

Peer Review of DEFRA report on ‘Modelling the consequences of the new cormorant licensing policy’, Central Science Laboratory, December 2004.

Executive summary of review

Strengths – In general, the report is based on a robust approach to modelling wild populations, makes good use of the available data, and draws sensible conclusions that are based on evidence rather than judgement. The analytical strengths of the study are that it uses a variety of modelling approaches to minimise reliance on any one type of population model, it uses many starting values for both demographic and harvesting parameters, and it employs two separate methods of assessing impact of harvesting on population size. Taken together, these approaches allow conservative interpretation of results, and sensible suggestions on how to improve data and models in the future.

Major query – The estimation of 1979 population size, with which harvested populations are compared, is poorly described and may be biased in a non-conservative direction with respect to the effect of harvesting on population size. I think this potential problem needs to be addressed because, as the report states, comparison with the 1979 population size may be important in any legal defence of the culling strategy.

Minor queries – (1) The models assume no immigration or emigration from the system being modelled; assume no ongoing change in habitat use among the English population of wintering cormorants; assume no age- or sex- biases in harvesting; and assume no changes in harvesting outside England. Some of these assumptions are mentioned briefly in introductory text, but I think they should be re-emphasised during the concluding section of the report and their potential effect discussed. (2) The report contains no details on the methods used in the sensitivity analysis and the results are difficult to interpret. (3) The section on Adaptive Resource Management is poorly linked to the rest of the report, is not specifically based on either the presented data or the models, and could be interpreted as the one section relying on judgement rather than evidence.

Reporting and methods

1) Does the report state and address all aspects and *objectives* of the study?

The report does state the aims of the study but I think this aspect of the report could be further improved. In the present draft, the aims are stated gradually through the text and, in my opinion, the reader only really fully understands the scope of the study after reading the full report. I therefore think that the report could be improved by stating a transparent overall aim at the end of the Introduction section, and a series of specific aims at the beginning of the methods section.

The report does address all aspects and objectives of the study. I do not think there is need for further widening of the scope of the report.

2) Are there any weaknesses in the *design of the model* that could draw doubt on its conclusions?

No, the report should not be weakened by weaknesses in model design. One of the major strengths of this study is that it uses a wide variety of modelling techniques, each

of which is based on a different set of assumptions regarding the way in which natural populations are regulated. Conclusions are then based on an overview of the results from various models, so that weaknesses of any one model should not have a significant effect on the overall advice provided. This approach is recognized as good practice in population biology, where individual systems are rarely known sufficiently well to confidently fit a single type of model.

The study does identify one type of model (the 'Bootstrap Model') that appears to be unsuitable for analysing this question at the current time. However, although the results of this model are shown in detail, the report clearly draws attention to the poor performance of this model, explains the most likely explanation for this poor performance (too few years of data), and makes sensible suggestions on how to overcome these difficulties in the future (gather more years of data).

Having praised the overall modelling approach employed in this study, I do have one potentially important concern regarding the analytical section of the report. The study uses two methods of assessing the impact of culling regimes - the likely extent of population decline, and secondly the probability of the future population size dropping below the historical population size in 1979. My concern relates to the second of these two methods and is threefold.

(i) The method by which the 1979 population size is estimated is not clearly described (see page 8, lines 4 to 6). Is the regression based on raw population counts or on log values? Since linear regression models are described, I presume log data were used. Why is only the 1986 value removed? In other analyses both the 1986 and the 1987 values are removed. Including the 1987 value would lead to a steeper exponential slope, which would in turn lead to a downward bias in the estimation of the size of the population in 1979. Such an underestimation would be non-conservative in the context of the current report, where one of the objectives is to estimate the probability of the future population size dropping below the historical population size in 1979.

(ii) Is it sensible to use a linear regression method to extrapolate historical population sizes when the introductory section to the report states that population size has increased at an unusually high rate in the last two decades? Assuming that population size has increased at an exponential rate between 1979 and 2001 may lead to an underestimation of the size of the population in 1979. Again, such an underestimation would be non-conservative in the context of the current report, where one of the objectives is to estimate the probability of the future population size dropping below the historical population size in 1979.

(iii) Why use a single mean estimate for the size of the population in 1979? Estimates of historical population sizes have confidence intervals and, to be conservative, projected future population sizes should be compared with the upper confidence interval for the 1979 population size.

I think these potential problems regarding the estimation of the size of the population in 1979 and the comparison with projected population sizes need to be addressed because, as the report states, comparison with the 1979 population size may be important in any legal defence of the culling strategy (page 8, lines 3 to 4).

