Comments on "Environmental variables determining the presence of an avian parasite: the case of the Philornis torquans complex (Diptera: Muscidae) in South America" by Cuervo et al.

Line 88-89 : this is a great example of how this information could be useful.
" to locate habitats with low risk of Philornis occurrence for the re-introduction of bird species or reinforcement of their populations"

Lines 133-136: This filtering method is a bit hand-wavy. It's tough because the presence-only sampling is from the literature, and sampling bias is therefor a huge issue - but it also means loss of information. Maybe elaborate on how the distribution "results" were evaluated (spatially? histogram of distance distributions?)?

Figure 1 needs some work with choosing labels that can be distinguished from one-another. A mix of light and dark colors would help.

Line 152: Need to define PET.

Line 150-156: I wasn't aware of this! How interesting! Given the very large spatial scale and very high resolution of the WorldClim data, plus the filtering to remove points clustered close together, I would think this wouldn't be an issue in this case. I would therefor like to see how their inclusion changes the results.

Line 158-175: I'm intrigued by the a prior hypotheses, but I'd rather see a bit more information about them (eg., in a table?) - what species were they conducted in. Short of looking up each reference myself, I did look up Sinclair (2015) and it's not specifically on these species, but insects in general. A review of sorts. That is not made clear in the text here: it sounds like it was observed in this species. It makes sense that it's not in the study species as the authors are claiming to be doing the first study on it. But the prior evidence and wherefrom it derives would be helpful to the reader.

Line 204-211: Nice!

Results: My main concern with the methods and results, and thus conclusions drawn, is that only 3 of over 900 models met the criteria for inclusion : is there correction for multiple tests. This is likely just my unfamiliarity with the specific methods here, but I implore the authors to explicitly explain how the methods control for multiple tests to an audience who may look at this with my same concern. I simply fear that the combination of very low initial sample size combined with the multiple model approach puts the study at risk of explaining artifacts. I have not yet made it to the discussion, so perhaps this is discussed, but I'd just like to see that the authors address why these climatic factors, which are enormously correlated, might only have been found significant in the 3 models? Is it simply due to the other variable sets being orthogonal to Set1? If so, a table of the variable sets and the relative variance explained by each one would be helpful.