Dear Mrs. de Angeli Dutra:  
  
I write to you regarding manuscript # OIK-08071 entitled "Can migratory birds spread avian haemosporidian parasites?" which you have submitted to Oikos.  
  
In view of the criticisms of the reviewer(s) and by recommendation from the subject editor, both found at the bottom of this letter, your manuscript has been rejected by Oikos. However, you are welcome to submit a thoroughly revised version of your manuscript. It will be treated as a new submission and may be evaluated by external reviewers.  
  
Author Contribution Indication  
The contributions of each author to this work must now be indicated when you submit your revised manuscript. To add Author Contributions using CRediT taxonomy (http://credit.niso.org/contributor-roles-defined/), simply click the “Provide CRediT Contribution” link for each author in the ‘Authors & Institutions’ step of the submission process. From there, you will be able to check applicable Author/Contributor Roles and, if available, specify the Degree of Contribution. You MUST provide this information as part of the revision process. Author Contributions will be published with the accepted article and cannot be edited after article acceptance. Therefore you must ensure the Author Contribution information you provide is accurate prior to final acceptance.  
  
Thank you for considering Oikos.  
  
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Sincerely,  
  
Prof. Dries Bonte  
Editor-in-Chief, Oikos  
  
  
  
Decision by the Subject Editor (Dr. Silke Bauer):  
  
Dear authors,  
Many thanks for submitting your ms to Oikos. As you will see below, two reviewers have commented on your study and ms. They were generally positive about it but raised several issues for improvement/clarification, some of them are fundamental (especially from rev1).  
In addition to the reviewers’ comments, I have some more comments from my own reading.  
First, indeed migratory birds are often assumed to disperse parasites along their migration routes. However, migration can also be a mechanism of reducing parasite prevalence through migratory escape, separation, etc. I would like to see this mentioned in the ms and the results discussed also considering these counteracting mechanism. Although this is partly brought up in the discussion, I would expect this much earlier in the paper.  
The presentation of all the models (and reports of their results) needs to be made much clearer and more concise. Please restructure the corresponding sections. I would also like to see effect sizes explicitly mentioned and explained in the results (e.g. how much larger is the geographic range of parasites that occur resident and migrants, etc.).  
Please also provide a table with bird species and their categorization as residents, partial or full migrants (perhaps in the supplement.  
Minor comments  
- L. 86 & 87 – “using” sounds a bit odd, perhaps infecting or found in might be more suitable  
- L. 125 – please provide reference or link to WorldClim  
- L. 145. Entire sentence seems redundant as same info given already in l. 143  
- L. 150. Change “..decided to build” to “built”  
- L. 155. Replace performed by run  
- L. 155. The second model seems redundant as you have already in the firs model a category for resident + partial/full migratory. Alternatively, this is contradicting your earlier statement (in first paragraph on this page)  
- L. 162ff – why separate models and not parasite genus included as a factor?  
- L. 175. Omit “bird” before prevalence  
- L. 176. Proportion of migr birds – you mean the number of individuals of mig species and not the proportion of mig species in the species pool at a locality, right? Please clarify.  
- L. 178. Replace in by on  
- L. 212. Replace “classed” by categorized or classified  
- L .216. “only” sounds counterintuitive with 175 lineages. How many were there in total?  
- L. 219. I guess “the” should be “that”?  
- L. 285. Partly redundant, please rephrase and/or merge sentences  
- L. 306. Such pattern could also be found if effects of parasites (lineages) on birds differ. See general comment on migration as a mechanism to change prevalence  
- Fig. 1 seems to be of relatively low quality - perhaps this resulted from the conversion but please check.  
-       Fig. 3 is redundant, info already provided in Fig. 2. In Fig2 spell out x-axis labels (rather than providing them in legend)  
- Combine Figs. 4& 5 into one multi-panel figure  
- Fig. 6. Needs more explanation. I wasn’t sure which model results are shown here. Incidence rate ratios seem not to be explained in the text; % migrants, %migrant species and number of migrants – seem like correlated variables – please clarify  
-  
  
I hope this helps you in revising your ms.  
Best wishes,  
Silke Bauer  
  
Reviewer(s)' Comments to Author:  
Reviewer: 1  
  
Comments to the Author  
This study is based on an impressive data from South America, consisting of blood samples from ~15,000 individuals (506 bird species / 156 localities), examined for presence and identification of haemosporidian parasites. The first hypothesis asks whether “migratory birds spread parasite lineages along their migratory routes” which is also the title of the manuscript “Can migratory birds spread avian haemosporidian parasites?” This is a somewhat trivial question to ask given that many publications have shown that migrants and local residents are sharing haemosporidian parasites, and that migrants frequently carry parasites outside the typical transmission range of the parasite (providing potential, but not yet realized, spread to resident birds). This hypothesis is tested by comparing the ranges of parasites found in both migrants and residents with parasites restricted to residents. I am not convinced that this a conclusive test of the hypothesis; if the former are found to have larger ranges (interpreted as caused by the spread of migrants) this could equally be explained by common parasites being more likely to infect both migrants and residents.  
  
