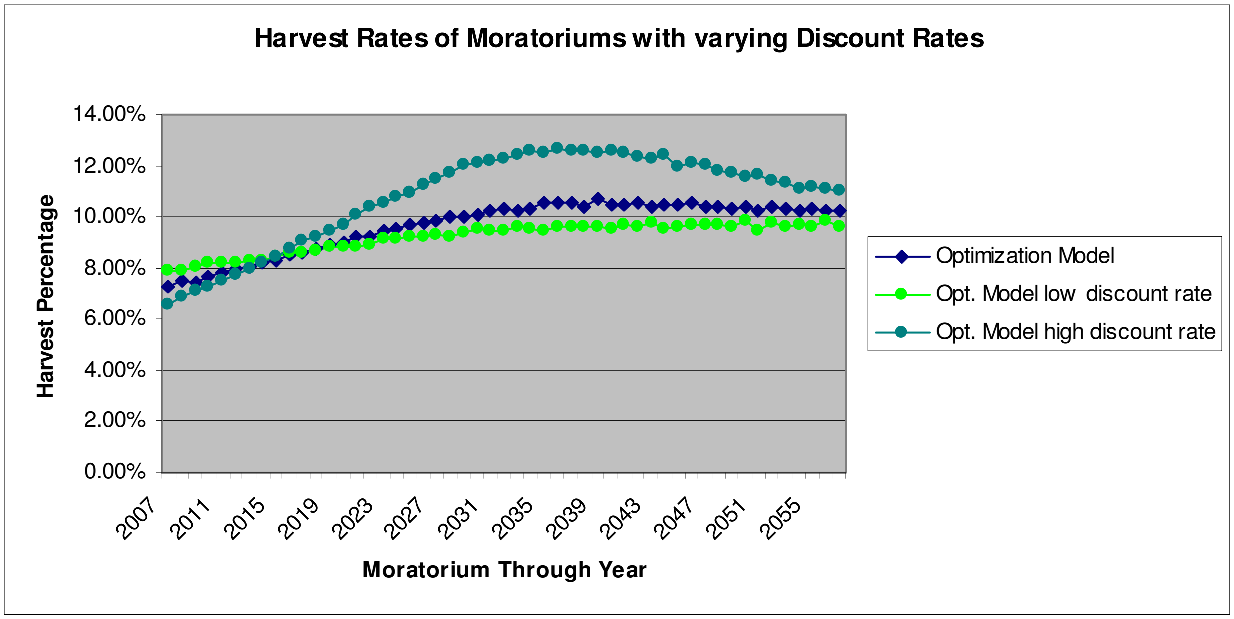


3.8 Sensitivity to the discount rate (δ)

Due to the long time frame of this study, the discount rate (δ) plays an important role in determining the optimal harvest rates in the future. In the initial analysis, we chose a relatively modest discount rate of δ = .04. We chose two additional discount rates to test the sensitivity of our model results to the discount rate; a low discount rate scenario (δ = .02), and a high discount rate scenario (δ = .07).

