**GULN BIRD MEETING, 4/27/23**

**ANALYSES COMPARED FOR A SUBSET OF PARK-SPECIES**

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| **NAME** | **DESCRIPTION / COMMENTS** | **DETECTION ESTIM. FROM** |
| STAN\_PCOUNT | Bayesian version of N-mixture pcount(). Analogous to pcount() that many networks use, but incorporates random effects | Repeat surveys |
| DISTANCE | Requires defined distance bins (can’t use 100m+). Most flexible options for fitting distance function. Separately use output as offset in GLMM. | Distance sampling |
| SOLYMOS | Detectability & availability calc., then separately used as offsets in GLMM. Can use unlimited distance bins, but just estimates it from the other distance bins so adds to overall estimates uncertainty | Distance & removal sampling, separately |
| GDISTANCE | Distance model, and also uses repeat surveys to estimate availability (relaxes strict distance assumptions) | Distance sampling & repeat surveys |
| GMULTMIX | Removal sampling, and also uses repeat surveys to estimate detection probability | Removal sampling & repeat surveys |
| GDISTREMOVAL | The whole shebang | Distance & removal sampling & repeat surveys |
| BRMS\_GLMM | Bayesian GLMM model for trends in relative abundance. USGS BBS uses a variant of this. | N/A |

**MAIN TAKEAWAYS FROM ANALYSES**

So far I don’t see distance, removal, or N-mixture models generating results that are more informative than simple GLMM for relative abundance. In fact, in some cases I’m suspicious of the estimates because of some wild swings in the model estimates compared to the raw counts. Also, more often than not, the limited goodness-of-fit tests available for these models have indicated poor model fit. BUT with some minor tweaks in the field we may be able to get more out of the distance and removal sampling data, and I think that would be worth trying. ALSO I think it would be worth pulling together the bird folks from other regions and networks for a larger discussion about what they “really” think about their analyses and if there is any movement toward a particular common analysis approach.

With the data so far (at least the subset I tried)

* Unable to fit data with removal models (time-to-detection)—models consistently failed goodness-of-fit tests
  + BUT don’t stop collecting the time-to-detection data. With much larger sample sizes we may be able to estimate 2-call-rate models, which should fit the observed data better.
* Distance models sometimes fit well, but it was hit and miss
  + One problem with these model fits was that best-fit model distribution was often negative binomial (NB), and NB is notorious for giving occasional wildly large estimates and having poor model fit. Identifying and measuring good model covariates may be able to explain sources of detection/abundance variability enough to use a Poisson model distribution.
  + The models also fit poorly for species that had unusually low detections in the 0-25 min (possibly due to observer avoidance). Originally I thought to combine the first two distance bins but that would leave only two known distance bins (0-50 and 50-100). Most distance models cannot work with unlimited distance bins (100m+) and those that do require a lot of data to be able to estimate well with an unknown distance bin.
* Analyses that used repeat visits to estimate detection/availability and correct the raw counts (STAN\_PCOUNT, GDISTANCE, GMULTMIX, GREMOVAL) tended to generate model estimates that were higher than raw counts (as expected) and closely paralleled the raw counts most of the time, with occasional “wild numbers”. This matches what some others (Matt & JP) have said about their bird estimates from such N-mixture models. To me, this isn’t particularly useful.
  + Repeat sampling is useful for other reasons, though (just maybe not for N-mixture modeling). Certainly the information from repeat visits seemed to influence model estimates (and CI’s) quite a bit.

**CHANGES TO CONSIDER IN THE FIELD (DISCUSSION TOPIC)**

* Is there any way the last distance bin could be split into a 100-150m bin and 150+ bin (or drop the 150+ altogether)? If so, we could use insights from future surveys to approximate what % of counts in the prior 100+ bins could be assigned to a 100-150m bin (roughly, and specific to each park-species). This would then give us 4 distance bins for estimating the distance function and if we need to combine the first 2 bins for certain species, it still leaves us with 3 distance bins, which is the minimum necessary to try to estimate a distance function.
* Consider using a decibel meter to collect more accurate values for a noise covariate? (But continue to collect the categorical noise covariate—prior to using decibel meter so not biased-- until/unless we determine that the decibel meter actually is a much better covariate for detectability.
* In the protocol, how long are observers asked to “wait” after arrival to allow birds to “settle down” before initiating counts? (or does that make no sense to do?) I’m wondering if some of the model-fitting issues with the first distance bin and first time interval bin might be partially addressed in some way in the field?
* Do the field ecologists have thoughts on additional covariates for detection or abundance that would be useful for reducing unexplained heterogeneity in the data?
* Can we get % forest covariate from NLCD data? Matt uses 1km radius for this estimate but said the 500m radius estimate was just as good as a covariate for his point count models. Finding an objective habitat covariate (we can evaluate various “sizes”) could really help these models and may make it possible then to combine parks in a single model (with Park as a fixed effect so separate abundance estimates and trends for each park). Having more data to estimate the distance and removal functions for a species may greatly improve model fits.
* Can the observers mark when a detection is visual rather than auditory? For distance and removal sampling, mixing these two types of data violates model assumptions. I know that most of the detections are auditory, and it would actually be best if we could just stick with recording auditory detections if that is the case. If we decide not to do distance or removal-based analyses, then this point is moot.

**JUST THINGS TO THINK ABOUT**

* What is the possibility of increasing the number of sites per park but not going to all sites every year? Is there a reason the original rotating panel was dropped? The year-to-year change is not so great (so far) that it seems necessary to visit each site every single year. Could get better inference to the Park and better habitat covariate estimates with more sites, it seems.