Letters to the Editor

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Model-based approaches to deal with detectability: a comment on Hutto (2016*a*)

To the Editor

In a recent paper, Hutto (2016*a*) challenges the need to account for detectability when interpreting data from point counts. A number of issues with model-based approaches to deal with detectability are presented, and an alternative suggested: surveying an area around each point over which detectability is assumed certain. The article contains a number of false claims and errors of logic, and we address these here. We provide suggestions about appropriate uses of distance sampling and occupancy modeling, arising from an intersection of design- and model-based inference.

We begin with three points about which we agree with Hutto (2016*a*): (1) "The most important thing a researcher can do is use some common sense…"; (2) "There will always be a trade-off between breadth and depth in sampling…"; and (3) "Fixed-radius surveys should not be universally condemned and should certainly not be the basis for rejecting a study outright." If these principles are followed for any given study, we argue that common sense would require that detectability be considered within the context of the study objectives. It may be possible to assume constant detectability over time, allowing estimation of trend without estimating detectability, or to assume all individuals on a plot are detected, in which case probability of detection is unity by assumption, but such assumptions must be reviewed and validated. A precautionary approach, and common sense, imply that the onus is on the researchers to show that the effects of not accounting for detectability are negligible for their goals. To do so, detectability cannot be ignored, and one must therefore collect relevant data about the detection process. This is certainly the case for Hutto's suggested method: the data are used to choose a distance up to which detection is certain. In the absence of a formal model, this choice is subjective. It risks being too small, discarding most of the data and compromising precision, or too large, resulting in downward bias in density estimates and unknown bias in trends and/or spatial variation in density (depending on whether detectability changes over time and/or space). Further, this distance will depend on many factors including species, habitat, and observer. In short, the suggestion implies an unspecified ad hoc "model" for incomplete detection as a function of distance.

Hutto's paper focused on criticisms of distance sampling and, to a lesser degree, occupancy modeling. While both approaches incorporate detection probability, they do so at quite different scales, and the types of inferences they are useful for differ. We, therefore, deal with them separately.

Distance Sampling Models

Distance sampling is arguably the most commonly used method to estimate densities of wild animal populations (Buckland et al. 2015). Like Hutto (2016*a*), we focus on point transect surveys, where surveying takes place at a set of point locations. Whether the method is useful in a particular study depends on the study goals, the ability to meet assumptions, and the consequences of violating those assumptions.

Assumptions of conventional distance sampling are (1) sampling locations are random within the area of inference, (2) animals at the point are detected with certainty, (3) the survey is a snapshot in time, (4) measurements are made without error, and (5) detections are independent events. The first is a design assumption, and thus guaranteed to hold, given proper design. It ensures animals are located independently of point locations, and so the distribution of animals with respect to the points is known (a triangular distribution), allowing derivation of the conventional estimators of density and abundance. The certain detection at a point is key, with downward bias if violated. Additional data can be collected if it is not possible to meet the assumption (e.g., Burt et al. 2014). If animals move during the recording period, the snapshot assumption is violated. Time spent at the point is thus a compromise: time must be sufficient to ensure that all animals at or near the point are detected, but not so much that substantial movement occurs. Buckland (2006) proposed a method in which animal positions are recorded at a snapshot moment, with time before the snapshot used to locate animals, and time after used to confirm locations. If movement is likely to cause strong bias, cue-based methods might be used, in which cues (e.g., song bursts) are the objects of interest (Buckland 2006). Large measurement error can induce substantial bias, and extensions incorporating measurement error models are available (e.g., Borchers et al. 2010). However, as suggested by Hutto (2016*a*), it is preferable to use design and field methods to minimize errors, rather than to rely on analytical methods to address the problem after data collection. Finally, methods are extremely robust to failure of the independence assumption; see Buckland (2006) for an example.