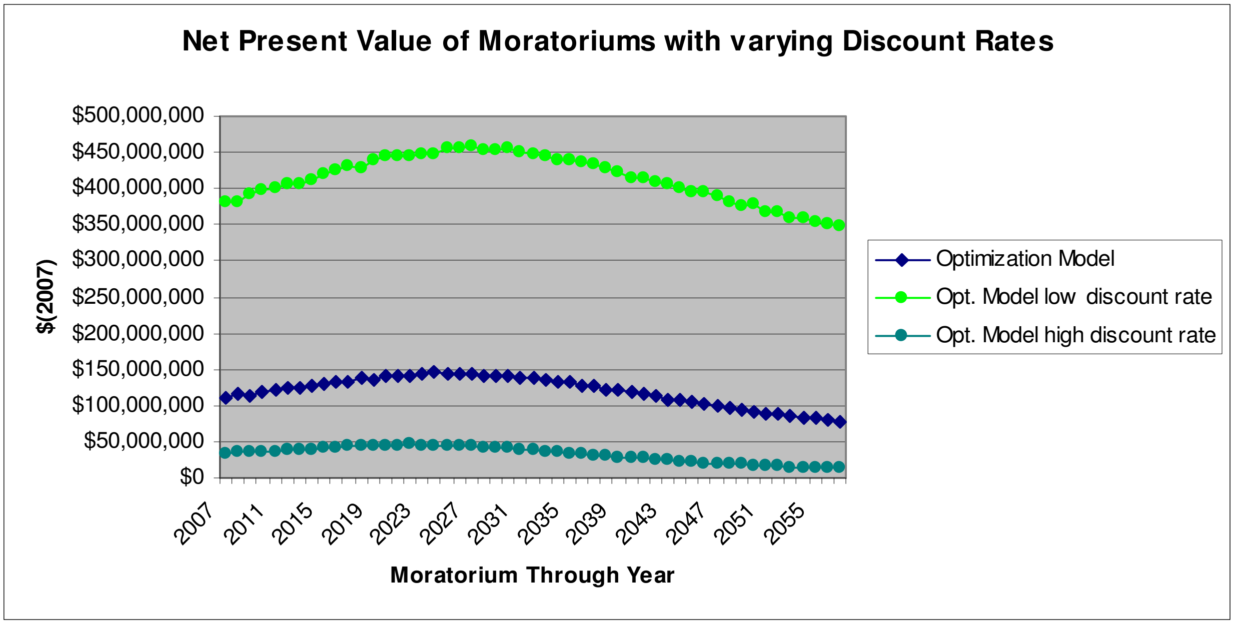
The optimal harvest rates are somewhat sensitive to changes in the discount rate, but the general concavity of the optimal harvest rates over different moratorium lengths remains the same. The optimal harvest rates for each discount rate are presented in Figure 10, and do not vary by more than three percentage points for any moratorium length. The high discount rate scenario begins below the original model and ends above it. This may seem counterintuitive as one would expect that the higher discount rate would force harvests to be higher earlier. However, with the high discount rate, harvests far out in the future are worth very little, so we prefer harvests earlier. The best way to increase harvests in the middle early years is to let the stock grow and harvest a smaller portion of stocks each year. At some length of moratorium, the stock has already increased in size so that a larger percentage harvest is now optimal because they are more valuable in the near term. The opposite argument holds for the case with the low discount rate.

Figure 10



The net present value estimates are shown in Figure 11. Not surprisingly, the discount rate has a dramatic effect on the net present value estimates for each scenario. However, each scenario still shows an inverted U shape as moratorium length increases, which suggests that for any reasonable discount rate, the net present value of this oyster resource could be improved by a moratorium. The optimal moratorium changes from year 2024 in the normal model to 2022 in the high discount rate scenario and to 2027 in the low discount rate scenario. This suggests that while the net present value estimates are quite different, the optimal moratorium length may not vary substantially.

