# 1 Poor availability of context-specific evidence hampers decision-

### 2 making in conservation

- 3
- 4 Authors:
- 5 Alec P. Christie<sup>1</sup> (Corresponding author: apc58@cam.ac.uk),
- 6 Tatsuya Amano<sup>1,3</sup>,
- 7 Philip A. Martin<sup>1,2</sup>,
- 8 Silviu O. Petrovan<sup>1</sup>,
- 9 Gorm E. Shackelford<sup>2</sup>,
- 10 Benno I. Simmons<sup>1,4,5</sup>,
- 11 Rebecca K. Smith<sup>1</sup>,
- 12 David R. Williams<sup>6</sup>,
- 13 Claire F. R. Wordley<sup>1</sup> and
- 14 William J. Sutherland<sup>1,2</sup>.
- 15
- 16 Affiliations:
- 17 <sup>1</sup>Conservation Science Group, Department of Zoology, University of Cambridge, The David
- 18 Attenborough Building, Downing Street, Cambridge CB3 3QZ, UK.
- 19 <sup>2</sup>BioRISC, St. Catharine's College, Cambridge CB2 1RL, UK.
- <sup>3</sup>School of Biological Sciences, University of Queensland, Brisbane, 4072 Queensland,
   Australia.
- <sup>4</sup>Department of Animal and Plant Sciences, University of Sheffield, Sheffield, S10 2TN United
   Kingdom.
- <sup>5</sup>Centre for Ecology and Conservation, College of Life and Environmental Sciences, University of Exeter, Penryn, UK
- <sup>6</sup>Sustainability Research Institute, School of Earth and Environment, University of Leeds. LS2
   9JT United Kingdom
- 28
- 29 Running title: Lack of relevant evidence limits conservation
- 30
- 31 Keywords: conservation evidence, conservation intervention, evidence-based conservation,
- 32 external validity, generalizability, local context, prioritization, relevant evidence, study design,
- 33 synthesis
- 34
- 35
- 36
- 37
- 38 39
- 40
- 40 11
- 41
- 42
- 43

#### 44 Abstract

45

46 Evidence-based conservation relies on robust and relevant evidence. Practitioners often prefer locally relevant studies whose results are more likely to 47 48 be transferable to the context of planned conservation interventions. To quantify 49 the availability of relevant evidence for amphibian and bird conservation we reviewed Conservation Evidence, a database of quantitative tests of 50 conservation interventions. Studies were geographically clustered and found at 51 extremely low densities - fewer than one study was present within a 2,000 km 52 53 radius of a given location. The availability of relevant evidence was extremely low 54 when we restricted studies to those studying biomes or taxonomic orders containing high percentages of threatened species, compared to the most 55 frequently studied biomes and taxonomic orders. Further constraining the 56 57 evidence by study design showed that only 17-20% of amphibian and bird studies used robust designs. Our results highlight the paucity of evidence on the 58 effectiveness of conservation interventions, and the disparity in evidence for local 59 contexts that are frequently studied and those where conservation needs are 60 61 greatest. Addressing the serious global shortfall in context-specific evidence requires a step change in the frequency of testing conservation interventions, 62 greater use of robust study designs and standardized metrics, 63 and 64 methodological advances to analyze patchy evidence bases.

65

- 66
- 67 68
- 69 70

## 84 Introduction

85

Tackling the biodiversity crisis with limited resources requires efficient and effective 86 87 conservation action (Dirzo et al., 2014; Sutherland, Pullin, Dolman, & Knight, 2004). To inform which conservation actions ('interventions') are effective and which are not, we need a large and 88 89 robust evidence base, ideally including large numbers of studies (replication of evidence; Fig.1A) with high internal validity (quality; Fig.1A) and external validity (relevance; Fig.1A). 90 91 However, the limited resources available for conservation research mean that the evidence 92 base for conservation is geographically and taxonomically biased (Christie, Amano, Martin, 93 Petrovan, et al., 2019; Fazey, Fischer, & Lindenmayer, 2005; Hickisch et al., 2019; Spooner, 94 Smith, & Sutherland, 2015). This is likely to limit the quality and relevance of evidence and impair effective decision-making (Cook, Possingham, & Fuller, 2013). Quantifying the availability 95 of relevant, reliable studies is necessary to understand the strength of evidence upon which 96 97 decisions are made, and to prioritize research on the effectiveness of conservation 98 interventions.

99 Practitioners and policymakers typically prefer to base their decisions on studies that are relevant (i.e., with high external validity; Fig.1) to their local context (Addison, Cook, & de Bie, 100 101 2016; Cook & Sgrò, 2017; Geijzendorffer et al., 2017). Using context-specific studies as 102 evidence helps to ensure that results are likely to be repeated if the intervention is implemented 103 again. The relevance of conservation studies to a given context will span multiple dimensions, 104 including: (i) bioclimatic (i.e., similarity between habitats or regions); (ii) taxonomic/functional 105 (i.e., similarity between taxa in terms of ecological function or taxonomic groups); and (iii) which 106 metric was used to quantify the effectiveness of an intervention (i.e., the response variables or 107 metrics of interest; Fig.1B). Other dimensions may also be important, such as the similarity 108 between a study's and a practitioner's socioeconomic and political contexts, but we focus on the 109 three dimensions above.

110

bioRxiv preprint doi: https://doi.org/10.1101/2020.02.13.946954; this version posted February 14, 2020. The copyright holder for this preprint (which was not certified by peer review) is the author/funder. All rights reserved. No reuse allowed without permission.



#### 112

Figure 1 - Framework of the desirable aspects of an ideal evidence base (stronger colors = more desirable). Fig.1A shows the three major desirable factors that an evidence base should have; large replication of evidence that is highly reliable (high internal validity) and highly relevant (high external validity). Fig.1B refers to the three dimensions that we will focus on that influence the overall relevance of evidence: i) bioclimatic (e.g., the study system), ii) taxonomic/functional (the study taxa) and iii) effectiveness measure (how you define and measure conservation success).

The first of these dimensions - bioclimatic relevance - refers to the similarity between the study ecosystem and the practitioner's ecosystem (Fig.1B). The second dimension taxonomic/functional relevance - concerns the similarity between the focal taxa of a study and the taxa of interest to the practitioner (Fig.1B). Together, these determine the ecological similarity between study and practitioner local contexts. This is vital because responses to interventions will vary between ecosystems and taxa. For example, the effectiveness of artificial nest boxes varies between different countries and habitats (Finch et al., 2019), while the effectiveness of translocation for New Zealand robins (*Petroica australis*) is unlikely to be relevant to a practitioner translocating Kakapo (*Strigops habroptila*). Practitioners who are interested in broader functional groups (e.g., seed dispersers or pollinators), taxa (e.g., birds, amphibians), or even whole ecosystems, may focus more on the functional relevance rather than taxonomic similarity of studied species.

