MEE-14-09-583 (Lucas et al.)

Responses to Referee's Comments

We thank both referees for their valuable and constructive comments. Please see our detailed response below. Line numbers refer to the revised manuscript unless otherwise stated. Citations in square brackets are to papers listed at the end of the current document.

Reviewer: 1  
  
Comments to the Corresponding Author  
It is rare that I have an opportunity to review such an potentially important paper  
  
My interest is in obtaining high quality monitoring data through large-scale citizen science. There are a number of challenges for bats and other taxa, but there have been some important and exciting developments in recent years for bats, including through other work by the authors here, that means that it is now possible to collect and analyse large volumes of acoustic data.  
  
With these data, an increasing number of projects are collecting such data, and there is challenge to do more with the information collected on bats passes, which are often used as a measure of relative abundance.   
  
For me, the most important development, would be to reliably infer true abundance from such data. This paper provides a novel approach to this problem.   
  
**Ref 1 Comment 1:** The main thing that I would like to see in this paper is discussion of other analytical approaches that could be applied to this problem. Practically, I would want to know whether the approach proposed here has significant advantages over other emerging alternatives. In particular, extensions of occupancy modelling have been applied to similar data where it is not possible to uniquely identify individuals, with the aim here as well of estimating true abundance, or at least to obtain a better understanding of relative abundance.  
  
Stanley & Royle 2005. Estimating site occupancy and abundance using indirect indices. Journal of Wildlife Management 69, 874-883.  
Guillera-Arroita & Morgan (2011). Species occupancy modelling for detection data collected along a transect. Journal of Agricultural, Biological, and Environmental Statistics 16, 301-317.

**Response:**

(Stanley & Royle 2005)

Assumes disjoint populations which gREM doesn't.

Occupancy does have problems with double counting.

Is relative abundance, not absolute.

Guillera-Arroita & Morgan (2011)

Looks great!

Estimates occupancy, NOT abundance.

Royle and Nichols (2003)

This paper DOES estimate true abundance from occupancy.

But assumes knowledge of parametric form of population distribution

Bayesian (not to everyone's liking).

In many cases throws away potentially useful information.

**Ref 1 Comment 2:** As discussed by the authors here, it would not be easy to field test how well this approach performs for species, such as bats, where this approach would be most valuable. I think that it is important to get this paper out now, but I would now like to see more extensive simulations next that do not assume an equal density across the area of interest, and make use of field data collected through tracking studies to consider how well the approach is likely to work in practice for different species. 

**Response:** What do you reckon? I think they mean they want to see this in later papers. But not sure.

Did Kate say to just fix typoes? Or put a response in?  
The only minor typo I can find it on page 13 (particularly).

Fixed

Reviewer: 2

The paper presents a natural extension of the random encounter model (REM) (Rowcliffe

et al., 2008) and ideal gas model (Hutchinson and Waser, 2007) of animal encounters. Here,

the generalised random encounter model (gREM) is proposed to allow inference of absolute

density from passive acoustics and camera trap surveys. The incorporation of a variable

detection zone and directionality in animal signals is a useful development, especially for

analysis of acoustic survey data. However, the gREM does not account for uncertain de-

tection of animal signals (as other methods do) and assumes knowledge of mean animal

speed and directionality—requiring extra field work. Further, the gREM does not account

for possibly detecting the same animal multiple times, a common occurence with acoustic

surveys.

I think this paper provides a good step toward further generalising encounter models

to better reflect the reality to which they are applied. Yet, I also think, due to the method’s

limitations, that its applicability is still to be realised.

I have included some suggestions that I hope may help improve the paper.

General Comments

**Ref 2 Comment 1.** Increasing sample size improves precision: in lines 57–60, 290–295, 301–302 and 318–320, you describe how precision improves as the number of captures increases or the

signal/sensor width increases. For any reasonable estimator, one would expect its pre-

cision to improve as sample size increases; and so, could you make it clear that greater

precision for a higher number of captures is a familiar result? Furthermore, you say

that greater sensor/signal widths improve precision as if this were a seperate find-

ing, yet it again is due to the number of captures increasing—could this be explicitly

stated? The equivalency of the two results is implied in lines 371–372.

**Response:** We have added clarifications in lines 290-295 and 318-320 that greater sensor/signal widths improve precision because of their increased capture rate. We have stated in lines 301-302 that the relationship between precision and number of captures is to be expected. “However, the precision was dependent on the number of captures across all four of the gREM submodels, where precision increases as number of captures increases, as would be expected for any statistical estimate”.

**Ref 2 Comment 2**. The effect of animal movement: in lines 60–61, you claim that different animal move-

ment models have no effect on precision or accuracy of the gREM; however, this is

contingent on perfect knowledge of animal directionality and average speed. Further-

more, it has been shown that animals with highly tortuous paths reduce precision (Gu-

rarie and Ovaskainen, 2012). Could this claim be better qualified?

**Response:**

**Ref 2 Comment 3**. Double counting with acoustic detectors: in lines 388–395, you mention the possibility

of detecting the same animal multiple times and claim that this would not affect accuracy of the gREM. Multiple detections of the same animal is a common problem when estimating density from acoustic data. The gREM is derived assuming each detection is with a different animal, and so multiple detections of a single animal causes positive bias in the density estimate, as well as less precision. Not being able to account for multiple detections, gREM may not be applicable to many acoustic surveys, calling into question the claims made in lines 62, 318, 325–332. Could this limitation of the gREM be discussed in the paper? Could the model be improved?

