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Vermont Fish & Wildlife Department and Board
Louis Porter, Chris Bernier, Mark Scott and Fish and Wildlife Board via email

Re: An Assessment of the Status and Harvest Trends of River Otter and Bobcat in Vermont

Dear Sirs:

My name is Weldon Bosworth. I live in Gilford, NH. As an environmental consultant for over 40 years, now retired, I have evaluated a variety of potential impacts to populations and communities of biological organisms using quantitative methods. I have a Ph.D. in zoology with a concentration in ecology. A summary of my training and experience is attached.

I recently learned that the state of Vermont is considering increasing the length of the trapping season for bobcats and otters. For a number of reasons, I am very much opposed to putting more trapping pressure on these two apex species. I decided to review the scientific basis supporting this proposal. Although I am not a wildlife biologist, my education and experience qualifies me to critically review the report: "An Assessment of the Status and Harvest Trends of River Otter and Bobcat in Vermont" (Assessment Report) with particular attention to its conformance to accepted scientific methodology. I have used a variety of statistical methods in the various studies I have conducted throughout my career. These methods have included various parametric and non-parametric methods including regression analysis, one-way and multivariate analysis of variance and co-variance, and other multivariate methods including hierarchical (cluster) analysis, principal components analysis, discriminant function analysis, etc.

I reviewed the Assessment Report with the intent of evaluating whether the evidence presented is adequate to support the proposal to extend bobcat trapping season for the month of December or to extend the otter season to coincide with beaver season. Based upon my review not only was there no data presented that would support those proposals, it appears that the Vermont Fish and Wildlife Department (Department) is relying only upon regression analysis of harvest data to evaluate the stability and sustainability of the bobcat and otter populations. There is no evidence of any population assessment methods such as a population model that could inform wildlife managers of the impact of the proposed changes to these populations. Population models focus on understanding the demographic processes influencing population growth through births, deaths, immigration and emigration. These models also show how the demography may be influenced by external factors such as environmental fluctuations and by management actions such as increasing the harvest. These types of models have been around for decades and despite some of their shortcomings should be the minimum foundation needed to support any resource management decision.

While the Assessment Report purports to provide an analysis that allows wildlife managers to weigh the pros and cons of extending the bobcat and otter seasons, in my opinion it not only fails in this attempt, but falls far short of the minimal expectations for a sound scientific analysis. Were this report submitted to a peer-reviewed journal I believe it would be rejected. Although superficially the Assessment Report appears to be a comprehensive analysis containing an appropriate statistical foundation for the proposed decisions, to an interested scientist with an understanding of the scientific method and use of statistics, it is not only very difficult to follow the logic presented in the report, it is impossible to verify any of the quantitative analysis

presented. As a result, the report “clouds” the issue before the Board rather than providing the Board with the information needed to make a sound decision.

A scientific report should clearly and transparently discuss the design of the analysis including the hypotheses that are tested, the data to be used, selection of Type I error rate, i.e., alpha level (=test of significance), discuss each of the variables that affect harvest and how they were controlled in the analysis, present methodology that is to be used to test significance of trends and relationships, and present the results in tables that include error and results of the significance tests. Lastly such a report should also discuss the uncertainty of the results is a way that a resource manager can quantitatively evaluate the consequences of their decision. None of this information is presented in the Assessment Report.

From a communication perspective the report is not organized in a clear and transparent manner and one gets easily confused as the discussion jumps from harvest data to license sales to CPUE data and seasonal variability within the same sections. More significantly, there are several problems with the parameters of the regression analysis conducted and the interpretation of the results. Some of these problems include:

1) The assessment is essentially a data mining effort, statistically speaking a "post hoc" analysis, i.e., done after the fact. Nevertheless, the design of the analysis should have been presented where the Type I error rate (alpha, i.e., the test of significance) and power of the tests used and the rationale for these choices discussed. Because these analyses are “post hoc” and because not all the independent variables controlled for, a more conservative alpha should have been used, e.g., 0.01 rather than 0.05. If a more conservative alpha was used few of the so-called "trends" would have been significant.

   What this means is that there is a strong likelihood that the conclusion that a trend in the harvest data existed at an alpha level of 0.05 is actually a pattern in the data that may have resulted from “chance” or random variation.

2) Although no data sets were presented in the report it is clear that there are several independent variables that could have influenced the “trends” in the harvest data that were evaluated by regression analysis. Trapping success, response to trapping surveys, catch per unit effort, weather and geography are but a few of the variables that were briefly discussed in the Assessment Report and which could have potentially affected the harvest data. The annual data that are presented and interpreted as a “trend” could have resulted from any or all of these variables influencing the harvest. Yet there is no discussion of how these several independent variables are controlled for in the analyses. The regression analyses conducted only asks whether there is a “trend” in the data, not which variables contributed to the “trend”.

   Multivariate analysis of variance (MANOVA) could have been used to answer the question of which of the independent variables exerted a significant effect on the harvest. MANOVA is mathematically similar to the regression analysis used, but with the difference that the independent variables are categorical. MANOVA can be used to ask whether there are differences among the means of some dependent variable, the harvest, that were more likely due to one of the variables than another. To the extent that the data don’t meet the assumptions of a MANOVA or the MANOVA was inconclusive, this would be evidence that the data should not be used as a foundation for decision making. Other multivariate methods, such as discriminant analysis might also have been used to infer which of the several variables were potentially most important in controlling the harvest.