132 The third dimension of relevance is the metric used to measure the effectiveness of an 133 intervention. Practitioners may be interested in different responses to interventions depending 134 on their focus (e.g., species or ecosystem-level responses) and effectiveness may vary 135 depending on the metric used (Capmourteres & Anand, 2016; Marshall, Wintle, Southwell, & 136 Kujala, 2019). For example, at the ecosystem-level, the effectiveness of bird boxes may be 137 measured using the species richness or diversity of birds using them (Caine & Marion, 1991). 138 while at the species-level, the number of individuals (Brawn & Balda, 1988), fledglings (Male, 139 Jones, & Robertson, 2006; Purcell, Verner, & Lewis W, 1997), or brood size (Browne, 2006) 140 may be measured. Similarly, the effectiveness of road mitigation interventions (e.g., tunnels or 141 bridges) may be measured by the numbers of individuals of different species using the 142 structures, but could also be measured in terms of levels of road mortality (Helldin & Petrovan, 143 2019). Therefore, the type of metric used by studies to measure effectiveness can have a major 144 influence on the relevance of evidence.

145 The reliability of an evidence base - the internal validity of its studies - ultimately determines the 146 overall quality of the evidence base and depends to a large extent on study design (Christie, 147 Amano, Martin, Shackelford, et al., 2019; De Palma et al., 2018; Spake & Doncaster, 2017). As 148 the conservation evidence base contains a wide variety of study designs (De Palma et al., 149 2018), there is likely to be variation in the reliability of inferences that can be drawn (Christie, 150 Amano, Martin, Shackelford, et al., 2019). This variation may lead scientists to make misleading 151 recommendations to practitioners, ultimately reducing the effectiveness of conservation 152 practice, and making it difficult for decision-makers to weigh the strength of evidence provided 153 by different studies.

154 The replication of evidence - the number of studies in the evidence base - is also important as 155 greater numbers of studies demonstrating repeatable and reproducible effectiveness will give us 156 greater confidence in the overall strength of the evidence. Decision-makers should rightly be 157 wary of basing decisions on a low number of studies where reproducible effectiveness has not 158 been or cannot be demonstrated - particularly given the current reproducibility crisis (Begley & 159 Ioannidis, 2015; Nosek & Errington, 2017; Open Science Collaboration, 2015). However, the 160 overall number of studies is not the only indicator of the strength of the evidence, since studies 161 with low internal validity (e.g., poor study designs) and/or external validity (i.e., low relevance) 162 may not constitute reliable evidence. Currently, we have a poor quantitative understanding of 163 the availability of relevant and reliable studies in the conservation literature.

164 In this study, we assess whether studies testing conservation interventions are distributed 165 across different contexts (bioclimatically, taxonomically, and by the metric used to measure 166 effectiveness) in ways that reflect the needs of conservation. We also quantify other desirable aspects of the evidence base for conservation in terms of the quantity and quality of available
studies; i.e., the number of studies that have tested different conservation actions, and how
many of these use robust study designs.

170

#### 171 Methods

172

#### 173 Conservation Evidence database

174

175 We assessed the availability of relevant evidence for conservation practice using Conservation 176 Evidence, a database of 5.525 publications as of January 2020 (Conservation Evidence, 2020a) 177 that have quantitatively assessed the effectiveness of conservation interventions. Interventions 178 are defined as management actions that a practitioner may undertake to benefit biodiversity 179 (see Sutherland et al. (2019) for detailed methods). When we refer to the number of studies per 180 intervention, we refer to the number of different tests of interventions - single publications may 181 report multiple tests of different interventions. We assessed the availability of evidence for 182 amphibians and birds based on synopses compiled in 2014 (n=419 studies; Smith & 183 Sutherland, 2014) and 2012 (n=1,232 studies; Williams et al., 2013), respectively. More recent 184 publications will obviously have increased the evidence base, but the broad patterns we quantify 185 are unlikely to have changed in the intervening years. We excluded meta-analyses or 186 systematic reviews from our analyses as these typically cannot be attributed to a particular local 187 context (e.g., biome or taxon). We also only included interventions for which studies were 188 present in the database. Since 32% (n=33) of interventions for amphibians and 25% (n=80) of 189 interventions for birds had no associated studies in the database (i.e., were untested or tests 190 were unpublished) or only included reviews or meta-analyses, the following analyses are likely 191 to be an optimistic assessment of the availability of evidence in conservation. We used R 192 statistical software version 3.5.1 (R Core Team, 2019) for all analyses.

- 193
- 194

196

#### 195 Local availability of studies by geographical distance

197 To calculate the average availability of studies within a certain distance of a given practitioner's 198 location, we generated 1,000 regularly spaced coordinates across certain parts of the world. For 199 amphibians, we spaced these coordinates over the combined extent of all amphibian species 200 ranges (IUCN, 2019) as this represents the possible range of locations in which a practitioner 201 might conduct an intervention to conserve amphibians. For birds, we spaced these coordinates 202 across the world's terrestrial land masses (using "OpenStreetMap" 2019; see Appendix S1 for 203 maps of coordinates) since although the combined distribution of all bird species is almost 204 global, most practitioners are likely to conduct interventions to conserve birds terrestrially. 205 Although non-terrestrial interventions are carried out by practitioners, the vast area covered by 206 the ocean would severely underestimate the availability of studies to a practitioner's likely 207 location. 19 non-terrestrial interventions for birds were found in the database (e.g., 'use 208 streamer lines to reduce seabird bycatch on longlines' or 'use high-visibility mesh on gillnets to 209 reduce seabird bycatch') containing 33 studies in total - these were still included in our analysis as these studies tended to be conducted within close proximity to a terrestrial landmass (i.e.,coastal).

212

213 We then calculated the Great Circle Distance from each study to each coordinate (see Appendix 214 S1 for details), binning distances into a series of categories (100 km, 1,000 km and then every 215 1,000 km up to and including 19,000 km). We also calculated the 'Global Mean', which is the 216 mean number of studies per intervention in the entire database - equivalent to approximately 217 20,000 km at the equator, the maximum distance separating any two coordinates. We then 218 calculated the mean number of studies within each distance bin across all coordinates, as well 219 as the number of studies that used different categories of study designs: i) any design, ii) 220 Before-After (BA), Control-Impact (CI), Before-After Control-Impact (BACI) or Randomized 221 Controlled Trial (RCT); iii) CI, BACI or RCT; iv) BACI or RCT designs (see Methods in Christie, 222 Amano, Martin, Petrovan, et al. 2019 for definitions of each design).