**Response:** We agree that double counting is often a problem in acoustic surveys. However, the gREM is in fact modelling encounters, rather than individuals, and so does not assume that each encounter is with a different individual (pg.340 in Hutchinson & Waser 2007). Another line of reasoning is to consider a population of one animal in a 1 x 1km square, with radius of 1km, animal velocity of 1km per minute and survey time of 1 minute. Then using the gas model equation (line 168), we would expect two encounters from the same individual. Conversely, if we experienced two encounters, we would predict an abundance of one. In this case, as the number of encounters is greater than the number of individuals, it is clear that the model is accounting for double counting.

**Ref 2 Comment 4**. Required knowledge of animal directionality and the detection zone: in lines 419–421, you state that perfect knowledge of animal directionality, animal speed, sensor width and sensor radius has been assumed. As such knowledge is often imperfect, it would be useful to know how misspecification would affect accuracy. The average profile can quickly change across boundaries (shown in Figure 4 of the paper) suggesting estimates would be sensitive to knowledge assumed. Also, you do not discuss how such measurements should be obtained as best survey practice. I think some practical advice would be useful if this method is to be applied.

**Response:**

**Ref 2 Comment 5.** Comparing gREM with existing methods: could other methods for acoustic data be better recognised within the paper? Furthermore, could the comparison between gREM and existing methods be better explained? For example, in lines 87–100, existing methods are described as “require information that is often unavaliable”; however, the gREM also requires knowledge of mean speed and detection radius which may be as difficult to obtain. Also, existing methods like distance sampling or capture-recapture allow for uncertain detection of animal signals whereas gREM assumes certain detection within the detection zone. It would also be useful to include description of other current methods applied to acoustic data (Marques et al., 2013), for example, in lines 422–424; and to acknowledge the applicablity of these methods, for example, in lines 431–432 by saying gREM is another possible method one can apply instead of “gREM will make it possible”.

**Response:**

**Ref 2 Comment 6**. Wilcoxon signed-ranks test: in lines 256–258, 287–290, you use a Wilcoxon signed-

ranks test to show that the gREM is accurate, however technically you only fail to reject

the null hypothesis that the median difference between the true and estimated den-

sity is zero, you do not accept the null hypothesis or prove it. I do not think this test is

necessary. Is it not sufficient to report the estimated bias of the density estimator? A

relatively small bias is indicative of an accurate estimator.

**Response:**

**Ref 2 Comment 7**. Reported precision: in lines 302–303, 373–378, you state that the coefficient of variation falls to 10% for all submodels at 100 captures. This result is dependent on the simulation scenario considered and, further, it ignores the uncertainty in assuming average speed, signal width and sensor width. Could it be made clearer that in practice the coefficient of variation will be much higher due to non-linear animal movement, multiple detections of a single animal and uncertain knowledge of speed and directionality?

**Response:**

**Specific Comments**

1. Line 93: “gas model”— calling it the “ideal gas model” could be clearer and a citation,

useful. See (Hutchinson and Waser, 2007).

**Response:**

2. Line 160: “animals outside the zone are never captured”— could this be made clearer?

Animals outside that then move into the detection zone are captured.

**Response:**

3. Line 228: what value does r take in the simulation? It would be useful to know for

reproducibility.

**Response:**

4. Line 237: “individuals were assigned 11 signal widths”— this sounds as if signal widths

were randomly allocated to each individual for every simulation; however, I think you

mean that a separate simulation was run for each signal width? Clarify?

**Response:**

5. Line 242: σ d = vT10 — why was this standard deviation chosen? Is it because a normal

distribution was used to simulate a strictly positive parameter (distance)? Could a biological justification be given, as you do for the other simulation parameters chosen?

**Response:**

6. Line 243: “this is the largest day range”— here you refer to an average velocity while

day range is a distance, might be better to say average velocity was chosen based on a

day range of. . .

**Response:**

7. Line 246: “or change direction (in a uniform distribution with maximum angle A)”— I

understand you are defining a correlated random walk, but this sentence could imply

you are sampling the new direction randomly from the interval (0, A). Clarify?

**Response:**

8. Line 249: Might be best to say it is a correlated random walk for (3) as you do later on

in the paper

**Response:**

9. Line 251: “were counted as they moved in and out of the detection zone”— I assume

animals were only counted as they entered and not counted again as they left, also

animals initially inside the detection zone would also be counted.

**Response:**

10. Line 253: “by assuming the number of captures per simulation” — Why would you

assume the number of captures? You obtain the number of captures from the simula-

tion. Clarify?

**Response:**

11. Line 256: “the density in the simulation”— better to say the true density to prevent

confusion with the density estimated from the simulated data?

**Response:**

12. Line 266: “we calculated how long the simulation needed to run. . .” — do you mean

how long on average, as the formula applies to the expected number of encounters.

**Response:**

13. Line 269: “real density” — perhaps use true density for consistency?

**Response:**

14. Line 271: “compare the precision between capture numbers” — I think more detail is

required: compare the precision of the density estimates from simulations with a differ-

ent expected number of captures.

**Response:**

15. Line 275: “using different amounts of time spent stationary” — should this not be the

time spent stationary on average? (This applies to all other instances of “time spent

stationary” in the paper.)

**Response:**

16. Line 338: “the sum of the average animal velocity and the sensor velocity” — could be

clearer? It is the average animal velocity (averaged over all bearings) relative to a single

(animal or sensor) focal individual (Hutchinson and Waser, 2007).

**Response:**

17. Lines 388–395: you begin by saying accuracy may be affected by the interaction between the movement model and detection zone, then end saying the opposite, in

brackets.

**Response:**

18. Line 411: “entire world”— better to say study area?

**Response:**

19. Line 416: “The distance travelled for an animal was assumed to be 40 km day −1 ”—

technically 40 km day −1 is a speed, also isn’t this the average distance per day?

**Response:**

**Response:**