



HAL
open science

Quantifying and addressing the prevalence and bias of study designs in the environmental and social sciences

Alec P Christie, David Abecasis, Mehdi Adjeroud, Juan C Alonso, Tatsuya Amano, Alvaro Anton, Barry P Baldigo, Rafael Barrientos, Jake E Bicknell, Deborah A Buhl, et al.

► To cite this version:

Alec P Christie, David Abecasis, Mehdi Adjeroud, Juan C Alonso, Tatsuya Amano, et al.. Quantifying and addressing the prevalence and bias of study designs in the environmental and social sciences. Nature Communications, 2020, 11 (1), 10.1038/s41467-020-20142-y . hal-03054312

HAL Id: hal-03054312

<https://hal.science/hal-03054312>

Submitted on 11 Dec 2020

HAL is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

1 Quantifying and addressing the prevalence and bias of study 2 designs in the environmental and social sciences

3
4 Alec P. Christie*¹, David Abecasis², Mehdi Adjeroud³, Juan C. Alonso⁴, Tatsuya Amano⁵, Alvaro
5 Anton⁶, Barry P. Baldigo⁷, Rafael Barrientos⁸, Jake E. Bicknell⁹, Deborah A. Buhl¹⁰, Just Cebrian¹¹,
6 Ricardo S. Ceia^{12,13}, Luciana Cibils-Martina^{14,15}, Sarah Clarke¹⁶, Joachim Claudet¹⁷, Michael D.
7 Craig^{18,19}, Dominique Davoult²⁰, Annelies De Backer²¹, Mary K. Donovan^{22,23}, Tyler D. Eddy^{24,25,26},
8 Filipe M. França²⁷, Jonathan P.A. Gardner²⁶, Bradley P. Harris²⁸, Ari Huusko²⁹, Ian L. Jones³⁰,
9 Brendan P. Kelaher³¹, Janne S. Kotiaho^{32,33}, Adrià López-Baucells^{34,35,36}, Heather L. Major³⁷, Aki
10 Mäki-Petäys^{38,39}, Beatriz Martín^{40,41}, Carlos A. Martín⁸, Philip A. Martin^{1,42}, Daniel Mateos-Molina⁴³,
11 Robert A. McConnaughey⁴⁴, Michele Meroni⁴⁵, Christoph F.J. Meyer^{34,35,46}, Kade Mills⁴⁷, Monica
12 Montefalcone⁴⁸, Norbertas Noreika^{49,50}, Carlos Palacín⁴, Anjali Pande^{26,51,52}, C. Roland Pitcher⁵³,
13 Carlos Ponce⁵⁴, Matt Rinella⁵⁵, Ricardo Rocha^{34,35,56}, María C. Ruiz-Delgado⁵⁷, Juan J. Schmitter-
14 Soto⁵⁸, Jill A. Shaffer¹⁰, Shailesh Sharma⁵⁹, Anna A. Sher⁶⁰, Doriane Stagnol²⁰, Thomas R. Stanley⁶¹,
15 Kevin D.E. Stokesbury⁶², Aurora Torres^{63,64}, Oliver Tully¹⁶, Teppo Vehanen⁶⁵, Corinne Watts⁶⁶,
16 Qingyuan Zhao⁶⁷, William J. Sutherland^{1,42}

17
18 *Correspondence to: apc58@cam.ac.uk

19 ¹Conservation Science Group, Department of Zoology, University of Cambridge, The David
20 Attenborough Building, Downing Street, Cambridge CB3 3QZ, UK.

21 ²Centre of Marine Sciences (CCMar), Universidade do Algarve, Campus de Gambelas, 8005-139
22 Faro, Portugal

23 ³Institut de Recherche pour le Développement (IRD), UMR 9220 ENTROPIE & Laboratoire
24 d'Excellence CORAIL, Université de Perpignan Via Domitia, 52 avenue Paul Alduy, 66860 Perpignan,
25 France

26 ⁴Museo Nacional de Ciencias Naturales, CSIC, Madrid, Spain

27 ⁵School of Biological Sciences, University of Queensland, Brisbane, 4072 Queensland, Australia

28 ⁶Education Faculty of Bilbao, University of the Basque Country (UPV/EHU). Sarriena z/g E-48940
29 Leioa, Basque Country.

30 ⁷U.S. Geological Survey, New York Water Science Center, 425 Jordan Rd., Troy, NY USA 12180

31 ⁸Universidad Complutense de Madrid, Departamento de Biodiversidad, Ecología y Evolución,
32 Facultad de Ciencias Biológicas, c/ José Antonio Novais, 12, E-28040 Madrid, Spain

33 ⁹Durrell Institute of Conservation and Ecology (DICE), School of Anthropology and Conservation,
34 University of Kent, Canterbury, CT2 7NR, UK

35 ¹⁰U.S. Geological Survey, Northern Prairie Wildlife Research Center, Jamestown, ND USA 58401

36 ¹¹Northern Gulf Institute, Mississippi State University, 1021 Balch Blvd, John C. Stennis Space
37 Center, Mississippi, USA 39529

38 ¹²MARE – Marine and Environmental Sciences Centre, Dept. Life Sciences, University of Coimbra,
39 Portugal

40 ¹³CFE – Centre for Functional Ecology, Dept. Life Sciences, University of Coimbra, Portugal

41 ¹⁴Departamento de Ciencias Naturales, Universidad Nacional de Río Cuarto (UNRC), Córdoba,
42 Argentina

43 ¹⁵CONICET, Buenos Aires, Argentina

44 ¹⁶Marine Institute, Rinnville, Oranmore, Galway, Ireland

45 ¹⁷National Center for Scientific Research, PSL Université Paris, CRIOBE, USR 3278 CNRS-EPHE-
46 UPVD, Maison des Océans, 195 rue Saint-Jacques 75005 Paris, France

47 ¹⁸School of Biological Sciences, University of Western Australia, Nedlands, WA, Australia 6009

48 ¹⁹School of Environmental and Conservation Sciences, Murdoch University, Murdoch, WA, Australia
49 6150

50 ²⁰Sorbonne Université, CNRS, UMR 7144, Station Biologique, F.29680 Roscoff, France

51 ²¹Flanders Research Institute for Agriculture, Fisheries and Food (ILVO), Ankerstraat 1, 8400 Ostend,
52 Belgium

53 ²²Marine Science Institute, University of California Santa Barbara, Santa Barbara, California 93106
54 USA

55 ²³Hawaii Institute of Marine Biology, University of Hawaii at Manoa, Honolulu, Hawaii 96822 USA

56 ²⁴Institute for Marine & Coastal Sciences, University of South Carolina, United States

57 ²⁵Centre for Fisheries Ecosystems Research, Fisheries & Marine Institute, Memorial University of
58 Newfoundland, St. John's, Canada

59 ²⁶School of Biological Sciences, Victoria University of Wellington, P O Box 600, Wellington 6140, New
60 Zealand

61 ²⁷Lancaster Environment Centre, Lancaster University, LA1 4YQ, Lancaster, UK

62 ²⁸Fisheries, Aquatic Science and Technology Laboratory, Alaska Pacific University, 4101 University
63 Dr. Anchorage, AK 99508, USA

64 ²⁹Natural Resources Institute Finland, Manamansalontie 90, 88300 Paltamo, Finland

65 ³⁰Department of Biology, Memorial University, St. John's, NL A1B 2R3, Canada

66 ³¹National Marine Science Centre and Marine Ecology Research Centre, Southern Cross University, 2
67 Bay Drive, Coffs Harbour, 2450, Australia

68 ³²Department of Biological and Environmental Science, University of Jyväskylä, Finland

69 ³³School of Resource Wisdom, University of Jyväskylä, Finland

70 ³⁴Centre for Ecology, Evolution and Environmental Changes – cE3c, Faculty of Sciences, University
71 of Lisbon, 1749-016 Lisbon, Portugal

72 ³⁵Biological Dynamics of Forest Fragments Project, National Institute for Amazonian Research and
73 Smithsonian Tropical Research Institute, 69011-970, Manaus, Brazil

74 ³⁶Granollers Museum of Natural History, Granollers, Spain

75 ³⁷Department of Biological Sciences, University of New Brunswick, PO Box 5050, Saint John NB, E2L
76 4L5, Canada

77 ³⁸Voimalohi Oy, Voimatie 23, 91100 Ii, Finland

78 ³⁹Natural Resources Institute Finland, Paavo Havaksen tie 3, 90014 University of Oulu, Finland

79 ⁴⁰Fundación Migres CIMA Ctra Cádiz Spain

80 ⁴¹Intergovernmental Oceanographic Commission of UNESCO, Marine Policy and Regional
81 Coordination Section Paris 07, France

82 ⁴²BioRISC, St. Catharine's College, Cambridge CB2 1RL, UK

83 ⁴³Departamento de Ecología e Hidrología, Universidad de Murcia, Campus de Espinardo, 30100
84 Murcia, Spain

85 ⁴⁴RACE Division, Alaska Fisheries Science Center, National Marine Fisheries Service, NOAA, 7600
86 Sand Point Way NE, Seattle, WA 98115 USA