Some key statements in Hutto (2016*a*) ignore well-established features of distance sampling. The first reads

"A decrease in number of bird detections with increasing distance from an observer is often a result of habitat heterogeneity rather than distance per se. As an extreme example, consider the nature of data that might be collected along a narrow riparian strip; the more riparian dependent species are never detected beyond 20 m, but one would be wrong to conclude from their detectability profile that they are not very detectable." This would only occur if the first assumption listed above is ignored. Points must be placed at random with respect to the population they are supposed to be sampling. If that does not happen, as in the example when sampling along rivers, density gradients with respect to the points are to be expected. Alternatives exist for such cases at the expense of a need for additional data and increased analytical complexity (e.g., Marques et al. 2010), but again we prefer a more sensible design that avoids the issues rather than such a model-based solution. A second invalid argument reads "Uniform vegetation conditions must lie within the range of detectability of a given species for its detectability profile to be meaningful." The property of pooling robustness (Buckland et al. 2004: 389–392) implies that estimation is largely unaffected by heterogeneity in probabilities of detection among individuals, habitats, etc. The estimated detection function represents the pooled detection function required to correct for detectability. Pooling robustness will not apply, for example, to habitat-specific density estimates, but it does apply to average densities through the study region, and to estimated total abundance. Nonetheless, sample size allowing, detectability can be modeled as a function of covariates, such as habitat, using multiple covariate distance sampling (see, e.g., Marques et al. 2007). A randomized sampling design of points should result in robust estimates of detection probability and density across a heterogeneous landscape, but proper stratification would also be required to provide habitat-specific density estimates. Conventional distance sampling may not be appropriate for small or narrow habitat patches relative to the typical survey radius.

One of Hutto's (2016*a*) main propositions is that, rather than estimating detectability and abundance using a model-based approach, one should use raw count data from fixed-radius point transects. Based on his own data (Hutto 2016*b*), an example of the Black-backed Woodpecker (*Picoides arcticus*) is presented. It is suggested one could simply truncate data within 80 m to obtain a "reasonable presence–absence index of bird abundance" and get the same patterns of occurrence with respect to fire severity as with distance sampling. We tested this claim by fitting a hazard-rate detection function model to this data set (Fig. 1). Code required to produce Fig. 1, including a short description of the data and some additional comments, are provided as online Appendix S1. Contrasting with Hutto's claims, the analysis of the woodpecker data clearly shows that at 80 m

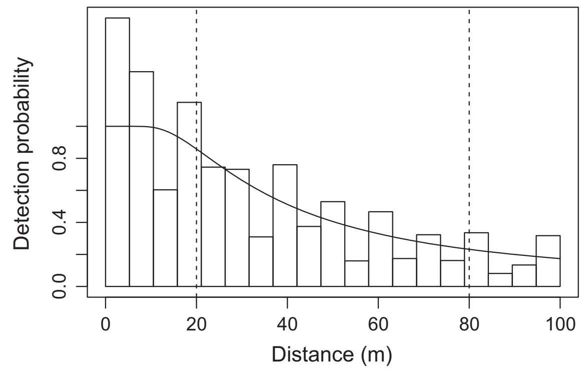

FIG. 1. Fitted hazard rate detection function model to the data presented in fig. 1 of Hutto (2016*a*: Fig. 1). There is strong evidence that, even at 20 m, detections are very likely to have been missed, and that at the 80 m at which Hutto (2016*a*) assumes detectability is still perfect, more than one-half (about 70%) of the detections might be missed. The 20 and 80 m distances are highlighted with dashed lines for easier reference.

detection is far from being certain, and even at 20 m detectability is no longer certain (cf. Fig. 1). Hutto (2016*a*) claims that "Data drawn from within a smaller 20 m radius become sparse. . ., so sample size artefacts are likely to have affected the resulting distribution pattern." Fig. 1 clearly shows that, if a fixed radius >20 m is chosen, for example 40 or 60 m, many birds will be missed, and density estimation assuming certain detection will be strongly biased. Given such clear influence of distance on detectability (cf. Fig. 1), one might ask why Hutto's analysis of proportions of woodpecker detections across habitat classes, using raw detection data truncated at 50 and 100 m, yield similar conclusions to densities estimated for each class based on distance sampling (Hutto 2016*a*: Fig. 3). Similarities arise because the woodpecker data do not exhibit differences in detectability by habitat class (Appendix S1: Fig. S6). Under strong habitat detectability differences, these patterns would not have coincided, and the use of uncorrected indices would likely have led to erroneous conclusions.