223

224 We then repeated this analysis using the same number of coordinates (n=1,000), but this time 225 by randomly selecting coordinates from amphibian and bird studies in the database (sampling 226 with replacement from amphibian studies as there were fewer than 1,000). Using both 227 approaches provided likely upper and lower bounds of evidence availability: regular coordinates 228 likely underestimated the availability of evidence to practitioners, giving equal weighting to 229 locations where conservation interventions are unlikely to occur (e.g., Antarctica) and those that 230 are more intensively managed (e.g., Europe). In contrast, using locations from existing 231 publications will likely overestimate study availability as this assumes that practitioners only 232 conduct interventions in locations where they have previously been tested.

233

We compared the results of the first analysis (regularly spaced coordinates) to the expected patterns we would observe if studies were regularly distributed. We did this by generating equal numbers of regularly spaced coordinates ('expected studies') as the number of amphibian and bird studies (419 and 1,232 coordinates, respectively) using the same methods and shapefiles as before. We then calculated the mean number of these 'expected studies' within each distance bin.

240

#### 241 Context-specific availability of studies

242

243 To quantify the amount of relevant and robust evidence on the effectiveness of different 244 conservation interventions, we required metadata that described each study's local context and 245 study design. By adapting previously described methods (Christie, Amano, Martin, Petrovan, et 246 al. 2019; Appendix S2), we extracted the biome, taxonomic order and reported metric type used 247 by each study (to quantify the number of relevant studies), as well as the broad category of 248 study design used (to quantify the number of robustly designed studies). When metric metadata 249 was extracted, we grouped similar metrics into the following nine metric types: count-based, 250 diversity, activity-based, physiological, survival, reproductive success, education-based, 251 regulation-based, and biomass (Appendix S2).

253 We quantified the number of studies per conservation intervention that met certain relevance 254 and study design criteria, to give an estimate of the availability of relevant and robust evidence. 255 To ensure that we did not artificially constrain the number of studies per intervention for different 256 subsets of studies (e.g., taxonomic order or biome), we grouped certain interventions that were 257 focused on single taxa or habitats but were fundamentally the same type of intervention (e.g., 258 'create ponds for newts' and 'create ponds for toads' would be grouped into 'create ponds'; see 259 Acknowledgements and Data for files describing these groupings). This resulted in a total of 71 260 and 226 interventions for amphibians and birds, respectively.

261

268

286

Using these interventions, we then undertook two analyses to quantify the availability of evidence under different scenarios: i) where we optimistically assume a given practitioner is interested in the most frequently studied local context; and ii) where we assume that a given practitioner is interested in local contexts in which a greater percentage of species are threatened (i.e., those classified as Vulnerable, Endangered or Critically Endangered status on the (IUCN, 2019) Red List).

269 The first analysis calculated the mean number of studies per intervention for both scenarios in 270 terms of three separate relevance criteria: biome, taxonomic order and metric. For the first 271 scenario we calculated the number of studies with the most frequently studied biome, order or 272 metric relative to each intervention. For the second scenario (to reflect conservation needs), we 273 calculated the number of studies with a randomly selected biome, taxonomic order or metric 274 from a weighted list (averaged over 1,000 repeated runs). This weighted list was generated so 275 that the probability of selection was determined by the percentage of species that are 276 threatened (i.e., those classified as Vulnerable, Endangered or Critically Endangered status on 277 the (IUCN, 2019) Red List) for each biome and taxonomic order, and the percentage usage of 278 each metric within each intervention in the database. We intersected shapefiles from the (IUCN, 279 2019) Red List with shapefiles of the world's terrestrial biomes (Dinerstein et al., 2017) to 280 determine the proportion of threatened species in each biome. We assumed that interventions 281 could be tested by studies in any biome and on any taxonomic order - this will likely mean that 282 our estimates for the second scenario are underestimates of study availability, for example, as certain interventions are unlikely to be conducted in certain biomes. However, we grouped 283 284 interventions so they were not defined as taxon or habitat-specific and used coarse criteria 285 (biome and taxonomic order) to limit this underestimation.

287 For the second analysis, we used a stepwise process to calculate the number of studies that 288 met one or more of the relevance criteria - only carrying forward studies if they met all previous 289 criteria. For example, considering the first scenario (most frequently studied context), we 290 counted the number of studies featuring the most frequently studied biome, then studies 291 featuring the most frequently studied biome AND taxonomic order, and then studies featuring 292 the most frequently studied biome AND taxonomic order AND metric. We also repeated this for 293 all possible orderings of biome, taxonomic order and metric (Fig.3 and Figs.S1-S5), as well as 294 for the second scenario (weighting towards biomes and taxonomic orders with greater 295 percentages of threatened species). Taxonomic orders could only be selected if at least one 296 species in that order was present in the previously selected biome - we determined which

orders were present in each biome by intersecting shapefiles from the (IUCN, 2019) Red List with shapefiles of terrestrial biomes (Dinerstein et al., 2017). The same was true for biomes when taxonomic order was the first relevance criteria to be selected (i.e., only biomes where that taxonomic order is present could be selected). In the final step, we also calculated the number of studies that used different categories of study designs (any design; BA, CI, BACI or RCT; CI, BACI or RCT; BACI or RCT).

303 304

#### 305 **Results**

306

307 We considered a total of 71 and 226 interventions for amphibians and birds (mean = 7.9 and 6.9 308 studies per intervention; Fig.2), respectively, that contained at least one study. Studies were not 309 evenly distributed geographically; the mean number of amphibian and bird studies per 310 intervention (black large circles in Fig.2) deviated, particularly for amphibians, from what we 311 would have expected if the same number of studies were regularly distributed (orange triangles 312 in Fig.2). On average, there was less than one study per intervention available within 2,000km 313 from a given regular point. When restricting analyses to more robust designs, the availability of 314 studies decreased substantially, with a higher proportion of amphibian studies using BA 315 designs, compared to birds, but a smaller proportion using CI (see drop-offs from orange to 316 blue, and blue to green lines, respectively; Fig.2).