87 ⁴⁵European Commission, Joint Research Centre (JRC), Ispra (VA), Italy

88 ⁴⁶School of Science, Engineering and Environment, University of Salford, Salford, M5 4WT, UK

89 ⁴⁷Victorian National Park Association, Carlton, Victoria, Australia

90 ⁴⁸Department of Earth, Environment and Life Sciences (DiSTAV), University of Genoa, Corso Europa
91 26, 16132 Genoa, Italy

92 ⁴⁹Department of Ecology, Swedish University of Agricultural Sciences, Uppsala, Sweden

93 ⁵⁰Chair of Plant Health, Institute of Agricultural and Environmental Sciences, Estonian University of
94 Life Sciences, Tartu, Estonia

95 ⁵¹Biosecurity New Zealand – Tiakitanga Pūtaiao Aotearoa, Ministry for Primary Industries – Manatū
96 Ahu Matua, 66 Ward St, PO Box 40742, Wallaceville, New Zealand

97 ⁵²National Institute of Water & Atmospheric Research Ltd (NIWA), 301 Evans Bay Parade, Greta
98 Point Wellington New Zealand

99 ⁵³CSIRO Oceans & Atmosphere, Queensland Biosciences Precinct, 306 Carmody Road, ST. LUCIA
100 QLD 4067 Australia

101 ⁵⁴Museo Nacional de Ciencias Naturales, CSIC, José Gutiérrez Abascal 2, E-28006, Madrid, Spain

102 ⁵⁵Fort Keogh Livestock and Range Research Laboratory, 243 Fort Keogh Rd, Miles City, Montana,
103 59301, USA.

104 ⁵⁶CIBIO-InBIO, Research Centre in Biodiversity and Genetic Resources, University of Porto, Vairão,
105 Portugal

106 ⁵⁷Departamento de Sistemas Físicos, Químicos y Naturales, Universidad Pablo de Olavide, ES-
107 41013 Sevilla, Spain

108 ⁵⁸El Colegio de la Frontera Sur, A.P. 424, 77000 Chetumal, QR, Mexico

109 ⁵⁹Division of Fish and Wildlife, New York State Department of Environmental Conservation, 625
110 Broadway, Albany, NY 12233-4756, USA

111 ⁶⁰University of Denver Department of Biological Sciences, Denver, CO, USA

112 ⁶¹U.S. Geological Survey, Fort Collins Science Center, Fort Collins, CO USA 80526

113 ⁶²School for Marine Science and Technology, University of Massachusetts Dartmouth, New Bedford,
114 MA, USA

115 ⁶³Georges Lemaître Earth and Climate Research Centre, Earth and Life Institute, Université
116 Catholique de Louvain, 1348 Louvain-la-Neuve, Belgium

117 ⁶⁴Center for Systems Integration and Sustainability, Department of Fisheries and Wildlife, 13 Michigan
118 State University, East Lansing, MI 48823, United States

119 ⁶⁵Natural Resources Institute Finland, Latokartanonkaari 9, 00790 Helsinki, Finland

120 ⁶⁶Manaaki Whenua – Landcare Research, Private Bag 3127, Hamilton 3216, New Zealand

121 ⁶⁷Statistical Laboratory, Department of Pure Mathematics and Mathematical Statistics, University of
122 Cambridge, Wilberforce Road, Cambridge, CB3 0WB, UK.

123

124

125

126

127

128

129

130

131

132

133 Abstract

134

135 Building trust in science and evidence-based decision-making depends heavily on the
136 credibility of studies and their findings. Researchers employ many different study designs
137 that vary in their risk of bias to evaluate the true effect of interventions or impacts. Here, we
138 empirically quantify, on a large scale, the prevalence of different study designs and the
139 magnitude of bias in their estimates. Randomised designs and controlled observational
140 designs with pre-intervention sampling were used by just 23% of intervention studies in
141 biodiversity conservation, and 36% of intervention studies in social science. We
142 demonstrate, through pairwise within-study comparisons across 49 environmental datasets,
143 that these types of designs usually give less biased estimates than simpler observational
144 designs. We propose a model-based approach to combine study estimates that may suffer
145 from different levels of study design bias, discuss the implications for evidence synthesis,
146 and how to facilitate the use of more credible study designs.

147

148 Introduction

149

150 The ability of science to reliably guide evidence-based decision-making hinges on the
151 accuracy and credibility of studies and their results^{1,2}. Well-designed, randomised
152 experiments are widely accepted to yield more credible results than non-randomised,
153 ‘observational studies’ that attempt to approximate and mimic randomised experiments³.
154 Randomisation is a key element of study design that is widely used across many disciplines
155 because of its ability to remove confounding biases (through random assignment of the
156 treatment or impact of interest^{4,5}). However, ethical, logistical, and economic constraints
157 often prevent the implementation of randomised experiments, whereas non-randomised
158 observational studies have become popular as they take advantage of historical data for new
159 research questions, larger sample sizes, less costly implementation, and more relevant and
160 representative study systems or populations⁶⁻⁹. Observational studies nevertheless face the
161 challenge of accounting for confounding biases without randomisation, which has led to
162 innovations in study design.

163

164 We define ‘study design’ as an organised way of collecting data. Importantly, we distinguish
165 between data collection and statistical analysis (as opposed to other authors¹⁰) because of
166 the belief that bias introduced by a flawed design is often much more important than bias
167 introduced by statistical analyses. This was emphasised by Light, Singer & Willet¹¹ (p. 5):

168 “You can't fix by analysis what you bungled by design...”; and Rubin³: “Design trumps
169 analysis.” Nevertheless, the importance of study design has often been overlooked in
170 debates over the inability of researchers to reproduce the original results of published
171 studies (so-called ‘reproducibility crises’^{12,13}) in favour of other issues (e.g., p-hacking¹⁴ and
172 Hypothesizing After Results are Known or ‘HARKing’¹⁵).

173

174 To demonstrate the importance of study designs, we can use the following decomposition of
175 estimation error equation¹⁶:

176 Estimation error = (Estimator - true causal effect) = (Design bias + Modelling bias + Statistical noise). (1)

177

178 This demonstrates that even if we improve the quality of modelling and analysis (to reduce
179 modelling bias through a better bias-variance trade-off¹⁷) or increase sample size (to reduce
180 statistical noise), we cannot remove the intrinsic bias introduced by the choice of study
181 design (design bias) unless we collect the data in a different way. The importance of study
182 design in determining the levels of bias in study results therefore cannot be overstated.

183

184 For the purposes of this study we consider six commonly used study designs; differences
185 and connections can be visualised in Fig.1. There are three major components that allow us
186 to define these designs: randomisation, sampling before and after the impact of interest
187 occurs, and the use of a control group.

188

189

190 Of the non-randomised observational designs, the Before-After Control-Impact (BACI)
191 design uses a control group and samples before and after the impact occurs (i.e., in the
192 ‘before-period’ and the ‘after-period’). Its rationale is to explicitly account for pre-existing
193 differences between the impact group (exposed to the impact) and control group in the
194 before-period, which might otherwise bias the estimate of the impact’s true effect^{6,18,19}.

195

196 The BACI design improves upon several other commonly used observational study designs,
197 of which there are two uncontrolled designs: After, and Before-After (BA). An After design
198 monitors an impact group in the after-period, while a BA design compares the state of the
199 impact group between the before- and after-periods. Both designs can be expected to yield
200 poor estimates of the impact’s true effect (large design bias; Equation (1)) because changes
201 in the response variable could have occurred without the impact (e.g., due to natural
202 seasonal changes; Fig.1).

203

204 The other observational design is Control-Impact (CI), which compares the impact group and
205 control group in the after-period (Fig.1). This design may suffer from design bias introduced
206 by pre-existing differences between the impact group and control group in the before-period;
207 bias that the BACI design was developed to account for^{20,21}. These differences have many
208 possible sources, including experimenter bias, logistical and environmental constraints, and
209 various confounding factors (variables that change the propensity of receiving the impact),
210 but can be adjusted for through certain data pre-processing techniques such as matching
211 and stratification²².

212

213 Among the randomised designs, the most commonly used are counterparts to the
214 observational CI and BACI designs: Randomised Control-Impact (R-CI) and Randomised
215 Before-After Control-Impact (R-BACI) designs. The R-CI design, often termed 'Randomised
216 Controlled Trials' (RCTs) in medicine and hailed as the 'gold standard'^{23,24}, removes any pre-
217 impact differences in a stochastic sense, resulting in zero design bias (Equation (1)).
218 Similarly, the R-BACI design should also have zero design bias, and the impact group
219 measurements in the before-period could be used to improve the efficiency of the statistical
220 estimator. No randomised equivalents exist of After or BA designs as they are uncontrolled.

