

## DELPHI DISCUSSION ON POPULATION SIZE AND TREND

Steve explained the process for the discussion. We desire to retain the anonymity in the discussion, but no big problem if it slips. He asked panelists to focus on the reasons why they scored the way they did. We displayed only raw scores and highlighted those that were greater than 50 to help identify any patterns.

## DISCUSSION ON POPULATION SIZE

It appears that some people chose one category over another. The concentration of numbers is within the upper and lower bounds, but the upper half received the most points. Therefore, the point estimate, in the minds of the panelists, is likely to be low. This indicates that the panelists may believe the methods are more likely to underestimate the population than to overestimate it.

The sampling methods may leave out birds in the interior forests. It is possible that it isn't a problem because the CERW are common along roads and we don't know if they have higher abundance away from roads. Additionally, the density of roads is great enough that there are very few interior forests. The detectability may be less than assumed in the mathematical model. Panelists discussed many reasons for not detecting birds and generally concluded that the error is likely greater for underestimating than for overestimating. In summary, route allocation, detectability, and route location along streams instead of ridge tops is likely to lead to missing some birds.

120m for detection is probably an overestimate. The BBS surveyors are not all keyed into CERW and a lower detectability distance adds underestimation error.

Petra Woods' unpublished study comparing interior and roadside abundance showed no statistical difference. Avian biology census methods, roadside bias study that showed no differences. John Bart's study showed a landscape change and over time, we are surveying different habitats. Leads to undercounting.

Route allocation is biased to roads, stream, and uneven sampling between years is a problem.

The method doesn't account for the proportion of the males that aren't singing, and we don't know if there is one female for each male, as the method presumes. We don't know how big the unpaired male population is, nor do we know what proportion is not signing at any given time. David Buehler independently estimated population size using habitat models and plot estimates [get reference] in the Cumberlands; his results are consistent with the PIF estimate.

Detectability is difficult at longer distances because the sonogram changes.

Males in Ontario do not tend to sing after egg laying [get reference]. Two song points in a week would get 80 % of birds. No (less than 2%) floaters in population—could be higher percentage if polygamous, which might indicate a larger than anticipated effective population size.

Could we statistically test for route allocation bias? There are two projects proposed, but they are not funded.

Little changes in detectability distances result in big changes in distance. The model using an average maximum distance, not an absolute maximum distance. Detectability differs depending on forest type and other landscape features. BCRs with fewer birds may overestimate population; this may be a problem in the periphery of the range.

Panelists separated the biases between missing birds because of route allocation (e.g., location and number) and detectability.

Statistical smoothing raises an issue with estimates.

Observer biases are probably not an issue because all observers are trained and highly qualified to detect CERW, and the methods correct for observer differences.

#### DISCUSSION ON TREND

Strong signal for the trend estimate within CI being correct. There is a strong reason to believe that this method is robust. There may be more of a tendency to miss a decline than to pick one up when it isn't there.

Breeding survival may be low because we don't have survival estimate on the non-breeding grounds. This may more of a concern for the future than in the past.

We don't know what is going on in all areas of the range but we know that a portion of the population is not sampled. This is a concern if the some populations are consistently missed and those populations are experiencing different pressures than the ones being sampled. Most of the panelists believe that this was a minor concern because the 50% of the birds not being sampled are probably not geographically separated or experiencing different pressures. They are probably just birds that weren't singing when surveys were taken.

Sample sizes of some BCRs are very small in the 1<sup>st</sup> 7 years of the survey and they might be weight the decline disproportionately. These rates of decline are consistent and the panelist didn't believe that the sample size problem unduly influenced the results.

The strong signal is because the methods are designed for this purpose, and they are refined repeatedly. Statistical support for this method is sound. Pattern of decline is consistent and reaffirms itself year after year. Sample is adequate for the purpose of

trend estimates. Signals are consistent anyway you break down the data, (spatially and temporally).

Methodology is strong, published, and vetted on the ground and reality checked by CERW experts.

Rangewide approximately 10,000 points, over 200 routes. Data are adequate to meet the implied rigor.

Change over time might not be dependent on abundance.

The change is good for the population sampled, but it might not be representative of the population as a whole because 50% of population is missed in the estimate. Only males are sampled.

The trend works because it is measures the same thing from year to year with the same methods.

One of the reasons we see strong agreement, as indicated by the scores, is because those with field experience get signals in the field that the trend is right.

Don't know if male female differences are important

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**QUESTION:** Is there evidence that CERW range has changed (areas added or lost) since 1966?

We had no maps that specifically addressed this question. We displayed the Atlas map from Ken's presentation as a rough reference. New Jersey to the North East are all new sightings. These sightings represent a very small number of birds. Pocket of birds in Arkansas and MO, which might be new but also might be new occurrences. The first nest in Ontario was not reported until 1952—the Lake Ontario to Canadian Shield, including Quebec population is maybe 40 years old. Some of the Ontario populations have blinked in and out.

Increase areas are not consistently on, just mostly on or highly on occasionally. West Virginia increase may be a likely to sample/methods error. Not a real increase. The expansion of the range is documented in the NJ in the 50s and New England in the 60s and 70s. This could be re-expansion. 1700 and 1800 southern Ontario had no trees. This information is summarized in Paul's assessment in state-by-state section. References included there.

Was there a similar range expansion in the Lake States? Precipitation gradient may be a factor.

Ken has all this information summarized for St. Louis conference. [get reference].  
Expansion is to the NE not the NW.

The CERW no longer occurs in the Vicksburg MS area. Disappeared during or after the 1960s. May be disappearing from LA. The BBS however, may not sample the right habitat to pick up CERW in Louisiana. Moore had birds in 60s and Anderson thinks they are gone now (pers com Paul Hamel).

Small-scale (woodlot scale) disappearances are not uncommon in Ontario.

Illinois sites are in question. Some think that they are missing, but Atlas still has sightings. Abundances are probably lower than 15 or 20 years ago according to local researchers (pers com Jason Jones).

North Carolina (Ranoake river) may be new since the 1950s, but don't know for sure.

Is the western edge eroding? The southwestern edge may be eroding (e.g., pops declining), but not disappearing.

SOUTH AMERICA – more observations because people are able to get deeper into the forest because of greater accessibility.

It is possible the Ecuador and Peru may have disappeared. The local biologists believe that these portions of the range are no longer being used. *El Grupo Ceruleo* had funded surveys. One route in Bolivia had birds. Refer to table in Maria Isabel's presentation. Maria Isabel believes that the range has retracted in SA because of land uses that destroy habitat in the tropics. The change started in the 1940s in Colombia when the government offered incentives for development of the forests. In Ecuador, petroleum development, during the duration of the BBS, increased access to the east slope of the Andes and incentives for human colonization of the east slope of Andes.

Paul showed centroid analysis with Monte Carlo simulation. Pockets of abundance have likely moved around.