A misguided criticism in Hutto (2016*a*) is that different analysts will arrive at different density estimates when modeling detectability. While, as in any modeling, subjective choices might be necessary, model selection and goodness-of-fit tools can help to reduce arbitrariness, and, more importantly, model uncertainty can be incorporated into inference. Further, one should aim for data that lead to robust inferences independent of particular analysis choices, in general achievable with good field methods and survey design. By contrast, under Hutto's suggested ad hoc fixed-radius approach, different analysts may select different fixed radii, potentially generating markedly different conclusions. Without a formal modeling framework there is no way of selecting an appropriate radius, evaluating the impact of different choices and propagating forward the variance induced by such process.

Hutto (2016*a*) criticizes "uncovering bird–habitat relationships" using model-based detectability methods. Again, this criticism is misguided. The problem is not distance sampling or any other detectability-based approach. Problems arise if researchers collect data at a given detectability scale (say kilometers) and then wish to make inferences at a completely different scale (say meters). Naturally, if one ignores such a mismatch, existing relationships might be obscured and spurious relationships found. However, ignoring detectability does not solve the issue. The solution is common sense: collect data at spatial (and temporal) scales for which inferences are needed. Having clearly defined questions is fundamental. If patches of habitat are, on average, 5 m across, collecting point transect data up to 30 m will be equally problematic, whether or not one accounts for detectability within that distance.

Occupancy Models

The general intent of occupancy models is to examine patterns of species presence across a landscape during a certain period of time, often referred to as a season. Depending upon the study objective, presence may be interpreted as "species is always at a location during the season" (i.e., locations are closed to changes in occupancy) or "species is present at some stage during the season" (i.e., location is used by the species; MacKenzie et al. 2006). Thus, irrespective of detection issues, the concept of species presence comprises both a spatial and a temporal aspect that should be well articulated as part of the study objective. Field work should then be consistent with the study objective. The dismissive nature of Hutto's comments about occupancy modeling is presumptuous about why a practitioner may be conducting point count surveys in the first place (e.g., to identify where a species is present within a very short timeframe, or to identify the level of use by a species of a range of habitats over a longer period of time). We agree that occupancy modeling will not be an appropriate tool for inferences under some types of objectives. However, there will be plenty of other situations where *occupancy* is a useful state variable and occupancy models are useful for separating out the biological and sampling processes using point-count data.

Hutto (2016*a*) states that "One of the most powerful measures of habitat suitability is actually reflected well in naïve detectability because it is probably safe to assume that a point where a bird is frequently detected is a much better place to be than a point where a bird is rarely detected." Many examples where this claim would be utterly wrong are possible, and any biologist involved in data collection would have his or her own counter examples to share. For example, animals might be more conspicuous in areas they tend to avoid; they may prefer to be in areas where they are less easily detected by predators or prey, and hence by surveyors. The assumption that "naïve detectability" must reflect habitat preferences in general is both dangerous and unjustified.