Figure 11



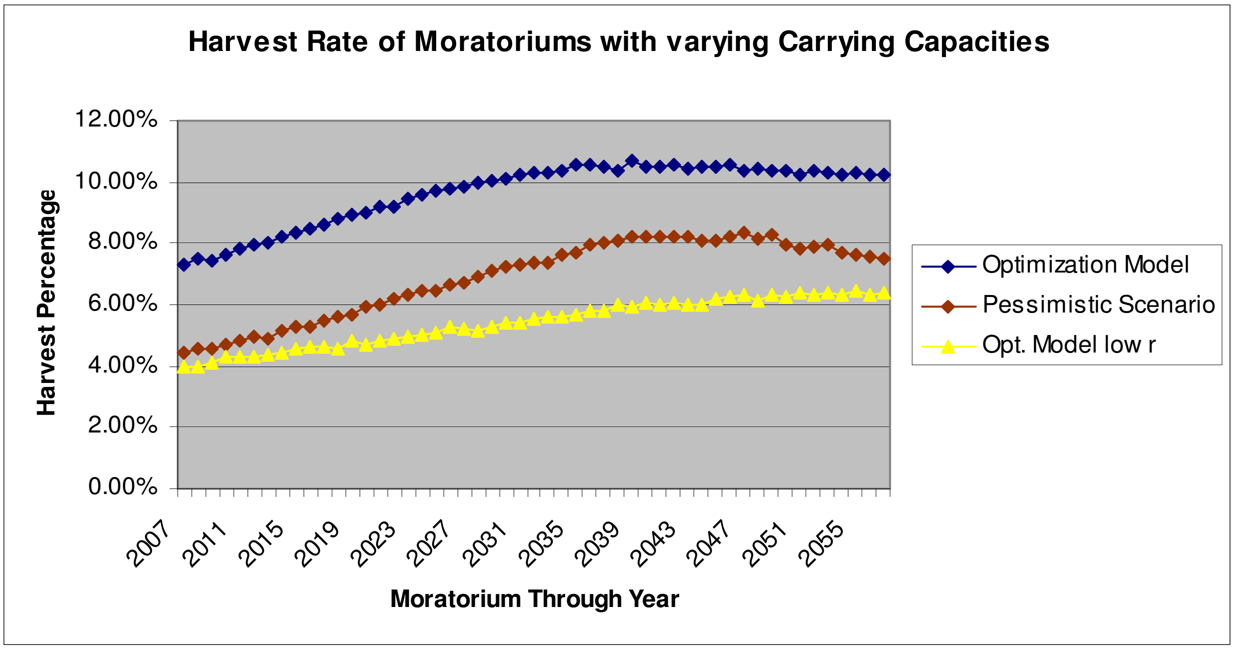
3.9 Pessimistic Scenario

Recently, it has been suggested that the Eastern oyster can no longer grow sufficiently well in the Chesapeake Bay to support a thriving oyster industry as a result of degradation in water quality, reduction in available habitat and increased disease mortality as well as many other potential reasons. This is evidenced by the proposal to introduce a non-native oyster. The last sensitivity test of our model uses the low growth rate (r = .184), the low carrying capacity estimate (k = 3678.9), the high discount rate (δ = .07), and no ecological services value in what we will call the pessimistic scenario.

This scenario is included to test two questions: "Under these pessimistic circumstances, can it be optimal to harvest the Eastern oyster to economic extinction as the current policy would suggest?" and "What are the optimal harvests if the oysters cannot grow sufficiently well in the Chesapeake Bay?" The first question asks whether our parameters were misspecified and the current policy really is an optimal economic outcome, or if the optimization model can do better under these pessimistic circumstances. The second question relates to an alternative to the introduction of a non-native oyster into the Chesapeake Bay. If the Eastern oyster can no longer grow sufficiently well to support an oyster industry as large as it has in the past (which appears to be the goal of management), what are the optimal harvest rates (and therefore industry size) under this scenario?

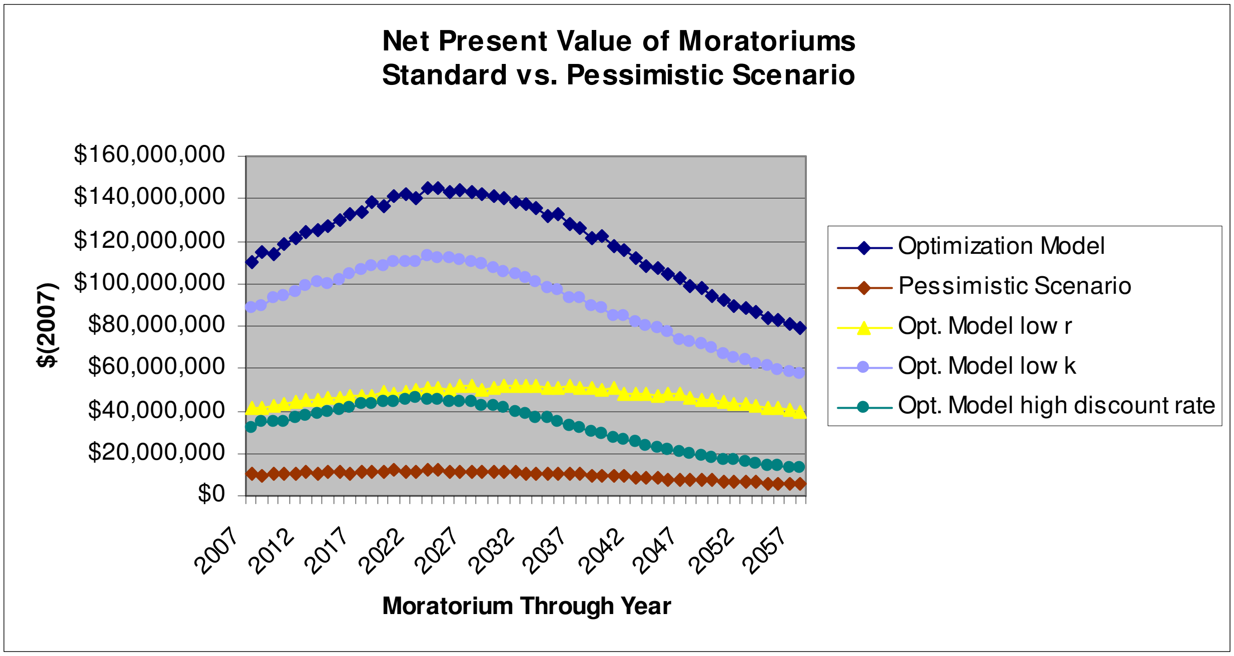
The optimal harvest rates under the pessimistic scenario are shown in Figure 12. Not surprisingly, they are lower than the original model and higher than the optimization model with only a low growth rate. With shorter moratoriums, the optimal harvest rates are close to those of the low growth rate scenario, but as the length of moratorium increases, the high discount rate leads to higher harvest rates to make up for the additional harvests occurring later in time. However, in all scenarios, the harvest rates are still substantially below the current policy of allowing watermen to harvest on average 28% of the population of market sized oysters annually.