221

222 It is important to briefly note that there is debate over two major statistical methods that can
223 be used to analyse data collected using BACI and R-BACI designs, and which is superior at
224 reducing modelling bias²⁵ (Equation (1)). These statistical methods are: i.) Differences in
225 Differences (DiD) estimator; and ii.) covariance adjustment using the before-period
226 response, which is an extension of Analysis of Covariance (ANCOVA) for generalised linear
227 models — herein termed 'covariance adjustment' (Fig.1). These estimators rely on different
228 assumptions to obtain unbiased estimates of the impact's true effect. The DiD estimator
229 assumes that the control group response accurately represents the impact group response
230 had it not been exposed to the impact ('parallel trends'^{18,26}) whereas covariance adjustment
231 assumes there are no unmeasured confounders and linear model assumptions hold^{6,27}.

232

233 From both theory and Equation (1), with similar sample sizes, randomised designs (R-BACI
234 and R-CI) are expected to be less biased than controlled, observational designs with
235 sampling in the before-period (BACI), which in turn should be superior to observational
236 designs without sampling in the before-period (CI) or without a control group (BA and After
237 designs^{7,28}). Between randomised designs, we might expect that an R-BACI design performs
238 better than a R-CI design because utilising extra data before the impact may improve the
239 efficiency of the statistical estimator by explicitly characterising pre-existing differences
240 between the impact group and control group.

241

242 Given the likely differences in bias associated with different study designs, concerns have
243 been raised over the use of poorly designed studies in several scientific disciplines^{7,29-35}.
244 Some disciplines, such as the social and medical sciences, commonly undertake direct
245 comparisons of results obtained by randomised and non-randomised designs within a single
246 study³⁶⁻³⁸ or between multiple studies (between-study comparisons³⁹⁻⁴¹) to specifically
247 understand the influence of study designs on research findings. However, within-study
248 comparisons are limited in their scope (e.g., a single study^{42,43}) and between-study
249 comparisons can be confounded by variability in context or study populations⁴⁴.
250 Overall, we lack quantitative estimates of the prevalence of different study designs and the
251 levels of bias associated with their results.

252

253 In this work, we aim to first quantify the prevalence of different study designs in the social
254 and environmental sciences. To fill this knowledge gap, we take advantage of summaries for
255 several thousand biodiversity conservation intervention studies in the Conservation Evidence
256 database⁴⁵ (www.conservationevidence.com) and social intervention studies in systematic
257 reviews by the Campbell Collaboration (www.campbellcollaboration.org). We then quantify
258 the levels of bias in estimates obtained by different study designs (R-BACI, R-CI, BACI, BA,
259 and CI) by applying a hierarchical model to approximately 1,000 within-study comparisons
260 across 49 raw environmental datasets from a range of fields. We show that R-BACI, R-CI
261 and BACI designs are poorly represented in studies testing biodiversity conservation and
262 social interventions, and that these types of designs tend to give less biased estimates than
263 simpler observational designs. We propose a model-based approach to combine study
264 estimates that may suffer from different levels of study design bias, discuss the implications
265 for evidence synthesis, and how to facilitate the use of more credible study designs.

266

267 Results

268

269 Prevalence of study designs

270

271 We found that the biodiversity-conservation (Conservation Evidence) and social-science
272 (Campbell Collaboration) literature had similarly high proportions of studies that used CI
273 designs and After designs, but low proportions of studies that used R-BACI, BACI, or BA
274 designs (Fig.2). There were slightly higher proportions of R-CI designs in social-science
275 reviews than in the biodiversity-conservation literature (Fig.2). The R-BACI, R-CI, and BACI

276 designs made up 23% of studies for biodiversity conservation, and 36% of studies for social
277 science.

278

279

280 Influence of different study designs on study results

281

282 In non-randomised datasets, we found that estimates of BACI (with covariance adjustment)
283 and CI designs were very similar, while the point estimates for most other designs often
284 differed substantially in their magnitude and sign. We found similar results in randomised
285 datasets for R-BACI (with covariance adjustment) and R-CI designs. For approximately 30%
286 of responses, in both non-randomised and randomised datasets, study design estimates
287 differed in their statistical significance (i.e., $p < 0.05$ versus $p \geq 0.05$), except for estimates of
288 (R-)BACI (with covariance adjustment) and (R-)CI designs (Table 1; Fig.3). It was rare for
289 the 95% confidence intervals of different designs' estimates to not overlap – except when
290 comparing estimates of BA designs to (R-)BACI (with covariance adjustment) and (R-)CI
291 designs (Table 1). It was even rarer for estimates of different designs to have significantly
292 different signs (i.e., one estimate with entirely negative confidence intervals versus one with
293 entirely positive confidence intervals; Table 1, Fig.3). Overall, point estimates often differed
294 greatly in their magnitude and, to a lesser extent, in their sign between study designs, but did
295 not differ as greatly when accounting for the uncertainty around point estimates – except in
296 terms of their statistical significance.

297

298

299 Levels of bias in estimates of different study designs

300

301 We modelled study design bias using a random effect across datasets in a hierarchical
302 Bayesian model; σ is the standard deviation of the bias term, and assuming bias is randomly
303 distributed across datasets and is on average zero, larger values of σ will indicate a greater
304 magnitude of bias (see Methods). We found that, for randomised datasets, estimates of both
305 R-BACI (using covariance adjustment; CA) and R-CI designs were affected by negligible
306 amounts of bias (very small values of σ ; Table 2). When the R-BACI design used the DiD
307 estimator, it suffered from slightly more bias (slightly larger values of σ), whereas the BA
308 design had very high bias when applied to randomised datasets (very large values of σ ;
309 Table 2). There was a highly positive correlation between the estimates of R-BACI (using
310 covariance adjustment) and R-CI designs ($\Omega[\text{R-BACI CA, R-CI}]$ was close to 1; Table 2).
311 Estimates of R-BACI using the DiD estimator were also positively correlated with estimates

312 of R-BACI using covariance adjustment and R-CI designs (moderate positive mean values of
313 Ω [R-BACI CA, R-BACI DiD] and Ω [R-BACI DiD, R-CI]; Table 2).

314

315 For non-randomised datasets, controlled designs (BACI and CI) were substantially less
316 biased (far smaller values of σ) than the uncontrolled BA design (Table 2). A BACI design
317 using the DiD estimator was slightly less biased than the BACI design using covariance
318 adjustment, which was, in turn, slightly less biased than the CI design (Table 2).

319

320 Standard errors estimated by the hierarchical Bayesian model were reasonably accurate for
321 the randomised datasets (see λ in Methods and Table 2), whereas there was some
322 underestimation of standard errors and lack-of-fit for non-randomised datasets.

323

324

325

326 Discussion

327

328 Our approach provides a principled way to quantify the levels of bias associated with
329 different study designs. We found that randomised study designs (R-BACI and R-CI) and
330 observational BACI designs are poorly represented in the environmental and social
331 sciences; collectively, descriptive case studies (the After design), the uncontrolled BA
332 design, and the observational CI design made up a substantially greater proportion of
333 intervention studies (Fig.2). And yet R-BACI, R-CI and BACI designs were found to be
334 quantifiably less biased than other observational designs.

335

336 As expected the R-CI and R-BACI designs (using a covariance adjustment estimator)
337 performed well; the R-BACI design using a DiD estimator performed slightly less well,
338 probably because the differencing of pre-impact data by this estimator may introduce
339 additional statistical noise compared to covariance adjustment, which controls for these data
340 using a lagged regression variable. Of the observational designs, the BA design performed
341 very poorly (both when analysing randomised and non-randomised data) as expected, being
342 uncontrolled and therefore prone to severe design bias^{7,28}. The CI design also tended to be
343 more biased than the BACI design (using a DiD estimator) due to pre-existing differences
344 between the impact and control groups. For BACI designs, we recommend that the
345 underlying assumptions of DiD and CA estimators are carefully considered before choosing
346 to apply them to data collected for a specific research question^{6,27}. Their levels of bias were
347 negligibly different and their known bracketing relationship suggests they will typically give

348 estimates with the same sign, although their tendency to over- or underestimate the true
349 effect will depend on how well the underlying assumptions of each are met (most notably,
350 parallel trends for DiD and no unmeasured confounders for CA; see Introduction)^{6,27}. Overall,
351 these findings demonstrate the power of large within-study comparisons to directly quantify
352 differences in the levels of bias associated with different designs.

353

354 We must acknowledge that the assumptions of our hierarchical model (that the bias for each
355 design (j) is on average zero and normally distributed) cannot be verified without gold
356 standard randomised experiments and that, for observational designs, the model was
357 overdispersed (potentially due to underestimation of statistical error by GLM(M)s or
358 positively correlated design biases). The exact values of our hierarchical model should
359 therefore be treated with appropriate caution, and future research is needed to refine and
360 improve our approach to quantify these biases more precisely. Responses within datasets
361 may also not be independent as multiple species could interact; therefore, the estimates
362 analysed by our hierarchical model are statistically dependent on each other, and although
363 we tried to account for this using a correlation matrix (see Methods, Equation (3)), this is a
364 limitation of our model. We must also recognise that we collated datasets using non-
365 systematic searches^{46,47} and therefore our analysis potentially exaggerates the intrinsic
366 biases of observational designs (i.e., our data may disproportionately reflect situations where
367 the BACI design was chosen to account for confounding factors). We nevertheless show that
368 researchers were wise to use the BACI design because it was less biased than CI and BA
369 designs across a wide range of datasets from various environmental systems and locations.
370 Without undertaking costly and time-consuming pre-impact sampling and pilot studies,
371 researchers are also unlikely to know the levels of bias that could affect their results. Finally,
372 we did not consider sample size, but it is likely that researchers might use larger sample
373 sizes for CI and BA designs than BACI designs. This is, however, unlikely to affect our main
374 conclusions because larger sample sizes could increase type I errors (false positive rate) by
375 yielding more precise, but biased estimates of the true effect²⁸.