Occupancy models are typically described in terms of requiring repeat surveys, but that does not necessitate repeat visits to a location (e.g., Guillera-Arroita 2017). There are ways in which the repeat survey information can be collected in a single visit (e.g., multiple survey methods, multiple observers, incorporating time of detection or spatial subsampling), provided such a design is appropriate given the study objective and system of interest. In particular, there are extensions that consider the information about when/where detections take place in continuous time and that are well suited to a single-visit protocol (e.g., Garrard et al. 2008). Thus, collecting appropriate data that allow detection issues to be addressed during an analysis does not necessarily imply going to fewer locations due to the need for multiple visits per site. Given uncertainty about the outcome of the survey with respect to the biological quantity of interest (e.g., a probability of a false absence > 0.15), going to as many places as possible at the expense of multiple visits to some sites just results in a lot of data of questionable accuracy. Essentially, imperfect detection leads to a measurement error problem in the quantity of interest (species presence/absence; Guillera-Arroita et al. 2014). MacKenzie and Royle (2005) demonstrated that, under imperfect detection, going to fewer places with a greater number of repeat surveys can lead to more precise estimates than going to more places with fewer repeat surveys. This is because the probability of a false absence (i.e., the measurement error) contributes to the standard error of the occupancy estimate. In some situations, the most effective use of additional field effort is to reduce uncertainty due to the measurement error rather than going to a greater number of places (i.e., quality data before quantity of data).

Finally, we note that while Hutto (2016*a*) implies that the assumptions of the basic occupancy model of MacKenzie et al. (2002) will often be violated with point-count data, there is a broad suite of "occupancy models" developed to extend and relax these assumptions (for details see Bailey et al. 2014 and Guillera-Arroita 2017).

Conclusion

We conclude by challenging Hutto's concluding remark: "Common sense and biological insight really ought to prevail over what has become a frighteningly blind application of model-based solutions to the potential detectability problem." We naturally agree that common sense and biological insight are fundamental in ecological research. However, model-based solutions do not preclude these, but complement them. Model-based approaches, be it in wildlife abundance and occupancy estimation, or in other sub-fields of ecology, have become widespread, and their utility in ecological research is well appreciated (see, e.g., the recent editorial by Honrado et al. 2016). Model-based approaches, informed by biological insight and tamed by common sense, are well established and fruitful in applied ecology. In many respects, model-based approaches evolved with the realization that standardizing survey protocols to minimize detection failure across complex and variable field conditions is usually unrealistic (Ellingson and Lukacs 2003). The relative ease of access to advanced methods via dedicated software does mean that practitioners can implement sophisticated model-based approaches without understanding their requirements and assumptions. That is not a problem of the methods, but of the way they are used. We argue that the advantages of open access software still outweigh the disadvantages, especially if common sense is used and appropriate training undertaken. Therefore, it is unjustified to state that model-based approaches in general, and those that account for detectability in particular, should be avoided altogether.

We agree with Hutto that there is a trade-off between breadth and depth in sampling, but point out that the correct way of resolving this trade-off is with a clearly defined a priori objective, underpinning the determination of an optimal study design (e.g., MacKenzie and Royle 2005). This brings us to what is the crux of many arguments about the performance of particular analytical methods: poorly defined study objectives. These should include a clear statement about the specific biological quantity of interest. Clearly defined objectives are vitally important for both short-term studies and long-term monitoring programs (Yoccoz et al. 2001). Without these, debates about appropriate field methods and analyses cannot be resolved because there is no common benchmark against which the pros and cons of alternative approaches can be assessed. We advocate an approach to wildlife science that integrates design- and model-based inference, allowing assumptions to be verified by data (Nichols et al. 2009). The approach advocated by Hutto (2016*a*) may be useful for generating hypotheses to be further investigated using more rigorous approaches. However, to support the effective conservation and management of wildlife populations, ecologists generally require reliable estimates of population size, along with associated measures of uncertainty (Martin et al. 2007). Even when assessing trends over time, ignoring detectability might obscure existing patterns or lead to perceived trends where no real trends exist (e.g., Norvell et al. 2003).

A model is a representation of reality, not to be confused with reality itself. Model-based approaches to deal with detectability can be extremely useful, and will undoubtedly continue to be of widespread use in ecology and applied fields such as wildlife management. Under a given setting, it might be possible to disregard detectability, having considered its effects and concluded that, for the objectives at hand, these can be accommodated via a carefully chosen design. However, the burden of proof must not be inverted (see, e.g., MacKenzie and Kendall 2002). It is the responsibility of researchers wishing to ignore detectability to justify their choice and to present evidence that it will not have a misleading impact on the outcome of their study.

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