3) In most cases the relationship between the harvest data and years was very low. This is the R^2 statistic cited in the regression analyses. The R^2 for all the regression analyses was less than 0.6, usually less than an R^2 of 0.4, and several less than an R^2 of 0.2; for the otter data the R^2 was 0.1466 over the 35 years of harvest, truly a very weak relationship between harvest and years. In some instances, the author even made the "freshman" mistake of talking about "slight" trends when the relationship was not significant at even a 0.05 alpha level.

   Although these "trends" may have been significant at the alpha level used (0.05), the relationship is very weak because of the scatter of data. Had the "S" statistic (standard error of the regression) been presented we could have used that to infer how much confidence a resource manager would have had in predicting the next year's harvest from this regression model.

   What this means is that although there may have been a significant trend (at least at the more liberal alpha level), the precision of the "predictor variable", i.e., or ability to predict the next data point is very low because of the substantial variability in data being analyzed. This is relevant because the question before the Board is whether the season, and thus the harvest, should be increased. With such low R^2, any prediction of whether such a "trend" continued is very uncertain. This is hardly a foundation upon which to base a sound scientific decision. With this much uncertainty clearly caution should be used when considering increasing harvest, particularly when the two species are apex predators that have a significant role in maintaining a balanced ecosystem.

4) There is no evidence presented on the power of these analyses. The power of any test of statistical significance is defined as the probability that it will reject a false null hypothesis. Very likely the power of these tests is very low, hence the reproducibility of these results is very low. Thus very little confidence can be placed on the results. Much of this low power results from the relatively small sample sizes, i.e. one data point per year, and the fact that the many independent variables have not been controlled for.

In my opinion because of the high variability of the data, the low power of the test, the use of a liberal alpha level and the fact that the independent variables were not controlled for in this analysis, the conclusions of the Assessment Report should be accorded little weight. There is no scientific foundation presented in this report that would allow a resource manager to understand the potential consequences of increasing the season for either species and certainly no data or evidence presented that would allow a conservative resource manager to err on the side of precaution in making that decision.

Even after doing all these "trend analyses", there is no evidence presented on the stability and sustainability of either the bobcat or otter populations, much less evidence that would allow one to predict the impact of the proposed season modifications on these populations. Absent a better understanding of the stability and sustainability of these populations along with transparent and objective decision criteria to guide the management decision on extending the season, any decision made by the Department with regard to the bobcat and otter season is essentially an "uncontrolled experiment" and I use that term advisedly since the science upon which this decision is apparently to be based is inadequate to determine the consequences of putting more trapping pressure on these species populations. Because of these reasons, I recommend that the Board oppose the proposal to extend the bobcat and otter seasons.

Weldon Bosworth Ph.D. - Experience

Overview

Dr. Bosworth is a Senior Ecologist with over 40 years of consulting experience in evaluating environmental impact and working with clients to develop strategies for site remediation. He has managed and led multidisciplinary teams evaluating potential environmental impact at several major marine, estuarine, aquatic and wetland sites and addressed a number of controversial site remedy issues. These efforts have included ecological risk assessments, natural resource damages assessments as well as evaluation of contaminated sediment management issues. This work includes lead management roles at six Superfund sites, four EPA Region V Area of Concern sites and several contaminated sediment sites in Canada.

He was a member of and past Chair of the Scientific Advisory Committee of the U.S. EPA's Hazardous Substances Research Center South/Southwest, a consortium of universities: Rice University, Louisiana State University and Georgia Tech, that conducted exploratory research in issues dealing with contaminated sediments and dredge materials. In this role he was responsible for critical review of funding proposals and approval of final reports.

While Vice President of Operations and President of Normandeau Associates (1972-1985) and President of Balsam Environmental Consultants (1986-1994) he was responsible for technical quality of all reports produced by the consulting staff of these companies.

Areas of Expertise

- Ecological Risk Assessment
- Natural Resource Damage Assessment
- Contaminated Sediment: Transport, Fate and Management
- Marine and Aquatic Ecology
- 316a & 316b Evaluations

Education

- PhD/Concentration in Ecology/1976/Oregon State University
- MS/Zoology/1969/University of New Hampshire
- BA/Zoology/1964/University of New Hampshire

Employment

- 1994 – 2016, AECOM (formerly URS Corporation and Dames & Moore), Senior Consultant
- 1986 – 1994. Balsam Environmental Consultants, Founder, President and Senior Consultant
- 1972 – 1985, Normandeau Associates, Inc., President, Executive Vice President, Vice President of Operations, and Project Manager

Registration/Certification: Professional Biologist, British Columbia, # 1230 (retired)

Professional Affiliations

- Past Chair and Member, Scientific Advisory Committee of the USEPA Hazardous Substance Research Center/South and Southwest, 1992-2002.
- Member, Society of Environmental Toxicology and Chemistry, 1998-2014
- Member, Marine Studies Curriculum Advisory Committee, Southern Maine Vocational Technical Institute, 1979-1980.

- Invited member to NOAA North and Mid-Atlantic Region Conference on Marine Pollution Studies, 1980.
- Executive Board Member, New England Estuarine Research Society, 1976-1980.
- Participated in OCEANLAB (undersea laboratory) workshop sponsored by New England Marine Advisory Service, 1976.

Publications

Authored and/or contributed to hundreds of technical reports on various aspects of biological communities. Author or co-author of several peer-reviewed publications.