376

377 Our analyses provide several empirically supported recommendations for researchers
378 designing future studies to assess an impact of interest. First, using a controlled and/or
379 randomised design (if possible) was shown to strongly reduce the level of bias in study
380 estimates. Second, when observational designs must be used (as randomisation is not
381 feasible or too costly), we urge researchers to choose the BACI design over other
382 observational designs — and when that is not possible, to choose the CI design over the
383 uncontrolled BA design. We acknowledge that limited resources, short funding timescales,

384 and ethical or logistical constraints⁴⁸ may force researchers to use the CI design (if
385 randomisation and pre-impact sampling are impossible) or the BA design (if appropriate
386 controls cannot be found²⁸). To facilitate the usage of less biased designs, longer-term
387 investments in research effort and funding are required⁴³. Far greater emphasis on study
388 designs in statistical education⁴⁹ and better training and collaboration between researchers,
389 practitioners and methodologists, is needed to improve the design of future studies; for
390 example, potentially improving the CI design by pairing or matching the impact group and
391 control group²², or improving the BA design using regression discontinuity methods^{48,50}.
392 Where the choice of study design is limited, researchers must transparently communicate
393 the limitations and uncertainty associated with their results.

394

395 Our findings also have wider implications for evidence synthesis, specifically the exclusion of
396 certain observational study designs from syntheses (the ‘rubbish in, rubbish out’
397 concept^{51,52}). We believe that observational designs should be included in systematic
398 reviews and meta-analyses, but that careful adjustments are needed to account for their
399 potential biases. Exclusion of observational studies often results from subjective, checklist-
400 based ‘Risk of Bias’ or quality assessments of studies (e.g., AMSTRAD 2⁵³, ROBINS-I⁵⁴, or
401 GRADE⁵⁵) that are not data-driven and often neglect to identify the actual direction, or
402 quantify the magnitude, of possible bias introduced by observational studies when rating the
403 quality of a review’s recommendations. We also found that there was a small proportion of
404 studies that used randomised designs (R-CI or R-BACI) or observational BACI designs
405 (Fig.2), suggesting that systematic reviews and meta-analyses risk excluding a substantial
406 proportion of the literature and limiting the scope of their recommendations if such exclusion
407 criteria are used^{32,56,57}. This problem is compounded by the fact that, at least in conservation
408 science, studies using randomised or BACI designs are strongly concentrated in Europe,
409 Australasia, and North America³¹. Systematic reviews that rely on these few types of study
410 designs are therefore likely to fail to provide decision makers outside of these regions with
411 locally relevant recommendations that they prefer⁵⁸. The Covid-19 pandemic has highlighted
412 the difficulties in making locally relevant evidence-based decisions using studies conducted
413 in different countries with different demographics and cultures, and on patients of different
414 ages, ethnicities, genetics, and underlying health issues⁵⁹. This problem is also acute for
415 decision-makers working on biodiversity conservation in the tropical regions, where the need
416 for conservation is arguably the greatest (i.e., where most of Earth’s biodiversity exists⁶⁰) but
417 they either have to rely on very few well-designed studies that are not locally relevant (i.e.,
418 have low generalisability), or more studies that are locally relevant but less well-
419 designed^{31,32}. Either option could lead decision-makers to take ineffective or inefficient
420 decisions. In the long-term, improving the quality and coverage of scientific evidence and

421 evidence syntheses across the world will help solve these issues, but shorter-term solutions
422 to synthesising patchy evidence bases are required.

423

424 Our work furthers sorely needed research on how to combine evidence from studies that
425 vary greatly in their design. Our approach is an alternative to conventional meta-analyses
426 which tend to only weight studies by their sample size or the inverse of their variance⁶¹;
427 when studies vary greatly in their study design, simply weighting by inverse variance or
428 sample size is unlikely to account for different levels of bias introduced by different study
429 designs (see Equation (1)). For example, a BA study could receive a larger weight if it had
430 lower variance than a BACI study, despite our results suggesting a BA study usually suffers
431 from greater design bias. Our model provides a principled way to weight studies by both the
432 likely amount of bias introduced by their study design and their variance and is therefore a
433 form of ‘bias-adjusted meta-analysis’^{62–66}. However, instead of relying on elicitation of
434 subjective expert opinions on the bias of each study, we provide a data-driven, empirical
435 quantification of study biases – an important step that was called for to improve such meta-
436 analytic approaches^{65,66}.

437

438 Future research is needed to refine our methodology, but our empirically grounded form of
439 bias-adjusted meta-analysis could be implemented as follows: 1.) collate studies for the
440 same true effect, their effect size estimates, standard errors, and the type of study design;
441 2.) enter these data into our hierarchical model, where effect size estimates share the same
442 intercept (the true causal effect), a random effect term due to design bias (whose variance is
443 estimated by the method we used), and a random effect term for statistical noise (whose
444 variance is estimated by the reported standard error of studies); 3.) fit this model and
445 estimate the shared intercept/true effect. Heuristically, this can be thought of as weighting
446 studies by both their design bias and their sampling variance and could be implemented on a
447 dynamic meta-analysis platform (such as metadataset.com⁶⁷). This approach has substantial
448 potential to develop evidence synthesis in fields (such as biodiversity conservation^{31,32}) with
449 patchy evidence bases, where reliably synthesising findings from studies that vary greatly in
450 their design is a fundamental challenge.

451

452 Our study has highlighted an often overlooked aspect of debates over scientific
453 reproducibility: that the credibility of studies is fundamentally determined by study design.
454 Testing the effectiveness of conservation and social interventions is undoubtedly of great
455 importance given the current challenges facing biodiversity and society in general and the
456 serious need for more evidence-based decision-making^{1,68}. And yet our findings suggest that

457 quantifiably less biased study designs are poorly represented in the environmental and
458 social sciences. Greater methodological training of researchers and funding for intervention
459 studies, as well as stronger collaborations between methodologists and practitioners is
460 needed to facilitate the use of less biased study designs. Better communication and
461 reporting of the uncertainty associated with different study designs is also needed, as well as
462 more meta-research (the study of research itself) to improve standards of study design⁶⁹.
463 Our hierarchical model provides a principled way to combine studies using a variety of study
464 designs that vary greatly in their risk of bias, enabling us to make more efficient use of
465 patchy evidence bases. Ultimately, we hope that researchers and practitioners testing
466 interventions will think carefully about the types of study designs they use, and we
467 encourage the evidence synthesis community to embrace alternative methods for combining
468 evidence from heterogeneous sets of studies to improve evidence-based decision-making in
469 all disciplines.

470

471 **Methods**

472

473 **Quantifying the use of different designs**

474

475 We compared the use of different study designs in the literature that quantitatively tested
476 interventions between the fields of biodiversity conservation (4,260 studies collated by
477 Conservation Evidence⁴⁵) and social science (1,009 studies found by 32 systematic reviews
478 produced by the Campbell Collaboration: www.campbellcollaboration.org).

479

480 Conservation Evidence is a database of intervention studies, each of which has
481 quantitatively tested a conservation intervention (e.g., sowing strips of wildflower seeds on
482 farmland to benefit birds), that is continuously being updated through comprehensive,
483 manual searches of conservation journals for a wide range of fields in biodiversity
484 conservation (e.g., amphibian, bird, peatland, and farmland conservation⁴⁵). To obtain the
485 proportion of studies with each design from Conservation Evidence, we simply extracted the
486 type of study design used by each study from the database in 2019 – the study design was
487 determined using a standardised set of criteria; reviews were not included (Table 3). We
488 checked if the designs reported in the database accurately reflected the designs in the
489 original publication and found that for a random subset of 356 studies, 95.1% were
490 accurately described.

491

492 Each systematic review produced by the Campbell Collaboration collates and analyses
493 studies that test a specific social intervention; we collated reviews that tested a variety of
494 social interventions across several fields in the social sciences, including education, crime
495 and justice, international development and social welfare (Supplementary Data 1). We
496 retrieved systematic reviews produced by the Campbell Collaboration by searching their
497 website (www.campbellcollaboration.org) for reviews published between 2013–2019 (as of
498 8th September 2019) — we limited the date range as we could not go through every review.
499 As we were interested in the use of study designs in the wider social-science literature, we
500 only considered reviews (32 in total) that contained sufficient information on the number of
501 included and excluded studies that used different study designs. Studies may be excluded
502 from systematic reviews for several reasons, such as their relevance to the scope of the
503 review (e.g., testing a relevant intervention) and their study design. We only considered
504 studies if the sole reason for their exclusion from the review was their study design – i.e.,
505 reviews clearly reported that the study was excluded because it used a particular study
506 design, and not because of any other reason, such as its relevance to the review’s research
507 questions. We calculated the proportion of studies that used each design in each systematic
508 review (using the same criteria as for the biodiversity-conservation literature – see Table 3)
509 and then averaged these proportions across all reviews.