317

When considering distance of studies to randomly selected study coordinates, the mean number of studies per intervention generally declined more gradually compared to a regular grid of coordinates (Fig.2), implying that studies are clustered in space. At distances below 5,000km these differences were particularly pronounced; for example, on average, 2.2 amphibian studies and 1.5 bird studies were within 2,000km of a random study coordinate, compared to only 0.3 amphibian studies and 0.2 bird studies within 2,000km of regularly spaced coordinate. This suggests that studies are slightly more clustered for amphibians than birds.





328 Figure 2 - The mean number of amphibian and bird studies per intervention using different study 329 designs found within a certain distance of different sets of coordinates. The maximum distance 330 that a study can be is shown on the x axis, starting with the Global Mean (mean number of 331 studies per intervention considering all studies in the database) and decreasing to a distance of 332 100 km. Regular coordinates (large circle, thick line) show the mean number of studies within a 333 certain distance from a set of regularly distributed coordinates. Expected coordinates (orange triangle) mimic how the availability of studies would be expected to change if studies were 334 335 regularly distributed (this is only shown for studies using any study design). Random Study 336 coordinates (small circle, thin line) show the mean number of studies within a certain distance 337 from a set of randomly selected coordinates where previous studies have been conducted.

340 The mean number of studies per intervention was substantially greater for the most frequently 341 studied biome (Amphibians: 5.0; Birds: 3.5), relative to each intervention, compared to biomes 342 with higher percentages of species that are threatened (Amphibians: 0.4; Birds: 0.4; Fig.3). 343 Similarly, the mean number of studies per intervention was substantially greater for the most 344 frequently studied order in each intervention (Amphibians: 7.2; Birds: 4.4), compared to a 345 taxonomic orders with higher percentages of species that are threatened (Amphibians: 0.4; 346 Birds: 0.01; Fig.3). There was a smaller difference in the mean number of studies per 347 intervention between studies that used the most frequently used metric (Amphibians: 5.2; Birds: 348 4.8), relative to each intervention, and studies that used a randomly selected metric from within 349 each intervention (Amphibians: 4.5; Birds: 3.9; Fig.3). The mean numbers of biomes, taxonomic 350 orders and metrics per intervention were 2.7, 2.6, and 3.1 for amphibians, respectively, and 2.4, 351 6.1, and 2.6 for birds, respectively.





355

Figure 3 - Mean number of studies per intervention when studies were counted based on whether they considered the most frequently studied biome, metric or order, and whether they considered a randomly selected biome, metric or taxonomic order from a weighted list. These weightings were based on the proportion of threatened species found in each biome or taxonomic order. 'All' indicates the mean number of studies per intervention when considering all studies.

362

The mean number of studies per intervention was also greater when we constrained by the most frequently studied biome, taxonomic order and metric in a stepwise process Fig.4A), compared to biomes and taxonomic orders with higher percentages of threatened species (Fig.4B). When we constrained by the most frequently studied biome, taxonomic order and metric, the greatest proportional decrease in the number of studies occurred once we further constrained by study design, by only counting studies using robust BACI or RCT designs (on average, ~20% of amphibian studies and ~17% of bird studies that had met all previous criteria;
 Fig.4A). When we constrained by biomes and taxonomic orders with higher percentages of
 threatened species, the greatest proportional decreases occurred when constraining by
 taxonomic order, most notably for birds, and by biome (Fig.4B).

The sequence in which criteria were applied did not substantially affect the magnitude of the decrease in the number of studies - e.g., when biome was selected before or after taxonomic order and metric (Supporting Information Fig.S1-5). The overall decrease in studies from applying all relevance criteria (biome, taxonomic order and metric) was similarly severe regardless of the sequence in which the criteria were applied (Supporting Information Fig.S1-5). For all sequences, constraining the evidence to studies that used robust BACI or RCT designs reduced the mean number of studies to less than one study after constraining by the most frequently studied biome, taxonomic order and metric (Fig.4A; Supporting Information Fig.S1-5). Doing the same after instead constraining by the biomes and taxonomic orders with higher percentages of threatened species reduced the mean number of studies to fewer than 0.01 studies with BACI or RCT designs (Fig.4B; Supporting Information Fig.S1-5).

bioRxiv preprint doi: https://doi.org/10.1101/2020.02.13.946954; this version posted February 14, 2020. The copyright holder for this preprint (which was not certified by peer review) is the author/funder. All rights reserved. No reuse allowed without permission.



397

398 Figure 4 - Mean numbers of amphibian and bird studies per intervention when only considering 399 studies that meet certain relevance criteria. In panel A, studies with the most frequently studied 400 biome, taxonomic order and metric relative to each intervention were counted - here we assume 401 practitioners are interested in the most frequently studied local context. At each step (left to 402 right) we add a further criterion, carrying forward relevant studies from the previous step - for 403 example, only studies conducted in the most frequently studied biome were carried forward into 404 the biome and order category. In panel B, studies with a selected biome, taxon and metric were 405 counted (y axis has a square root transformation). Here we assume practitioners are more likely 406 to be interested in: biomes that are inhabited by higher proportions of threatened species; 407 taxonomic orders that have higher relative proportions of threatened species; and metrics that 408 are most frequently used within each intervention. At the final step, studies are counted based 409 on the study design they use (see Methods for details of study designs).

#### 410 **Discussion**

#### 411

412 Our work demonstrates that not only is there a general paucity of studies testing conservation 413 interventions, but that the distribution of these studies does not reflect conservation needs. 414 Specifically, there is a lack of studies testing conservation interventions in biomes and for 415 taxonomic orders containing high percentages of threatened amphibian and bird species. Given 416 substantial declines of bird fauna (Rosenberg et al., 2019) and severe threats to amphibians 417 (Grant, Muths, Schmidt, & Petrovan, 2019), a better understanding of the effectiveness of 418 interventions targeting threatened species is urgently required. Furthermore, a given decision-419 maker is likely to struggle to find robust studies addressing their local context. Addressing this 420 deficit will be challenging, but there are several possible ways to improve the evidence base for 421 conservation.

422

423 A fundamental problem that needs to be overcome in the long-term is the lack of studies testing 424 conservation interventions. Williams, Balmford, & Wilcove (in review) found that only 15% of 425 studies from a representative sample of the conservation literature tested interventions. 426 Evaluation of interventions should become mainstream, both as a topic of academic research 427 and as an activity for on-the-ground conservationists (Baylis et al., 2016). The publication of 428 these tests, whether the results are positive, negative, or neutral, is critical to building a strong 429 evidence base for conservation (Catalano, Lyons-White, Mills, & Knight, 2019). Current efforts 430 to facilitate this include the Applied Ecology Resources repository (British Ecological Society, 431 2020), 'Evidence' articles in the journal Conservation Science and Practice (Society for 432 Conservation Biology, 2020), and the journal Conservation Evidence (Conservation Evidence, 433 2020b).