3) Are any assumptions made in the model sound and clearly identifiable?

In the main technical sections of the report, yes most assumptions are usually stated and explained in sufficient detail for a reasonably knowledgeable reader to understand the limitations of the conclusions.

There are, however, several cases where key assumptions or even parameter estimates are based on unpublished reports. The most important of these unpublished reports are Kershaw & Hughes (1997), Wernham et al (1999) and Hughes et al (2000). Without being able to refer to the detailed methods and results in these reports it is not possible to judge whether the associated assumptions and parameters are sound. The same is true of the statement that the modelling approach is based on the unpublished manuscript by Smith et al (in press July 2004). In such cases, where most readers would not be able to easily obtain the unpublished report, it would be useful to explain briefly how the estimate was obtained and upon what assumptions it may rest.

There are also a few areas where the models make some important assumptions but these are not highlighted in the conclusions section. For instance, the models assume no immigration or emigration from the system being modelled; assume no ongoing change in habitat use among the English population of wintering cormorants; assume no age- or sex- biases in harvesting; and assume no changes in cull regimes outside England. When combined with the slow life history of the great cormorant, which shows a substantial delay in reproduction between sexual maturity and age of first reproduction, these assumptions could create a lag effect in the relationship between culling regime and population size. Some of these assumptions are mentioned briefly in introductory text, but I think they should be re-emphasised during the concluding section of the report and their potential effect discussed.

In addition to these general points, I also have a couple of technical queries. For instance, why are annual peak counts of cormorants used to estimate the overall population size, rather than a measure of central tendency? Why are 1.455 and 2.695 chosen as extrapolation factors – why use halfway values, and between which figures are these halfway? Why is 50% used as the criterion for acceptance in interpreting the results of the simulations where the population in 2007 declines by more than a given percentage? And why not present in the text the detailed results of the density-independent models based on the proposed culling regime (3,000; 3,000 and 2,000)?

Finally, the report contains no details on the methods used in the sensitivity analysis and the results are difficult to interpret. A detailed description of the methods need to be added to the report and the associated figure (Table 7) needs to be explained in more detail if these sensitivity analyses are to remain in the report.

Data and analysis

4) Are uncertainties in i) the data used in the model and ii) the model itself identified and recognisable?

Yes, I think that the report does an excellent job of explaining the difficulties involved in collecting, calibrating, and modelling this type of data. The concluding sections are also, in general, careful to draw attention to the limitations of the current data, models and results.

5) Are the figures and tables in the report of the model adequate, not actually or potentially misleading, and support the inferences drawn from them?

Most of the figures and tables are clearly presented, are representative of the overall results, and support the biological conclusions drawn.

The only exception to this, in my opinion, is Table 7, which describes the results of the sensitivity analyses. I found this table very difficult to read and suggest that it needs to be redrawn to match the standard presentation of sensitivity analyses. The methods underlying this sensitivity analysis also need to be described in detail in the methods section.

Concluding

6) Is any relevant subject matter or evidence ignored or under-represented?

No, the concluding sections of the report make excellent use of the results from the models. The models themselves were tightly focussed on a couple of key questions – the likely extent of population decline and the probability of population size dropping below historical levels – so there is little risk of any information being overlooked.

7) Are conclusions based on judgement rather than evidence clearly recognisable?

In general, the conclusions are based closely on the evidence presented in the results and the link between the analyses and the conclusions is made very clear. This is a substantial strength of the report.

The only section that does not appear to be closely based on the evidence presented in the report is the section on Adaptive Resource Management. This section is poorly linked to the rest of the report and, apart from the last paragraph on the Frederiksen et al. (2001) paper, adds very little of substance to the report. I suggest that this section is reduced and/or moved to the introductory section where it could not be confused with the conclusions drawn from the current study.

8) Do the model and logical arguments support the conclusions/ recommendations?

In general, yes, the results of the models do support to conclusions and the recommendations on how to improve the models in the future are based on observed difficulties.

Overall

9) Does the work represent sound and robust science and are the conclusions supported by the evidence and analysis presented?

My opinion is that, in general, the report is based on a sound and robust approach to modelling wild populations, makes good use of the available data, and draws sensible conclusions that are based on evidence rather than judgement.

My most substantial worry about this study and the report is the robustness of the estimate of the size of the population in 1979, and the comparisons between this

historical estimate and the projected size of the population under various cull regimes. I think that this estimate and this comparison needs to be looked at again in detail and either the current methods need to be better justified or else more robust methods needs to be developed.

I have also identified a number of more minor weaknesses in the report, but most of these do not have a major impact on either the results or the conclusions themselves.