510

511 Within-study comparisons of different study designs

512

513 We wanted to make direct within-study comparisons between the estimates obtained by
514 different study designs (e.g., see ^{38,70,71} for single within-study comparisons) for many
515 different studies. If a dataset contains data collected using a BACI design, subsets of these
516 data can be used to mimic the use of other study designs (a BA design using only data for
517 the impact group, and a CI design using only data collected after the impact occurred).
518 Similarly, if data were collected using a R-BACI design, subsets of these data can be used to
519 mimic the use of a BA design and a R-CI design. Collecting BACI and R-BACI datasets
520 would therefore allow us to make direct within-study comparisons of the estimates obtained
521 by these designs.

522

523 We collated BACI and R-BACI datasets by searching the Web of Science Core Collection⁷²
524 which included the following citation indexes: Science Citation Index Expanded (SCI-
525 EXPANDED) 1900-present; Social Sciences Citation Index (SSCI) 1900-present Arts &
526 Humanities Citation Index (A&HCI) 1975-present; Conference Proceedings Citation Index -
527 Science (CPCI-S) 1990-present; Conference Proceedings Citation Index - Social Science &

528 Humanities (CPCI-SSH) 1990-present; Book Citation Index - Science (BKCI-S) 2008-
529 present; Book Citation Index - Social Sciences & Humanities (BKCI-SSH) 2008-present;
530 Emerging Sources Citation Index (ESCI) 2015-present; Current Chemical Reactions (CCR-
531 EXPANDED) 1985-present (Includes Institut National de la Propriete Industrielle structure
532 data back to 1840); Index Chemicus (IC) 1993-present. The following search terms were
533 used: ['BACI'] OR ['Before-After Control-Impact'] and the search was conducted on the 18th
534 December 2017. Our search returned 674 results, which we then refined by selecting only
535 'Article' as the document type and using only the following Web of Science Categories:
536 'Ecology', 'Marine Freshwater Biology', 'Biodiversity Conservation', 'Fisheries',
537 'Oceanography', 'Forestry', 'Zoology', 'Ornithology', 'Biology', 'Plant Sciences', 'Entomology',
538 'Remote Sensing', 'Toxicology' and 'Soil Science'. This left 579 results, which we then
539 restricted to articles published since 2002 (15 years prior to search) to give us a realistic
540 opportunity to obtain the raw datasets, thus reducing this number to 542. We were able to
541 access the abstracts of 521 studies and excluded any that did not test the effect of an
542 environmental intervention or threat using an R-BACI or BACI design with response
543 measures related to the abundance (e.g., density, counts, biomass, cover), reproduction
544 (reproductive success) or size (body length, body mass) of animals or plants. Many studies
545 did not test a relevant metric (e.g., they measured species richness), did not use a BACI or
546 R-BACI design, or did not test the effect of an intervention or threat — this left 96 studies for
547 which we contacted all corresponding authors to ask for the raw dataset. We were able to
548 fully access 54 raw datasets, but upon closer inspection we found that three of these
549 datasets either: did not use a BACI design; did not use the metrics we specified; or did not
550 provide sufficient data for our analyses. This left 51 datasets in total that we used in our
551 preliminary analyses (Supplementary Data 2).

552

553 All the datasets were originally collected to evaluate the effect of an environmental
554 intervention or impact. Most of them contained multiple response variables (e.g., different
555 measures for different species, such as abundance or density for species A, B, and C).
556 Within a dataset, we use the term "response" to refer to the estimation of the causal effect on
557 one response variable. There were 1,968 responses in total across 51 datasets. We then
558 excluded 932 responses (resulting in the exclusion of one dataset) where one or more of the
559 four time-period and treatment subsets (Before Control, Before Impact, After Control, and
560 After Impact data) consisted of entirely zero measurements, or two or more of these subsets
561 had more than 90% zero measurements. We also excluded one further dataset as it was the
562 only one to not contain repeated measurements at sites in both the before- and after-
563 periods. This was necessary to generate reliable standard errors when modelling these data.

564 We modelled the remaining 1,036 responses from across 49 datasets (Supplementary Table
565 1).

566

567 We applied each study design to the appropriate components of each dataset
568 using Generalised Linear Models (GLMs^{73,74}) because of their generality and ability to
569 implement the statistical estimators of many different study designs. The model structure of
570 GLMs was adjusted for each response in each dataset based on the study design specified,
571 response measure and dataset structure (Supplementary Table 2). We quantified the effect
572 of the time period for the BA design (After vs Before the impact) and the effect of the
573 treatment type for the CI and R-CI designs (Impact vs Control) on the response variable
574 (Supplementary Table 2). For BACI and R-BACI designs, we implemented two statistical
575 estimators: 1.) a DiD estimator that estimated the true effect using an interaction term
576 between time and treatment type; and 2.) a covariance adjustment estimator that estimated
577 the true effect using a term for the treatment type with a lagged variable (Supplementary
578 Table 2).

579

580 As there were large numbers of responses, we used general *a priori* rules to specify models
581 for each response; this may have led to some model misspecification, but was unlikely to
582 have substantially affected our pairwise comparison of estimates obtained by different
583 designs. The error family of each GLM was specified based on the nature of the measure
584 used and preliminary data exploration: count measures (e.g., abundance) = poisson; density
585 measures (e.g., biomass or abundance per unit area) = quasipoisson, as data for these
586 measures tended to be overdispersed; percentage measures (e.g., percentage cover) =
587 quasibinomial; and size measures (e.g., body length) = gaussian.

588

589 We treated each year or season in which data were collected as independent observations
590 because the implementation of a seasonal term in models is likely to vary on a case-by-case
591 basis; this will depend on the research questions posed by each study and was not feasible
592 for us to consider given the large number of responses we were modelling. The log link
593 function was used for all models to generate a standardised log response ratio as an
594 estimate of the true effect for each response; a fixed effect coefficient (a variable named
595 treatment status; Supplementary Table 2) was used to estimate the log response ratio⁶¹. If
596 the response had at least ten 'sites' (independent sampling units) and two measurements
597 per site on average, we used the random effects of subsample (replicates within a site)
598 nested within site to capture the dependence within a site and subsample (i.e., a
599 Generalised Linear Mixed Model or GLMM^{73,74} was implemented instead of a GLM);
600 otherwise we fitted a GLM with only the fixed effects (Supplementary Table 2).

601

602 We fitted all models using R version 3.5.1⁷⁵, and packages lme4⁷⁶ and MASS⁷⁷. Code to
603 replicate all analyses is available (see Data and Code Availability). We compared the
604 estimates obtained using each study design (both in terms of point estimates and estimates
605 with associated standard error) by their magnitude and sign.

606

607 A model-based quantification of the bias in study design estimates

608

609 We used a hierarchical Bayesian model motivated by the decomposition in Equation (1) to
610 quantify the bias in different study design estimates. This model takes the estimated
611 intervention effects and their standard errors as inputs. Let $\hat{\beta}_{ij}$ be the true effect estimator in
612 study i using design j and $\hat{\sigma}_{ij}$ be its estimated standard error from the corresponding GLM or
613 GLMM. Our hierarchical model assumes:

$$\hat{\beta}_{ij} = \beta_i + \gamma_{ij} + \varepsilon_{ij},$$

614

$$\beta_i \sim N(0, \sigma_\beta^2), \gamma_{ij} \sim N(0, \sigma_j^2), \varepsilon_i \sim N(0, \Lambda), \quad (2)$$

615 where β_i is the true effect for response i , γ_{ij} is the bias of design j in response i , and ε_{ij} is
616 the sampling noise of the statistical estimator. Although γ_{ij} technically incorporates both the
617 design bias and any misspecification (modelling) bias due to using GLMs or GLMMs
618 (Equation (1)), we expect the modelling bias to be much smaller than the design bias^{3,11}. We
619 assume the statistical errors ε_i within a response are related to the estimated standard
620 errors through the following joint distribution:

621

$$\Lambda = \lambda \cdot \text{diag}(\hat{\sigma}_i) \Omega \text{diag}(\hat{\sigma}_i), \quad (3)$$

622 where Ω is the correlation matrix for the different estimators in the same response and λ is a
623 scaling factor to account for possible over/under-estimation of the standard errors.