434

435 Simply publishing more tests of conservation interventions, even at an increasing rate, is 436 however unlikely to solve the paucity of locally relevant studies. For example, even though 437 adding 1,000 studies testing interventions on birds would increase the mean number of studies 438 to approximately 11 studies across the current 226 interventions, these studies would still be 439 spread thin across a myriad of local contexts where the need for conservation is often not the 440 greatest (see also Wilson et al., 2016). Although Reboredo Segovia, Romano, & Armsworth 441 (2020) suggest that the number of general conservation studies in tropical locations correlates 442 with the number of threatened species, the results of this study and (Christie, Amano, Martin, 443 Petrovan, et al., 2019) suggest this is not the case for conservation studies testing interventions. 444 Therefore, we need concrete solutions enabling conservationists to generate and collate more 445 experimental evidence on the effectiveness of conservation interventions in underrepresented 446 locations and on underrepresented taxa (Christie, Amano, Martin, Petrovan, et al., 2019; 447 Donaldson et al., 2016; Murray, Green, Williams, Burfield, & de Brooke, 2015). For example, 448 funders, principal investigators and heads of conservation organizations need to enhance and 449 prioritize funding to test interventions in underrepresented areas. Evidence synthesis also needs 450 to incorporate more evidence from non-English language and grey literature publications to help 451 address underrepresented local contexts (Amano, González-Varo, & Sutherland, 2016; Amano 452 & Sutherland, 2013) - for example, publications from over 317 non-English language journals 453 are starting to be added to the Conservation Evidence database through the Transcending

Language Barriers to Environmental Sciences project (TRANSLATE, 2020). Making concerted efforts to acquire grey literature from organizations and groups outside academia will also be important.

457

458 The low proportion of studies using robust study designs, regardless of their relevance to a local 459 context, is also challenging. That more robustly designed studies are concentrated in North 460 America, Europe and Australia also compounds earlier taxonomic and biogeographical biases 461 (Christie, Amano, Martin, Petrovan, et al., 2019). If few robustly designed studies are available 462 for informing conservation, decision-makers may have to consider a wider range of studies that 463 may be less robust or relevant, potentially reducing the effectiveness of decision-making and 464 future practice (Slavin, 1995; Tugwell & Haynes, 2006; Whittaker, 2010). To increase the quality 465 of studies available for decision-making, we must recognize that the quality of studies testing 466 interventions may be limited in different ways. Studies evaluating mitigation efforts are often not 467 constrained by cost, but rather by short timescales and their focus on meeting legislative 468 requirements (for example, conserving legally protected species). Studies testing non-mitigation 469 interventions will likely be more constrained by cost, as well as short timescales (e.g., PhD 470 funding). Acknowledging how real-world constraints affect the choice of study design is 471 essential to devising approaches to improving the evidence base for conservation. While better 472 training of early career scientists, consultants and researchers in appropriate study designs for 473 causal inference may help, ultimately more regulatory and funder-led measures (e.g., requiring 474 grantees to demonstrate rigorous study design) will be required (De Palma et al., 2018; Grant et 475 al., 2019).

476

477 Given the general lack of evidence across conservation, there is also a need to use a 478 standardized set of metrics to evaluate conservation effectiveness (McQuatters-Gollop et al., 479 2019). Using a diversity of metrics may be necessary to assess multiple important aspects of an 480 intervention's effectiveness, but a lack of consistency in the metrics used to report results often 481 makes the evidence base difficult to synthesize - especially if different metrics yield different 482 results (Mace & Baillie, 2007). Prioritisation of the most relevant metrics of effectiveness for 483 different interventions with input from decision-makers and practitioners is essential to facilitate 484 inter-study comparisons (McQuatters-Gollop et al., 2019). Initiatives aiming to do this are 485 underway in topics such as fishery habitats (Lederhouse & Link, 2016) and protected areas 486 (Nolte & Agrawal, 2013; Pomeroy, Parks, & Watson, 2004), and are supported by the Essential 487 Biodiversity Variables framework (Jetz et al., 2019). Funders could help strengthen these efforts 488 by requiring grantees to follow such initiatives and use consistent metrics when evaluating 489 interventions.

490

Increasing the size and quality of the evidence base for conservation decision-making will be a slow process, but conservation practitioners need to make decisions now. Until the evidence base improves, excluding studies from evidence syntheses because they do not meet certain quality or relevance criteria could lead to little or no evidence being used to inform conservation efforts (Davies & Gray, 2015; Gurevitch & Hedges, 1999; Lortie, Stewart, Rothstein, & Lau, 2015). Moreover, studies that do not meet these criteria may still provide useful evidence, 497 particularly in the absence of more relevant and robust studies (Burivalova et al., 2019; Cook,
498 Mascia, Schwartz, Possingham, & Fuller, 2013; Gough & White, 2018).

499

500 Therefore, we need novel approaches to rigorously synthesizing studies that vary considerably 501 in their relevance and robustness to maximize the use of the current imperfect evidence base. 502 We believe that weighting approaches in both quantitative meta-analyses and more qualitative 503 evidence synthesis would help maximize the number of studies available, while giving greater 504 influence to studies with desirable characteristics. This could involve giving greater influence to 505 more robustly designed studies (e.g., using accuracy weights from Christie, Amano, Martin, 506 Shackelford, et al. 2019 and evidence hierarchies from Mupepele, Walsh, Sutherland, & 507 Dormann 2016), and giving more weight to more relevant studies (e.g., weighting by the 508 relevance of studies to a decision-maker's local context, as proposed in healthcare by Kneale, 509 Thomas, O'Mara-Eves, & Wiggins 2019). To generate objective weights of study relevance that 510 reflect the likely generalizability of study results, we need studies which help us to understand 511 how generalizability varies between interventions for different ecological (e.g., artificial nest 512 boxes; Finch et al. 2019), socioeconomic, and political contexts. Understanding why some 513 interventions work in certain contexts and not others is fundamentally important for evidence-514 based decision-makers (Grant et al., 2019).

