A critique of 'Collision mortality has no discernable effect on population trends of North American Birds'

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In a recent paper, Arnold and Zink (2011) attempt to quantify the effects of collisions with large, man-made constructions on population trends of North American birds. They do so by investigating whether there is a statistical association between an estimated index of collision mortalities of birds and estimated long-term population trends in the North America Breeding Bird Survey (BBS). The question of whether there is a causal relationship or not is very relevant for conservation because large numbers of birds die every year due to the collision with buildings, wind turbines, electric power lines or other obstacles and the effects of this additional mortality on bird populations is currently unknown (Klem 1990, de Lucas et al. 2007, Hager et al. 2008, Drewitt and Langston 2008). To tackle this problem, Arnold and Zink (2011) first regressed the species-specific number of dead individuals found under a series of monitored buildings against the estimated continental population size of each species and an index of overlap of the species distribution with collision sites. Subsequently, they used the residual of a species as an index of its collision vulnerability. In a second step, they correlated vulnerability indices with the estimated long-term population trends of these species from analyses of the North American Breeding Bird Survey. Arnold and Zink (2011) found no correlation, and therefore stated "collision mortality has no discernable effect on population trends of North American birds". We think that based on their study this bold conclusion is not justified, because there are fundamental flaws and also other problems in the analysis of Arnold and Zink (2011).

First of all, we challenge the very basis of their study, viz. to interpret the absence of a relationship between estimated collision risk and estimated population trend as evidence that collision mortality has no effects on population trends in North American birds. The following example illustrates the fundamental flaw in this reasoning (see figure 1 on www.oikostat.ch/exchange/...). Imagine a species A, which would have a strong overall positive population trend in the absence of collisions. In the presence of collisions the observed population trend is slightly negative. The vulnerability of species A, expressed as the difference between the two population trends, is large. In contrast, species B is in dire trouble, perhaps because its habitat in the breeding or the wintering grounds is being destroyed. Its population trend in the absence of collisions is strongly negative. As species B has a low collision vulnerability, the observed population trend is only slightly less than what its population trend would be in the absence of collision mortality. The vulnerability of species B to collisions, again expressed as the difference between the two trends, is much smaller than that of species A. This example shows that a high collision vulnerability need not necessarily result in an observed, more negative population trend than a low collision vulnerability. Any correlation between collision vulnerability and population trends must be corrected for other effects on the population trends, before any inference can be made about the effects of collision mortality.

Thus, clearly in any correlative study, there is a risk that an observed relationship is not causal, but may be induced by another mechanism (e.g., Aldrich 1995) or, alternatively, that a causal relationship is hidden by the effects of another mechanism. Potential factors that may either affect population trends and/or collision vulnerability need to be included in such an analysis, otherwise there is a high risk to jump to unjustified conclusions due to pseudocorrelation. As an example of such confounding factors, imagine that bird species are categorized as urban and non-urban species. Urban species may generally have higher relative collision vulnerability than non-urban species, because they live closer to buildings on average. At the same time, urban species have higher population growth rates than non-urban species (Sauer and Link 2011). Even if there is indeed a negative correlation between collision vulnerability and population trend within each group, this may no longer be discernible if all species, urban and non-urban, are analyzed jointly without consideration of the habitat factor (see figure 2 on www.oikostat.ch/exchange/...). The conclusion would then be that there is no effect of collision mortality on population trends, which is clearly the wrong conclusion in this example. Another possible confounding factor in the analysis of Arnold and Zink of which we can think of is the life-history of the species. It is likely that many such confounding factors occur. Multivariate analyses should be used and confounding factors included in order to minimize the risk of reaching wrong conclusions. Surely, there may be always other confounding factors which are unknown, but a proper discussion of this problem is needed when such strong claims are made as the authors do in their title.

Third, Arnold and Zink (2011) also calculated the power of their analysis to detect effects and concluded that their analysis had a very high power to detect even partial effects. This reasoning is false. It is not possible to detect partial effects with high statistical power. This follows from the definition of a partial effect which is the regression coefficient that one would expect if all the other variables in the regression equation had been held constant experimentally (e.g. Sokal and Rohlf 1995). Thus, partial effects can only be detected when other effects on population changes are taken into account in the analyses. Furthermore, there are measurement errors along both axes (each species-specific population trend has an error, as has each collision vulnerability index), and these were not taken into account in the analysis. If measurement errors are included, the power would decline dramatically. Finally, even if the statistical power of the Arnold and Zink analysis is high, it does not prevent from the problem of a correlative study that causation cannot be inferred.

Lastly, we have serious reservations about an analysis that lumps all species: we do not think that this is meaningful for conservation management. Even if a sound analysis could show that there is no relationship between population trends and collision mortality across all species, this would not necessarily mean that no group of species, no single species or no population of one or some species is affected. Also, we would usually worry less about effects in such species as, say, introduced European starlings (Sturnus vulgaris) than on rare and declining native species, such as, say, Bachman's warbler (Vermivora bachmanii). Arnold and Zink (2011) do not make the claim that no species or no individual population is affected, but we fear that stakeholder interested in building obstacles like towers, wind turbines or electric power lines could use this line of argumentation to avoid costly mitigation measures and to reduce the risk of not getting the permission for the construction. The management goal must be to avoid losing more species or affecting their populations negatively by the construction of such obstacles. Hence, assessments must necessarily be species-specific or even population-specific. As an analogy, consider the management of hunting pressure in game management. No sensible person would want to know whether hunting has an effect on the population trends across all species and across an entire area of North America. Rather, we want to know how strongly harvesting affects a particular population of each species and then define regulations based on that. It may be true that habitat loss and fragmentation are the main reasons for the negative population trends in many species and that collisions of birds with towers, buildings, wind turbines or electric power lines are of lesser importance to determine population trajectories. However, this does not mean that no species or no population is affected by this factor.

Moreover, collision risks are rapidly increasing in importance. Careful comparative analyses can be insightful to obtain general conclusions averaged over many species, but we think that in the case of the effects of collisions on bird populations this is not what is needed. It is risky to make general and strong claims about the effect across all species, since we may buy the loss of some species or populations with the wellbeing of others. We therefore call for sound species-specific population assessment of the potential impact of obstacles and question the usefulness of such general analyses in this case.

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