624 This model effectively quantifies the bias of design j using the value of σ_j (larger values =
625 more bias) by accounting for within-response correlations using the correlation matrix Ω and
626 for possible under-estimation of the standard error using λ . We ensured that the prior
627 distributions we used had very large variances so they would have a very small effect on the
628 posterior distribution — accordingly we placed the following disperse priors on the variance
629 parameters:

630

$$\sigma_\beta, \sigma_1, \dots, \sigma_J \sim \text{Inv-Gamma}(1, 0.02), \lambda \sim \text{Gamma}(2, 2), \Omega \sim \text{LKJ}(1) \quad (4)$$

631 We fitted the hierarchical Bayesian model in R version 3.5.1 using the Bayesian inference
632 package rstan⁷⁸.

633

634 Data Availability

635 All data analysed in the current study are available from Zenodo,
636 <https://doi.org/10.5281/zenodo.3560856>. Source data are provided with this paper.

637

638 Code Availability

639 All code used in the current study is available from Zenodo,
640 <https://doi.org/10.5281/zenodo.3560856>.

641

642

643

644

645

646

647

648

649

650

651

652

653 References

654

- 655 1. Donnelly, C. A. *et al.* Four principles to make evidence synthesis more useful for policy. *Nature*
656 **558**, 361–364 (2018).
- 657 2. McKinnon, M. C., Cheng, S. H., Garside, R., Masuda, Y. J. & Miller, D. C. Sustainability: Map the
658 evidence. *Nature* **528**, 185–187 (2015).
- 659 3. Rubin, D. B. For objective causal inference, design trumps analysis. *The Annals of Applied*
660 *Statistics* **2**, 808–840 (2008).
- 661 4. Peirce, C. S. & Jastrow, J. On small differences in sensation. *Memoirs of the National Academy*
662 *of Sciences* **3**, (1884).
- 663 5. Fisher, R. A. *Statistical methods for research workers*. (Oliver and Boyd, 1925).
- 664 6. Angrist, J. D. & Pischke, J.-S. *Mostly harmless econometrics: An empiricist's companion*.
665 (Princeton University Press, 2008).

- 666 7. de Palma, A. *et al.* Challenges With Inferring How Land-Use Affects Terrestrial Biodiversity:
667 Study Design, Time, Space and Synthesis. in *Next Generation Biomonitoring: Part 1* 163–199
668 (Elsevier Ltd., 2018).
- 669 8. Sagarin, R. & Pauchard, A. Observational approaches in ecology open new ground in a
670 changing world. *Frontiers in Ecology and the Environment* **8**, 379–386 (2010).
- 671 9. Shadish, W. R., Cook, T. D. & Campbell, D. T. *Experimental and quasi-experimental designs for*
672 *generalized causal inference*. (Houghton Mifflin, 2002).
- 673 10. Rosenbaum, P. R. *Design of observational studies*. vol. 10 (Springer, 2010).
- 674 11. Light, R. J., Singer, J. D. & Willett, J. B. *By design: Planning research on higher education. By*
675 *design: Planning research on higher education*. (Harvard University Press, 1990).
- 676 12. Ioannidis, J. P. A. Why Most Published Research Findings Are False. *PLOS Medicine* **2**, e124
677 (2005).
- 678 13. Open Science Collaboration. Estimating the reproducibility of psychological science. *Science*
679 **349**, aac4716–aac4716 (2015).
- 680 14. John, L. K., Loewenstein, G. & Prelec, D. Measuring the prevalence of questionable research
681 practices with incentives for truth telling. *Psychological Science* **23**, 524–532 (2012).
- 682 15. Kerr, N. L. HARKing: Hypothesizing after the results are known. *Personality and Social*
683 *Psychology Review* **2**, 196–217 (1998).
- 684 16. Zhao, Q., Keele, L. J. & Small, D. S. Comment: Will competition-winning methods for causal
685 inference also succeed in practice? *Statistical Science* **34**, 72–76 (2019).
- 686 17. Friedman, J., Hastie, T. & Tibshirani, R. *The Elements of Statistical Learning*. vol. 1 (Springer
687 series in statistics, 2001).
- 688 18. Underwood, A. J. Beyond BACI: Experimental designs for detecting human environmental
689 impacts on temporal variations in natural populations. *Marine and Freshwater Research* **42**,
690 569–587 (1991).
- 691 19. Stewart-Oaten, A. & Bence, J. R. Temporal and Spatial Variation in Environmental Impact
692 Assessment. *Ecological Monographs* **71**, 305–339 (2001).
- 693 20. Eddy, T. D., Pande, A. & Gardner, J. P. A. Massive differential site-specific and species-specific
694 responses of temperate reef fishes to marine reserve protection. *Global Ecology and*
695 *Conservation* **1**, 13–26 (2014).
- 696 21. Sher, A. A. *et al.* Native species recovery after reduction of an invasive tree by biological
697 control with and without active removal. *Ecological Engineering* **111**, 167–175 (2018).
- 698 22. Imbens, G. W. & Rubin, D. B. *Causal Inference in Statistics, Social, and Biomedical Sciences*.
699 (Cambridge University Press, 2015).
- 700 23. Greenhalgh, T. *How to read a paper: the basics of Evidence Based Medicine*. (John Wiley &
701 Sons, Ltd, 2019).
- 702 24. Salmond, S. S. Randomized Controlled Trials: Methodological Concepts and Critique.
703 *Orthopaedic Nursing* **27**, (2008).

- 704 25. Geijzendorffer, I. R. *et al.* How can global conventions for biodiversity and ecosystem services
705 guide local conservation actions? *Current Opinion in Environmental Sustainability* **29**, 145–
706 150 (2017).
- 707 26. Dimick, J. B. & Ryan, A. M. Methods for Evaluating Changes in Health Care Policy. *JAMA* **312**,
708 2401 (2014).
- 709 27. Ding, P. & Li, F. A Bracketing Relationship between Difference-in-Differences and Lagged-
710 Dependent-Variable Adjustment. *Political Analysis* **27**, 605–615 (2019).
- 711 28. Christie, A. P. *et al.* Simple study designs in ecology produce inaccurate estimates of
712 biodiversity responses. *Journal of Applied Ecology* **56**, 2742–2754 (2019).
- 713 29. Watson, M. *et al.* An analysis of the quality of experimental design and reliability of results in
714 tribology research. *Wear* **426–427**, 1712–1718 (2019).
- 715 30. Kilkenny, C. *et al.* Survey of the Quality of Experimental Design, Statistical Analysis and
716 Reporting of Research Using Animals. *PLoS ONE* **4**, (2009).
- 717 31. Christie, A. P. *et al.* The challenge of biased evidence in conservation. *Conservation Biology*
718 [cobi.13577](https://doi.org/10.1111/cobi.13577) (2020) doi:10.1111/cobi.13577.
- 719 32. Christie, A. P. *et al.* Poor availability of context-specific evidence hampers decision-making in
720 conservation. *Biological Conservation* **248**, 108666 (2020).
- 721 33. Moscoe, E., Bor, J. & Bärnighausen, T. Regression discontinuity designs are underutilized in
722 medicine, epidemiology, and public health: a review of current and best practice. *Journal of*
723 *Clinical Epidemiology* **68**, 132–143 (2015).
- 724 34. Goldenhar, L. M. & Schulte, P. A. Intervention research in occupational health and safety. *J.*
725 *Occup. Med.* **36**, 763–778 (1994).
- 726 35. Junker, J. *et al.* A Severe Lack of Evidence Limits Effective Conservation of the World’s
727 Primates. *BioScience* (2020) doi:10.1093/biosci/biaa082.
- 728 36. Altindag, O., Joyce, T. J. & Reeder, J. A. Can Nonexperimental Methods Provide Unbiased
729 Estimates of a Breastfeeding Intervention? A Within-Study Comparison of Peer Counseling in
730 Oregon. *Evaluation Review* **43**, 152–188 (2019).
- 731 37. Chaplin, D. D. *et al.* The Internal And External Validity Of The Regression Discontinuity
732 Design: A Meta-Analysis Of 15 Within-Study Comparisons. *Journal of Policy Analysis and*
733 *Management* **37**, 403–429 (2018).
- 734 38. Cook, T. D., Shadish, W. R. & Wong, V. C. Three conditions under which experiments and
735 observational studies produce comparable causal estimates: New findings from within-study
736 comparisons. *Journal of Policy Analysis and Management* **27**, 724–750 (2008).
- 737 39. Ioannidis, J. P. A. *et al.* Comparison of evidence of treatment effects in randomized and
738 nonrandomized studies. *Journal of the American Medical Association* **286**, 821–830 (2001).
- 739 40. dos Santos Ribas, L. G., Pressey, R. L., Loyola, R. & Bini, L. M. A global comparative analysis of
740 impact evaluation methods in estimating the effectiveness of protected areas. *Biological*
741 *Conservation* **246**, 108595 (2020).