515

516 Overall, we have shown that the evidence base for conservation does not reflect the needs of 517 conservation. When this is combined with the general paucity of robust studies testing 518 conservation interventions, we conclude that there is a serious lack of locally relevant and 519 robust studies to inform decision-making in conservation. We hope that the conservation 520 community can work together to improve the state of the conservation evidence base. Doing so 521 will require much greater collaboration between research and practice. Testing interventions 522 needs to become more routine, use a more standardized suite of metrics and robust study 523 designs, and, most importantly, focus on the locations and taxa where evidence is most needed 524 to inform conservation action. In the meantime, we need to explore ways to better analyze the 525 current patchy evidence base of conservation and ensure that we can support the shift towards 526 more evidence-based policy and practice.

527

#### 528 Acknowledgements and Data

529 We would like to thank Anne Mupepele for their useful comments on the manuscript and all past 530 and present members of the Conservation Evidence project. All data analyzed in this study and 531 code to repeat analyses are available from https://doi.org/10.5281/zenodo.3634780.

532

#### 533 Author funding sources

TA was supported by the Grantham Foundation for the Protection of the Environment, the
Kenneth Miller Trust and the Australian Research Council Future Fellowship (FT180100354);
WJS, PAM, CFRW, SOP and GES are supported by Arcadia and The David and Claudia

537 Harding Foundation; RKS was supported by the MAVA Foundation; BIS and APC were 538 supported by the Natural Environment Research Council as part of the Cambridge Earth 539 System Science NERC DTP [NE/L002507/1]. BIS is also supported by the Natural Environment 540 Research Council [NE/S001395/1] and a Royal Commission for the Exhibition of 1851 Research

- 541 Fellowship.
- 542

#### References 543

544

561

563

567 568

569

- 1. Addison, P. F. E., Cook, C. N., & de Bie, K. (2016). Conservation practitioners' 545 perspectives on decision triggers for evidence-based management. Journal of 546 Applied Ecology, 53(5), 1351–1357. https://doi.org/10.1111/1365-2664.12734 547
- 548 Amano, T., González-Varo, J. P., & Sutherland, W. J. (2016). Languages Are Still 549 a Major Barrier to Global Science. PLOS Biology, 14(12), e2000933. Retrieved from https://doi.org/10.1371/journal.pbio.2000933 550
- 551 3. Amano, T., & Sutherland, W. J. (2013). Four barriers to the global understanding 552 of biodiversity conservation: wealth, language, geographical location and security. Proceedings of the Royal Society B: Biological Sciences, 280(1756), 553 20122649. 554
- 4. Baylis, K., Honey-ros, J., Corbera, E., Ezzine-de-blas, D., Ferraro, P. J., Jan, B., 555 556 ... Wunder, S. (2016). Mainstreaming Impact Evaluation in Nature Conservation 557 3. 9(February), 58–64. https://doi.org/10.1111/conl.12180
- 558 5. Begley, C. G., & Ioannidis, J. P. A. (2015). Review Reproducibility in Science 559 Improving the Standard for Basic and Preclinical Research. 116–126. 560 https://doi.org/10.1161/CIRCRESAHA.114.303819
- 6. Bivand, R., Keitt, T., & Rowlingson, B. (2019). rgdal: Bindings for the "Geospatial" 562 Data Abstraction Library R package version 1.4-8. Retrieved from https://cran.rproject.org/package=rgdal
- 564 7. Bivand, R., & Rundel, C. (2019). rgeos: Interface to Geometry Engine - Open 565 Source ('GEOS'). R package version 0.5-2. Retrieved from https://cran.r-566 project.org/package=rgeos%0A
  - 8. Bivand, R. S., Pebesma, E., & Gomez-Rubio, V. (2013). Applied spatial data analysis with R (Second). Retrieved from http://www.asdar-book.org/
  - 9. Brawn, J. D., & Balda, R. P. (1988). Population biology of cavity nesters in northern Arizona: do nest sites limit breeding densities? The Condor, 90(1), 61-71.
- 572 10. British Ecological Society. (2020). Applied Ecological Resources repository and 573 Ecological Solutions and Evidence journal. Retrieved January 10, 2020, from 574 https://besjournals.onlinelibrary.wiley.com/journal/26888319
- 575 11. Browne, S. J. (2006). Effect of nestbox construction and colour on the occupancy and breeding success of nesting tits Parus spp. Bird Study, 53(2), 187–192. 576
- 12. Burivalova, Z., Allnutt, T., Rademacher, D., Schlemm, A., Wilcove, D. S., & 577 Butler, R. A. (2019). What works in tropical forest conservation, and what does 578 not: Effectiveness of four strategies in terms of environmental, social, and 579 economic outcomes. Conservation Science and Practice, in press(March), 1–15. 580 581 https://doi.org/10.1111/csp2.28

582 13. Caine, L. A., & Marion, W. R. (1991). Artificial Addition of Snags and Nest Boxes to Slash Pine Plantations (Colocacion de maderos y cajas de anidamiento en 583 plantaciones de Pinus elliottii). Journal of Field Ornithology, 97–106. 584 585 14. Capmourteres, V., & Anand, M. (2016). "Conservation value": a review of the concept and its quantification. Ecosphere, 7(10), e01476. 586 587 https://doi.org/10.1002/ecs2.1476 588 15. Catalano, A. S., Lyons-White, J., Mills, M. M., & Knight, A. T. (2019). Learning 589 from published project failures in conservation. Biological Conservation, 238, 108223. https://doi.org/https://doi.org/10.1016/j.biocon.2019.108223 590 591 16. Christie, A. P., Amano, T., Martin, P. A., Petrovan, S. O., Shackelford, G. E., Simmons, B. I., ... Sutherland, W. J. (2019). The challenge of heterogeneous 592 evidence in conservation. BioRxiv, 797639. https://doi.org/10.1101/797639 593 594 17. Christie, A. P., Amano, T., Martin, P. A., Shackelford, G. E., Simmons, B. I., & Sutherland, W. J. (2019). Simple study designs in ecology produce inaccurate 595 estimates of biodiversity responses. Journal of Applied Ecology, 56(12), 2742-596 597 2754. https://doi.org/10.1111/1365-2664.13499 598 18. Conservation Evidence. (2020a). Conservation Evidence. Retrieved February 4, 2020, from www.conservationevidence.com 599 600 19. Conservation Evidence. (2020b). Conservation Evidence journal. Retrieved from 601 https://www.conservationevidence.com/collection/view 602 20. Cook, C. N., Mascia, M. B., Schwartz, M. W., Possingham, H. P., & Fuller, R. A. 603 (2013). Achieving Conservation Science that Bridges the Knowledge-Action 604 Boundary. Conservation Biology, 27(4), 669–678. 605 https://doi.org/10.1111/cobi.12050 606 21. Cook, C. N., Possingham, H. P., & Fuller, R. A. (2013). Contribution of 607 Systematic Reviews to Management Decisions. Conservation Biology, 27(5), 902-915. https://doi.org/10.1111/cobi.12114 608 609 22. Cook, C. N., & Sgrò, C. M. (2017). Aligning science and policy to achieve 610 evolutionarily enlightened conservation. Conservation Biology, 31(3), 501–512. 611 https://doi.org/10.1111/cobi.12863 23. Davies, G. M., & Gray, A. (2015). Don't let spurious accusations of 612 613 pseudoreplication limit our ability to learn from natural experiments (and other 614 messy kinds of ecological monitoring). Ecology and Evolution, 5(22), 5295–5304. 615 https://doi.org/10.1002/ece3.1782 616 24. De Palma, A., Sanchez-Ortiz, K., Martin, P. A., Chadwick, A., Gilbert, G., Bates, A. E., ... Purvis, A. (2018). Challenges With Inferring How Land-Use Affects 617 Terrestrial Biodiversity: Study Design, Time, Space and Synthesis. In Next 618 Generation Biomonitoring: Part 1 (1st ed., pp. 163–199). Elsevier Ltd. 619 620 25. Dinerstein, E., Olson, D., Joshi, A., Vynne, C., Burgess, N. D., Wikramanayake, 621 E., ... Saleem, M. (2017). An Ecoregion-Based Approach to Protecting Half the 622 Terrestrial Realm. BioScience, 67(6), 534–545. 623 https://doi.org/10.1093/biosci/bix014 26. Dirzo, R., Young, H. S., Galetti, M., Ceballos, G., Isaac, N. J. B., & Collen, B. 624 625 (2014). Defaunation in the Anthropocene. Science, 345(6195), 401–406. 626 https://doi.org/10.1126/science.1251817 27. Donaldson, M. R., Burnett, N. J., Braun, D. C., Suski, C. D., Hinch, S. G., Cooke, 627