Figure 12



Not surprisingly, the net present value estimates for the pessimistic scenario, which are presented in Figure 13, are much lower than any of the previous models. Combining all of the negative influences of a low growth rate, low carrying capacity, and a high discount rate on the net present value of the fishery results in a fishery which is valued at 10% of the original optimization model.

Figure 13



However, the net present value of the optimization model and the current policy model with the pessimistic scenario is presented in Figure 14 and suggests that even this scenario results in an inverted U shaped net present value curve. The optimal harvest moratorium for the pessimistic scenario is through the year 2023, which is just one year before the original optimization model's optimal harvest moratorium. While the moratorium does not appear to increase the value of the oyster fishery by as much as the optimization model, the additional two million dollars in net present value terms for waiting until after 2023 to begin harvesting results in a 20% increase in the value of the fishery. The current policy model also results in an inverted U shaped net present value curve, but result in a lower value for the fishery in all moratorium lengths.

Figure 14

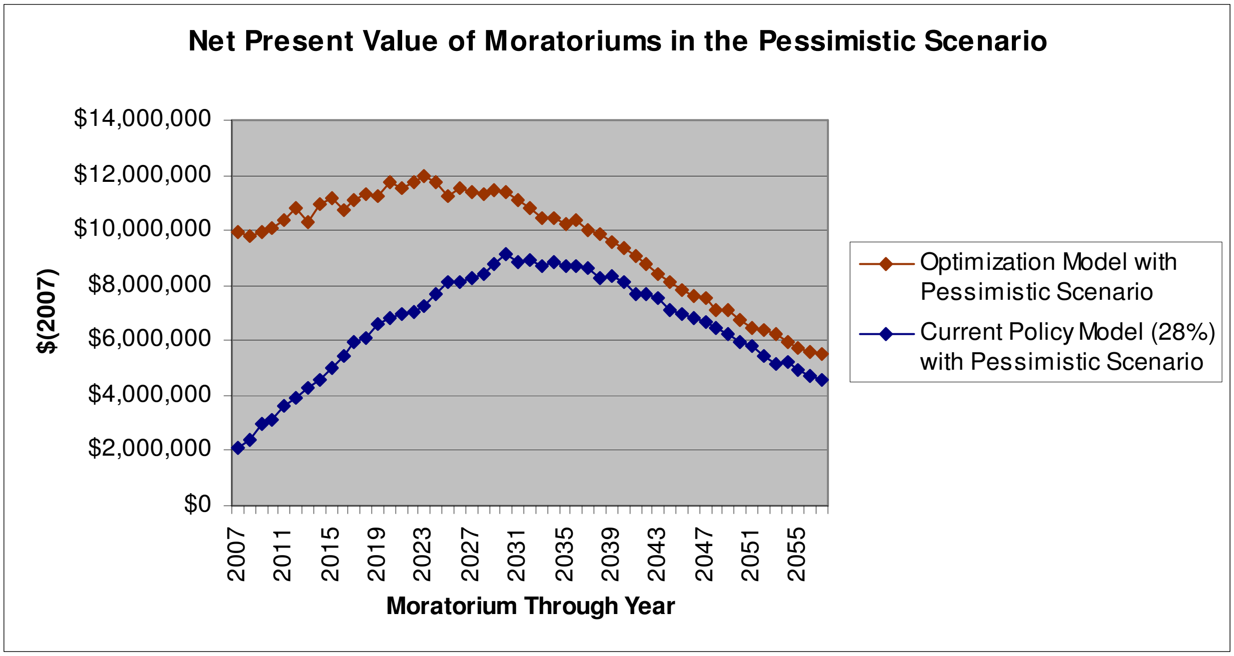


Figure 14 shows that, for some lengths, a moratorium can increase the net present value of the oyster fishery at harvest rates that are lower than the original optimization model. These lower harvest rates never lead to the depletion of the Eastern oyster population in the Maryland portion of the Chesapeake Bay in any of the 1000 simulations for each potential moratorium year. In contrast, the current policy model leads to depletion of the oyster population in nearly all of the simulations for each potential moratorium year. Therefore, under these pessimistic circumstances, the model suggests that the current policy is not optimal, and also suggests that we should be harvesting at a lower harvest rate, not a higher one. Even with these pessimistic circumstances, the Eastern oyster in the Chesapeake Bay can have substantial value if the stock is allowed to rebound and then is harvested at a modest rate.