- 742 41. Benson, K. & Hartz, A. J. A Comparison of Observational Studies and Randomized, Controlled
743 Trials. *New England Journal of Medicine* **342**, 1878–1886 (2000).
- 744 42. Smokorowski, K. E. *et al.* Cautions on using the Before-After-Control-Impact design in
745 environmental effects monitoring programs. *Facets* **2**, 212–232 (2017).
- 746 43. França, F. *et al.* Do space-for-time assessments underestimate the impacts of logging on
747 tropical biodiversity? An Amazonian case study using dung beetles. *Journal of Applied Ecology*
748 **53**, 1098–1105 (2016).
- 749 44. Duvendack, M., Hombrados, J. G., Palmer-Jones, R. & Waddington, H. Assessing ‘what works’
750 in international development: meta-analysis for sophisticated dummies. *Journal of*
751 *Development Effectiveness* **4**, 456–471 (2012).
- 752 45. Sutherland, W. J. *et al.* Building a tool to overcome barriers in research-implementation
753 spaces: The Conservation Evidence database. *Biological Conservation* **238**, 108199 (2019).
- 754 46. Gusenbauer, M. & Haddaway, N. R. Which academic search systems are suitable for
755 systematic reviews or meta-analyses? Evaluating retrieval qualities of Google Scholar,
756 PubMed, and 26 other resources. *Research Synthesis Methods* **11**, 181–217 (2020).
- 757 47. Konno, K. & Pullin, A. S. Assessing the risk of bias in choice of search sources for
758 environmental meta-analyses. *Research Synthesis Methods* **11**, 698–713 (2020).
- 759 48. Butsic, V., Lewis, D. J., Radeloff, V. C., Baumann, M. & Kuemmerle, T. Quasi-experimental
760 methods enable stronger inferences from observational data in ecology. *Basic and Applied*
761 *Ecology* vol. 19 (2017).
- 762 49. Brownstein, N. C., Louis, T. A., O’Hagan, A. & Pendergast, J. The Role of Expert Judgment in
763 Statistical Inference and Evidence-Based Decision-Making. *The American Statistician* **73**, 56–
764 68 (2019).
- 765 50. Hahn, J., Todd, P. & Klaauw, W. Identification and Estimation of Treatment Effects with a
766 Regression-Discontinuity Design. *Econometrica* **69**, 201–209 (2001).
- 767 51. Slavin, R. E. Best evidence synthesis: An intelligent alternative to meta-analysis. *Journal of*
768 *Clinical Epidemiology* **48**, 9–18 (1995).
- 769 52. Slavin, R. E. Best-Evidence Synthesis: An Alternative to Meta-Analytic and Traditional
770 Reviews. *Educational Researcher* **15**, 5–11 (1986).
- 771 53. Shea, B. J. *et al.* AMSTAR 2: A critical appraisal tool for systematic reviews that include
772 randomised or non-randomised studies of healthcare interventions, or both. *BMJ (Online)*
773 **358**, 1–8 (2017).
- 774 54. Sterne, J. A. C. *et al.* ROBINS-I: a tool for assessing risk of bias in non-randomised studies of
775 interventions. *BMJ* **355**, i4919 (2016).
- 776 55. Guyatt, G. *et al.* GRADE guidelines: 11. Making an overall rating of confidence in effect
777 estimates for a single outcome and for all outcomes. *Journal of Clinical Epidemiology* **66**, 151–
778 157 (2013).

- 779 56. Davies, G. M. & Gray, A. Don't let spurious accusations of pseudoreplication limit our ability
780 to learn from natural experiments (and other messy kinds of ecological monitoring). *Ecology*
781 *and Evolution* **5**, 5295–5304 (2015).
- 782 57. Lortie, C. J., Stewart, G., Rothstein, H. & Lau, J. How to critically read ecological meta-
783 analyses. *Research Synthesis Methods* **6**, 124–133 (2015).
- 784 58. Gutzat, F. & Dormann, C. F. Exploration of Concerns about the Evidence-Based Guideline
785 Approach in Conservation Management: Hints from Medical Practice. *Environmental*
786 *Management* **66**, 435–449 (2020).
- 787 59. Greenhalgh, T. Will COVID-19 be evidence-based medicine's nemesis? *PLOS Medicine* **17**,
788 e1003266 (2020).
- 789 60. Barlow, J. *et al.* The future of hyperdiverse tropical ecosystems. *Nature* **559**, 517–526 (2018).
- 790 61. Gurevitch, J. & Hedges, L. v. Statistical Issues in Ecological Meta-analyses. *Ecology* **80**, 1142–
791 1149 (1999).
- 792 62. Stone, J. C., Glass, K., Munn, Z., Tugwell, P. & Doi, S. A. R. Comparison of bias adjustment
793 methods in meta-analysis suggests that quality effects modeling may have less limitations
794 than other approaches. *Journal of Clinical Epidemiology* **117**, 36–45 (2020).
- 795 63. Rhodes, K. M. *et al.* Adjusting trial results for biases in meta-analysis: combining data-based
796 evidence on bias with detailed trial assessment. *Journal of the Royal Statistical Society: Series*
797 *A (Statistics in Society)* **183**, 193–209 (2020).
- 798 64. Efthimiou, O. *et al.* Combining randomized and non-randomized evidence in network meta-
799 analysis. *Statistics in Medicine* **36**, 1210–1226 (2017).
- 800 65. Welton, N. J., Ades, A. E., Carlin, J. B., Altman, D. G. & Sterne, J. A. C. Models for Potentially
801 Biased Evidence in Meta-Analysis Using Empirically Based Priors. *Journal of the Royal*
802 *Statistical Society. Series A (Statistics in Society)* **172**, 119–136 (2009).
- 803 66. Turner, R. M., Spiegelhalter, D. J., Smith, G. C. S. & Thompson, S. G. Bias modelling in
804 evidence synthesis. *Journal of the Royal Statistical Society: Series A (Statistics in Society)* **172**,
805 21–47 (2009).
- 806 67. Shackelford, G. E. *et al.* Dynamic meta-analysis: a method of using global evidence for local
807 decision making. *bioRxiv* 2020.05.18.078840 (2020) doi:10.1101/2020.05.18.078840.
- 808 68. Sutherland, W. J., Pullin, A. S., Dolman, P. M. & Knight, T. M. The need for evidence-based
809 conservation. *Trends in ecology & evolution* **19**, 305–308 (2004).
- 810 69. Ioannidis, J. P. A. Meta-research: Why research on research matters. *PLOS Biology* **16**,
811 e2005468 (2018).
- 812 70. LaLonde, R. J. Evaluating the econometric evaluations of training programs with experimental
813 data. *The American Economic Review* 604–620 (1986).
- 814 71. Long, Q., Little, R. J. & Lin, X. Causal inference in hybrid intervention trials involving
815 treatment choice. *Journal of the American Statistical Association* **103**, 474–484 (2008).
- 816 72. Thomson Reuters. ISI Web of Knowledge. <http://www.isiwebofknowledge.com> (2019).

- 817 73. Stroup, W. W. *Generalized linear mixed models: modern concepts, methods and applications*.
818 (CRC press, 2012).
- 819 74. Bolker, B. M. *et al.* Generalized linear mixed models: a practical guide for ecology and
820 evolution. *Trends in ecology & evolution* **24**, 127–135 (2009).
- 821 75. R Core Team. R: A language and environment for statistical computing. R Foundation for
822 Statistical Computing. (2019).
- 823 76. Bates, D., Mächler, M., Bolker, B. & Walker, S. Fitting Linear Mixed-Effects Models using
824 lme4. *Journal of Statistical Software* **67**, 1–48 (2015).
- 825 77. Venables, W. N. & Ripley, B. D. *Modern Applied Statistics with S*. (Springer, 2002).
- 826 78. Stan Development Team. RStan: the R interface to Stan. R package version 2.19.3 (2020).

827

828 Acknowledgements

829 We are grateful to the following people and organisations for contributing datasets to this
830 analysis: P. Edwards, G.R. Hodgson, H. Welsh, J.V. Vieira, authors of van Deurs et al. 2012,
831 T. M. Grome, M. Kaspersen, H. Jensen, C. Stenberg, T. K. Sørensen, J. Støttrup, T. Warnar,
832 H. Mosegaard, Axel Schwerk, Alberto Velando, Dolores River Restoration Partnership, J.S.
833 Pinilla, A. Page, M. Dasey, D. Maguire, J. Barlow, J. Louzada, Jari Florestal, R.T. Buxton,
834 C.R. Schacter, J. Seoane, M.G. Conners, K. Nickel, G. Marakovich, A. Wright, G. Soprone,
835 CSIRO, A. Elozegi, L. García-Arberas, J. Díez, A. Rallo, Parks and Wildlife Finland, Parc
836 Marin de la Côte Bleue. Author funding sources: T.A. was supported by the Grantham
837 Foundation for the Protection of the Environment, Kenneth Miller Trust and Australian
838 Research Council Future Fellowship (FT180100354); W.J.S. and P.A.M. were supported by
839 Arcadia, MAVA, and The David and Claudia Harding Foundation; A.P.C. was supported by
840 the Natural Environment Research Council via Cambridge Earth System Science NERC
841 DTP (NE/L002507/1); D.A. was funded by Portugal national funds through the FCT –
842 Foundation for Science and Technology, under the Transitional Standard – DL57 / 2016 and
843 through the strategic project UIDB/04326/2020; M.A. acknowledges Koniambo Nickel SAS,
844 and particularly Gregory Marakovich and Andy Wright; J.C.A. was funded through by
845 Dirección General de Investigación Científica, projects PB97-1252, BOS2002-01543,
846 CGL2005-04893/BOS, CGL2008-02567 and Comunidad de Madrid, as well as by contract
847 HENARSA-CSIC 2003469-CSIC19637; A.A. was funded by Spanish Government: MEC
848 (CGL2007-65176); B.P.B. was funded through the U.S. Geological Survey and the New York
849 City Department of Environmental Protection; R.B. was funded by Comunidad de Madrid
850 (2018-T1/AMB-10374); J.A.S. and D.A.B. were funded through the U.S. Geological Survey
851 and NextEra Energy; R.S.C. was funded by the Portuguese Foundation for Science and
852 Technology (FCT) grant SFRH/BD/78813/2011 and strategic project UID/MAR/04292/2013;