628	S. J., & Kerr, J. T. (2016). Taxonomic bias and international biodiversity
629	conservation research. FACETS, 1(1), 105–113. https://doi.org/10.1139/facets-
630	2016-0011
631	28. Fazey, I., Fischer, J., & Lindenmayer, D. B. (2005). What do conservation
632	biologists publish? Biological Conservation, 124(1), 63–73.
633	https://doi.org/https://doi.org/10.1016/j.biocon.2005.01.013
634	29. Finch. T., Branston, C., Clewlow, H., Dunning, J., Franco, A. M. A., Račinskis, E.,
635	Butler, S. J. (2019). Context-dependent conservation of the cavity-nesting
636	European Roller, Ibis, 161(3), 573–589, https://doi.org/10.1111/ibi.12650
637	30. Geijzendorffer, I. R., van Teeffelen, A. J. A., Allison, H., Braun, D., Horgan, K.,
638	Iturrate-Garcia, M., Quatrini, S. (2017). How can global conventions for
639	biodiversity and ecosystem services guide local conservation actions? Current
640	Opinion in Environmental Sustainability 29 145–150
641	31 Gough D & White H (2018) Evidence standards and evidence claims in web
642	based research portals Retrieved from https://uploads-
643	ssl webflow com/59f07e67422cdf0001904c14/5bfffe39daf9c956d0815519 CEHL
611	EVIDENCE STANDARDS REPORT V14 WER odf
6/5	32 Grant E H C Muths E Schmidt B R & Petrovan S O (2019) Amphibian
6/6	conservation in the Anthronocene <i>Biological Conservation</i> 236 5/3-5/7
647	bttps://doi.org/bttps://doi.org/10.1016/i biocon.2019.03.003
6/8	33 Gurevitch I & Hedges I V (1990) Statistical issues in ecological
640	mote analyses. Ecology $80(4)$ 1142 1140
049 650	$24$ Helldin L $\cap$ 8 Detroyon S $\cap$ (2010) Effectiveness of small read tunnels and
000	54. Heliulii, J. O., & Petrovali, S. O. (2019). Effectiveness of small road tunnels and
051	Sweden Dear 1, 7, a7549, https://doi.org/10.7717/pagri 7549
652	Sweden. PeerJ, 7, e7518. https://doi.org/10.7717/peerJ.7518
653	35. HICKISCH, R., HODGETTS, T., JOHNSON, P. J., SIIIERO-ZUDIRI, C., TOCKHER, K., &
654	Macdonaid, D. W. (2019). Effects of publication bias on conservation planning.
655	Conservation Biology, 33(5), 1151–1163. https://doi.org/10.1111/cobi.13326
656	36. Hijmans, R. J. (2017). geosphere: Spherical Trigonometry. R package version
657	1.5-7. Retrieved from https://cran.r-project.org/package=geosphere
658	37. IUCN. (2019). IUCN Red List. Retrieved November 12, 2019, from
659	https://www.iucnredlist.org/
660	38. Jetz, W., McGeoch, M. A., Guralnick, R., Ferrier, S., Beck, J., Costello, M. J.,
661	Merow, C. (2019). Essential biodiversity variables for mapping and monitoring
662	species populations. <i>Nature Ecology &amp; Evolution</i> , 3(4), 539–551.
663	39.Kneale, D., Thomas, J., O'Mara-Eves, A., & Wiggins, R. (2019). How can
664	additional secondary data analysis of observational data enhance the
665	generalisability of meta-analytic evidence for local public health decision making?
666	Research Synthesis Methods, 10(1), 44–56. https://doi.org/10.1002/jrsm.1320
667	40. Lederhouse, T., & Link, J. S. (2016). A Proposal for Fishery Habitat Conservation
668	Decision-Support Indicators. Coastal Management, 44(3), 209–222.
669	https://doi.org/10.1080/08920753.2016.1163176
670	41. Lortie, C. J., Stewart, G., Rothstein, H., & Lau, J. (2015). How to critically read
671	ecological meta-analyses. Research Synthesis Methods, 6(2), 124–133.
672	https://doi.org/10.1002/jrsm.1109
673	42. Mace, G. M., & Baillie, J. E. M. (2007). The 2010 biodiversity indicators:

674 675	challenges for science and policy. <i>Conservation Biology</i> , <i>21</i> (6), 1406–1413. 43. Male, S. K., Jones, J., & Robertson, R. J. (2006). Effects of nest-box density on
676	the behavior of Tree Swallows during nest building. <i>Journal of Field Ornithology</i> ,
677	77(1), 61–66.
678	44. Marshall, E., Wintle, B. A., Southwell, D., & Kujala, H. (2019). What are we
079	medsuming? A review of metrics used to describe biodiversity in onsets
680	exchanges. Biological Conservation, 108250.
681	nttps://doi.org/nttps://doi.org/10.1016/j.biocon.2019.108250
682	45. McQuatters-Gollop, A., Mitchell, I., Vina-Herbon, C., Bedford, J., Addison, P. F.
683	E., Lynam, C. P., Otto, S. A. (2019). From Science to Evidence – How
684	Biodiversity Indicators Can Be Used for Effective Marine Conservation Policy and
685	Management . <i>Frontiers in Marine Science</i> , Vol. 6, p. 109. Retrieved from
686	https://www.frontiersin.org/article/10.3389/fmars.2019.00109
687	46. Mupepele, AC., Walsh, J. C., Sutherland, W. J., & Dormann, C. F. (2016). An
688	evidence assessment tool for ecosystem services and conservation studies.
689	Ecological Applications, 26(5), 1295–1301. https://doi.org/10.1890/15-0595
690	47. Murray, H. J., Green, E. J., Williams, D. R., Burfield, I. J., & de Brooke, M. L.
691	(2015). Is research effort associated with the conservation status of European
692	bird species? Endangered Species Research, 27(3), 193–206.
693	https://doi.org/10.3354/esr00656
694	48. Nolte, C., & Agrawal, A. (2013). Linking Management Effectiveness Indicators to
695	Observed Effects of Protected Areas on Fire Occurrence in the Amazon
696	Rainforest, Conservation Biology, 27(1), 155–165, https://doi.org/10.1111/i.1523-
697	1739.2012.01930.x
698	49. Nosek, B. A., & Errington, T. M. (2017). Making sense of replications. <i>ELife</i> , 6.
699	e23383. https://doi.org/10.7554/eLife.23383
700	50. Open Science Collaboration, (2015). Estimating the reproducibility of
701	psychological science. Science .349(6251) aac4716
702	https://doi.org/10.1126/science.aac4716
703	51. OpenStreetMap. (2019). Retrieved December 14, 2019, from
704	http://openstreetmapdata.com/data/land-polygons
705	52. Pebesma, E. J., & Bivand, R. S. (2005). Classes and methods for spatial data in
706	R. R News, 5(2).
707	53. Pomeroy, R. S., Parks, J. E., & Watson, L. M. (2004). How is your MPA doing?: a
708	guidebook of natural and social indicators for evaluating marine protected area
709	management effectiveness. IUCN.
710	54. Purcell, K. L., Verner, J., & Lewis W. O. (1997). A comparison of the breeding
711	ecology of birds nesting in boxes and tree cavities. The Auk. 114(4), 646–656.
712	55. R Core Team. (2019). R: A language and environment for statistical computing.
713	R Foundation for Statistical Computing. Retrieved from https://www.r-project.org/
714	56 Reboredo Segovia A L. Romano D. & Armsworth P. R. (2020) Who studies
715	where? Boosting tropical conservation research where it is most needed
716	Frontiers in Ecology and the Environment fee 2146
717	https://doi.org/10.1002/fee.2146
718	57 Rosenberg K V Dokter A M Blancher P I Sauer I R Smith A C Smith
719	P. A., Marra, P. P. (2019). Decline of the North American avifauna. <i>Science</i> ,

720	eaaw1313. https://doi.org/10.1126/science.aaw1313
721	58. Slavin, R. E. (1995). Best evidence synthesis: An intelligent alternative to meta-
722	analysis. Journal of Clinical Epidemiology, 48(1), 9–18.
723	https://doi.org/10.1016/0895-4356(94)00097-A
724	59. Smith, R. K., & Sutherland, W. J. (2014). Amphibian conservation: global
725	evidence for the effects of interventions (Vol. 4). Pelagic Publishing Ltd.
726	60. Society for Conservation Biology. (2020). Conservation Science and Practice.
727	Retrieved January 10, 2020, from
728	https://conbio.onlinelibrary.wiley.com/journal/25784854
729	61. Spake, R., & Doncaster, C. P. (2017). Use of meta-analysis in forest biodiversity
730	research: key challenges and considerations. Forest Ecology and Management,
731	400, 429–437. https://doi.org/10.1016/j.foreco.2017.05.059
732	62. Spooner, F., Smith, R. K., & Sutherland, W. J. (2015). Trends, biases and
733	effectiveness in reported conservation interventions. Conservation Evidence, 12,
734	2–7.
735	63. Sutherland, W. J., Pullin, A. S., Dolman, P. M., & Knight, T. M. (2004). The need
736	for evidence-based conservation. <i>Trends in Ecology &amp; Evolution</i> , 19(6), 305–308.
737	64. Sutherland, W. J., Taylor, N. G., MacFarlane, D., Amano, T., Christie, A. P.,
738	Dicks, L. V, … Wordley, C. F. R. (2019). Building a tool to overcome barriers in
739	research-implementation spaces: The Conservation Evidence database.
740	Biological Conservation, 238, 108199.
741	https://doi.org/10.1016/j.biocon.2019.108199
742	65. TRANSLATE. (2020). TRANSLATE - Transcending Language Barriers to
743	Environmental Sciences. Retrieved January 11, 2020, from
744	https://researchers.uq.edu.au/research-project/35572
745	66. Tugwell, P., & Haynes, R. B. (2006). Assessing claims of causation. Clinical
746	Epidemiology: How to Do Clinical Practice Research, 356–387.
747	67. Whittaker, R. J. (2010). Meta-analyses and mega-mistakes: calling time on
748	meta-analysis of the species richness-productivity relationship. <i>Ecology</i> , 91(9),
749	2522–2533.
750	68. Williams, D. R., Balmford, A., & Wilcove, D. S. (n.d.). The past and future role of
751	conservation science in protecting biodiversity. In Conservation Letters.
752	69. Williams, D. R., Pople, R. G., Showler, D. A., Dicks, L. V, Child, M. F., Zu
753	Ermgassen, E. K. H. J., & Sutherland, W. J. (2013). Bird Conservation: Global
754	evidence for the effects of interventions (Vol. 2). Pelagic Publishing.
755	70. Wilson, K. A., Auerbach, N. A., Sam, K., Magini, A. G., Moss, A. S. L., Langhans,
756	S. D., Meijaard, E. (2016). Conservation Research Is Not Happening Where It
757	Is Most Needed. PLOS Biology, 14(3), e1002413. Retrieved from
758	https://doi.org/10.1371/journal.pbio.1002413