The second hypothesis is of more general interest; is the spread of parasites by migrants of such magnitude that it has a measurable effect on the prevalence and richness of parasites in the host-parasite communities along the migration routes? I think the manuscript would be much stronger with a focus on this question. More details are also need on how you have calculated some of the key variables, like proportion of migrants in the community and species diversity. In some places in the discussion, you are drawing too strong conclusions (demonstrating effects) as your findings are more correctly statistical associations between variables.  
  
Detailed comments  
Line 37. Not clear what you mean by “act as an environmental filter to new species colonization”  
  
Lines 165-170. When you tested whether prevalence is higher in localities with more migrants, I wonder whether you only included resident species in the analyses. It is not clearly stated but I feel that this is how it should be done given that you want to know whether migrants have any impact on the local parasite community.  
  
Lines 180-181. “include only species with 10 or more birds..” In total or minimum 10 / locality?  
  
Line 235. “Proportion of migratory birds in the local avian community”. I could not find how you obtained this estimate. Is it the proportion of migrants among the captured birds? If this is the case, I wonder how well this would correspond to the actual “Proportion of migratory birds in the local avian community” as there are many potential biases (e.g. migrants may be more or less easy to catch depending on whether they are ground, canopy or edge foragers). Also, how was the catching at the 155 localities timed relative to the expected presence of migrants?  
  
Lines 260-262. This sentence needs to be rephrased. You have not demonstrated that migratory birds may disperse parasites (“demonstrate” followed by “may” is also confusing). I agree that you have shown that “lineages infecting both migrants and residents are more widespread than lineages restricted to residents”. However I don’t think you can separate whether this a result of migrants spreading these parasites, or that some lineages are just more common and thus more likely to infect both migrants and residents.  
  
One major finding discussed on lines 265 and 303-307 is the different effects of migrants on the prevalence of Plasmodium and Haemoproteus. This is interesting but to be confident that this is a solid biological finding, it would be good to know that this is not driven by the samples of the host species at the localities. Some species groups have more Haemoproteus others have more Plasmodium. Given the large number of host species sampled there must be quite different species compositions across the localities. If migrants are more common where the local birds have more Haemoproteus, it is of course an interesting association but the increased prevalence may not be due to the effect of the migrants.  
  
Lines 315-316. This is speculation that goes beyond your data – delete.  
  
Line 317. Replace “demonstrate” with a less strong word. You just have a statistical association.  
  
Line 353. Replace “demonstrate” for reasons given above.  
  
Line 357. “resident host species harbour the greatest parasite richness…”. But the resident species makes up 90% of the species so not that surprising, unless you mean that this is per species.  
  
Line 362 “migrants appear to select bird communities”. Delete, you have no data to reach this conclusion  
  
Lines 364-367. This is not a suitable closing sentence of the study. It is well established in the literature that migrants can carry haemosporidian parasites throughout their migration routes, and if I understand the study correctly, you have not directly addressed this question (would require data from a migrant at breeding quarters, stopover sites and non-breeding (winter) quarters.  
  
  
Reviewer: 2  
  
Comments to the Author  
In present study authors aim to clarify whether migratory birds influence the spread of haemosporidian parasites in South America and whether areas often crossed by migrants contain higher prevalence and richness of haemosporidians. They combined more than 13000 samples dataset and data obtained from MalAvi database. The results demonstrate that migrant birds may contribute to dispersal of haemosporidian infections. At the same time, it seems that migrant birds are present in regions with lower prevalence of Plasmodium and migrant birds can decrease the richness of haemosporidians.  
In my opinion, this study contains a very good example of how merge of parasite’s data belonging to different parasite genus and generalization can cause false conclusions. For instance, in the analysis of haemosporidian parasites authors showed no correlation between the occurrence of migrants and the number of infected hosts. But when analysis was made separately for Plasmodium and Haemoproteus parasites, it was found that there are negative and positive relationships between migrants and prevalence of infection.    
The manuscript in general is concise and clear, the conclusions are justified by data.  
  
There are several points that should be addressed for consideration as well.  
Authors use expressions throughout the text as “local percent of migrants” (Line 239) or “local number of infections” (Lines 234, 479, 483) which should be edited to “percent of migrants in area?” and “the number of local infections?”.    
Line 310. Correct to …due to the fact that haemosporidians…  
Line 325. Does any of these references contain information about the birds as well?  
Line 358. Correct to …despite the fact that migrants…  
Lines 481-482, 485-486, 487-488. Legends of figures should contain only explanatory information about the figure, but not the interpretation of the results.  
Figures 4 and 5 could be merged to one with A and B.