Editor replies to rebuttal

I appreciate the authors attempt to clarify all the issues and noted a bit of their exasperation at several of my comments. Perhaps some of my comments are simply a matter of different perspectives and we'd have interesting discussions in our free time over these ideas. So, I will try to keep my comments here to what I still consider the important implications and consequences for the quality of the manuscript.

The first and foremost point of disagreement is over some of the replies to my comments on analyses. The first is the possibility of type I error. Since the effective sample size to estimate population trends is 11 (Palo) and 15 (PUVI), power is low. Thus, I was suggesting caution in some interpretations. I recommended the use of MARK, and mark and recapture type analyses. These are often required to insure that studies of population trends correctly analyse the data for powerful conclusions. The authors stated that this kind of analysis might require "vastly more data than we have." While I respect their additional comment that "First, the focus of this paper is not to estimate survival, but instead to consider how an index of population size may have changed over time for different species in this assemblage." I continue to think that this issue needs to be resolved in some way. Perhaps as the authors suggest, showing how captures over time might indicate population trends. But, my biggest concern is, if it is true that a population trend analysis using mark and recapture analytical methods requires "vastly more data" to explain the trends, then are they not also stating that their sample size is inadequate for this kind of analysis? The authors state that they are not estimating "survival" yet the entire idea of the paper is fundamentally about survival – population decline.

Next, the authors state that there was no evidence of autocorrelation. My comment was not about a statistical concept, but rather a biological one. For example, if we assume that capture rates or capture numbers during the field season indicate (in some way) population size, then we must recognize that population size should not be able to vary as widely as it does here from year to year. You can't have more individuals in one year than the sum of the population plus reproduction in the previous year – which is temporal autocorrelation. Biologically populations are always temporally autocorrelated, as can be seen from life table analysis (I am not suggesting you do that here!). My concern was about the biological side of the question. For example, with the Bullfinch, the (captured) population more than doubled in one year, 2001-2002 (PUVI) and 2004-2005-2006 (PALO). In the Solitaire, similar dynamics occurred – in 2008-2009 and 2010, the PALO population (captured) more than halved, then more than doubled, and in PUVI, along the course of the study it did that several times. Thus, my point being that during the time interval in question, while a regression line suggests a decline, the actual data suggest that the population can increase dramatically towards the end of the time period that the regression says it is declining. Which brings me to another point, about comparing statistical results to intuitive results based on examining the figures.

The authors state - "The purpose of conducting formal statistical analyses is so that one does not need to subjectively guess about the nature of a relationship between two variables." and I mostly agree. However, an entire issue of Ecology (March 2014) was devoted to this idea. A common theme running through the papers was that researchers must examine their data (usually visually) to see that the results agree with the logic (that is, to see whether the statistical outcome in fact makes good logical sense). Thus, finding evidence in a figure that goes against a statistical analysis of that figure is not necessarily being subjective. Often, subjectivity is blindly accepting the statistical result without recognizing that the data do not support that interpretation. Again, we arrive at a philosophical (perhaps) junction that might better be resolved in a friendly conversation over coffee or other beverages. Suffice it to say here that, in my looking at the figures, several (specifically – Rufous-throated Solitaire, Green-tailed Tanager at PALO, Bullfinch at PUVI, Highland Tanager at PALO due to 1997 and then the very low capture rate after, Western Chat-tanager for similar reasons as the Highland Tanager) suggest that there these analyses do not suggest a long-term decline, but rather simply show a lot of variability or simply the

effects of one strange year.

A part of this question is that analysing the analyses, the authors did not find any evidence of issues with the analysis – however, I can't help but wonder if that is a consequence again of sample size. That is, when sample sizes are small it is extremely hard to reject the null hypothesis of normally distributed residuals, for example. Thus, the argument becomes sort of circular – sample sizes are small, so it is hard to test whether analyses based on these sample sizes are robust. This idea is part of what I meant when I mentioned the adjustment to 1000 mist-net hours. For example, if captures used more than 1000 hours, then rounding down (interpolation) is no issue. However, if captures used much less than 1000 hours and some species were seldom captured, this "rounding up" (extrapolation) can often be a problem. The literature may show this procedure is common, but again, logically and biologically, we must consider the consequences.

Also, I was surprised that you chose to dismiss out of hand my comments about net avoidance. I have been netting birds for over 30 years now, and I have seen evidence that suggests net avoidance from year to year. Especially, if during the few days of your study you recaptured the same individual more than once. They could certainly become net shy. Since you do not include recaptures in any analysis, it is impossible for a reader to evaluate your assertion of no influence of previous capture on probability of future capture. Also, it is something that, despite the small sample size, could easily be examined. Simply testing whether the number of previous captures is related to the probability of future capture. Also, while survival might be low, in my own studies, I find tropical birds easily survive 2-5 breeding seasons, depending upon the species. Thus, I simply suggest that some information that is useful to understand whether your data truly suggest population declines could be gotten from recapture information.

At this juncture, I will close this part of my comments by saying that I think that the overall idea of the paper, and the clear declines (both visually and statistically in some species in some places) of interesting species, along with the difficulties of actually showing population declines in species that are not abundant, are important issues that need be discussed more, especially in places with a lot of endemism, or high diversity and low abundance (tropical regions everywhere). Thus, I do not expect you to "fix" everything that I have commented on here, but rather would like you to think about incorporating some of the ideas in your discussion and your analysis. That is, I am still not convinced that any species other than the Green-tailed Tananger only in PUVI, and perhaps the Western Chat-Tanager in PALO. Thus, if recaptures or if some other ideas I mention above were discussed, perhaps I would be more convinced.

Next, in the paper, you talk of 14 sampling periods (first sentence of Results - "In 14 banding sessions conducted over 13 years (Table 1), yielding >22,000 net hours, we captured a total of 31 species (Table 2)." yet in the figures of PUVI, there are 15 data points and 11 in PALO. Please check the manuscript and clarify accordingly. Also, please confirm that 11 or 15 were used in the analyses rather than 14 (if appropriate).

Line 38 in the revised manuscript – you accidentally edited out the word "by" which needs to be put back in between the words "caused" and "ongoing" - "Many of these taxa are of substantial conservation concern given extensive habitat loss caused ongoing deforestation in both Haiti and the Dominican Republic (Stattersfield et al., 1998; Latta, 2005)."

Line 107 - "We analyzed trends in capture rate for 6 endemic species that we believed were adequately sampled by our methods and for which we had obtained adequate sample sizes." While I may have suggested clarifying before, I think this sentence could delete the last part after the word "methods."

Last but not least. Your regressions suggest that there should be a slope that could be interpreted as

“rate of decline in captures.” While you show the slope in the figures, I would think that mentioning this rate could be useful in interpreting your results. You might mention how many years until no captures will occur with your sampling effort. This has practical implications for how much effort must be expended to examine population declines in species such as those in this study. It might be that it becomes economically unfeasible to study decline because, to capture a reasonable number of individuals, would require an enormous effort, during which time many non-target species will be captured for no reason. You speak of rate of decline as percentages, but would it not make sense to use something that is more concrete – captures per mist-net hour (1000 mnh), for example? Finally, and I leave this up to you to decide, when talking about percentage decline, you maintained the use of fractions of percentages.