853 A.D.B. was funded through the Belgian offshore wind monitoring program (WINMON-BE),
854 financed by the Belgian offshore wind energy sector via RBINS—OD Nature; M.K.D. was
855 funded by the Harold L. Castle Foundation; P.M.E. was funded by the Clackamas County
856 Water Environment Services River Health Stewardship Program and the Portland State
857 University Student Watershed Research Project; T.D.E., J.P.A.G. and A.P. were supported
858 by funding from the New Zealand Department of Conservation (Te Papa Atawhai) and from
859 the Centre for Marine Environmental & Economic Research, Victoria University of
860 Wellington, New Zealand; F.M.F. was funded by CNPq-CAPES grants (PELD site 23
861 403811/2012-0, PELD-RAS 441659/2016-0, BEX5528/13-5 and 383744/2015-6) and BNP
862 Paribas Foundation (Climate & Biodiversity Initiative, BIOCLIMATE project); B.P.H. was
863 funded by NOAA-NMFS sea scallop research set-aside program awards NA16FM1031,
864 NA06FM1001, NA16FM2416, and NA04NMF4720332; A.L.B. was funded by the Portuguese
865 Foundation for Science and Technology (FCT) grant FCT PD/BD/52597/2014, Bat
866 Conservation International student research fellowship and CNPq grant 160049/2013-0;
867 L.C.M. acknowledges Secretaría de Ciencia y Técnica (UNRC); R.A.M. acknowledges
868 Alaska Fisheries Science Center, NOAA Fisheries, and U.S. Department of Commerce for
869 salary support; C.F.J.M. was funded by the Portuguese Foundation for Science and
870 Technology (FCT) grant SFRH/BD/80488/2011; R.R. was funded by the Portuguese
871 Foundation for Science and Technology (FCT) grant PTDC/BIA-BIC/111184/2009, by
872 Madeira's Regional Agency for the Development of Research, Technology and Innovation
873 (ARDITI) grant M1420-09-5369-FSE-000002 and by a Bat Conservation International
874 student research fellowship; J.C. and S.S. were funded by the Alabama Department of
875 Conservation and Natural Resources; A.T. was funded by the Spanish Ministry of Education
876 with a Formacion de Profesorado Universitario (FPU) grant AP2008-00577 and Dirección
877 General de Investigación Científica, project CGL2008-02567; C.W. was funded by Strategic
878 Science Investment Funding of the Ministry of Business, Innovation and Employment, New
879 Zealand; J.S.K. acknowledges Boreal Peatland LIFE (LIFE08 NAT/FIN/000596), Parks and
880 Wildlife Finland and Kone Foundation; J.J.S.S. was funded by the Mexican National Council
881 on Science and Technology (CONACYT 242558); N.N. was funded by The Carl Tryggers
882 Foundation; I.L.J. was funded by a Discovery Grant from the Natural Sciences and
883 Engineering Research Council of Canada; D.D. and D.S. were funded by the French
884 National Research Agency via the "Investment for the Future" program IDEALG (ANR-10-
885 BTBR-04) and by the ALGMARBIO project; R.C.P. was funded by CSIRO and whose
886 research was also supported by funds from the Great Barrier Reef Marine Park Authority,
887 the Fisheries Research and Development Corporation, the Australian Fisheries Management
888 Authority, and Queensland Department of Primary Industries (QDPI). Any use of trade, firm,

889 or product names is for descriptive purposes only and does not imply endorsement by the
890 U.S. Government.

891

892 Author contributions

893 A.P.C., T.A., P.A.M., Q.Z., and W.J.S. designed the research; A.P.C. wrote the paper; D.A.,
894 M.A., J.C.A., A.A., B.P.B, R.B., J.B., D.A.B., J.C., R.S.C., L.C.M., S.C., J.C., M.D.C, D.D.,
895 A.D.B., M.K.D., T.D.E., P.M.E., F.M.F., J.P.A.G., B.P.H., A.H., I.L.J., B.P.K., J.S.K., A.L.B.,
896 H.L.M., A.M., B.M., C.A.M., D.M., R.A.M, M.M., C.F.J.M.,K.M., M.M., N.N., C.P., A.P.,
897 C.R.P., C.P., M.R., R.R., M.C.R., J.J.S.S., J.A.S., S.S., A.A.S., D.S., K.D.E.S., T.R.S., A.T.,
898 O.T., T.V., C.W. contributed datasets for analyses. All authors conducted review, editing,
899 and approved manuscript.

900

901 Competing interests

902 The authors declare no competing interests.

903

904

905

906 Figure legends

907

908 Fig.1 – Comparison of different study designs used to evaluate the effect of an impact. A
909 hypothetical study set-up is shown where the abundance of birds in three impact and control
910 replicates (e.g., fields represented by blocks in a row) are monitored before and after an
911 impact (e.g., ploughing) that occurs in year zero. Different colours represent each study
912 design and illustrate how replicates are sampled. Approaches for calculating an estimate of
913 the impact for each design are also shown, along with synonyms from different disciplines.

914

915 Fig.2 – Percentage of studies with different study designs in the biodiversity-conservation
916 and social-science literature. Studies from the biodiversity-conservation literature were
917 screened from the Conservation Evidence database (n=4,260 studies) and studies from the
918 social-science literature were screened from 32 Campbell Collaboration systematic reviews
919 (n=1,009 studies – note studies excluded by these reviews based on their study design were
920 still counted). Percentages for the social-science literature were calculated for each
921 systematic review (blue data points) and then averaged across all 32 reviews (blue bars and
922 black vertical lines represent mean and 95% Confidence Intervals, respectively).

923 Percentages for the biodiversity-conservation literature are absolute values (shown as green
924 bars) calculated from the entire Conservation Evidence database (after excluding reviews).

925 Source data are provided as a Source Data file. BA = Before-After, CI = Control-Impact,
926 BACI = Before-After-Control-Impact, R-BACI = Randomised BACI, R-CI = Randomised CI.

927
 928 Fig.3 - Pairwise comparisons of t-statistics for estimates obtained using different study
 929 designs for responses across 49 different datasets (non-randomised or randomised). t-
 930 statistics are obtained from two-sided t-tests of estimates obtained by each design for
 931 different responses in each dataset using Generalised Linear Models (see Methods). For
 932 randomised datasets, BACI and CI axis labels refer to R-BACI and R-CI designs (denoted by
 933 'R-'). DiD = Difference in Differences; CA = covariance adjustment. Lines at t-statistic values
 934 of 1.96 denote boundaries between cells and colours of points indicate differences in
 935 direction and statistical significance ($p < 0.05$; grey = same sign and significance, orange =
 936 same sign but difference in significance, red = different sign and significance). Numbers
 937 refer to the number of responses in each cell. Source data are provided as a Source Data
 938 file. BA = Before-After, CI = Control-Impact, BACI = Before-After-Control-Impact.

939
 940
 941
 942
 943
 944

945 Tables

946
 947 Table 1 – Pairwise comparison of estimates obtained using different study designs. This
 948 shows the proportion of responses in which there were differences in the magnitude (by
 949 $>100\%$) and sign of estimates, and differences in the significance, sign and overlap between
 950 associated 95% confidence intervals. For randomised datasets, BACI and CI labels refer to
 951 R-BACI and R-CI designs (denoted by 'R-'). The 100% difference in magnitude criterion is
 952 set relative to the smaller estimate. DiD = Difference in Differences; CA = covariance
 953 adjustment. 95% Conf. Ints. refers to 95% Confidence Intervals and P.E. refers to point
 954 estimate. BA = Before-After, CI = Control-Impact, BACI = Before-After-Control-Impact.

Randomised (R-)						
Design 1	Design 2	No overlap (95% Conf. Ints.)	$>100\%$ difference in magnitude (P.E.)	Different significance (95% Conf. Ints.)	Different signs (P.E.)	Significantly different sign (95% Conf. Ints.)
BACI DiD	BACI CA	0.01	0.68	0.27	0.32	0.00
BACI DiD	CI	0.01	0.69	0.27	0.32	0.00
BACI DiD	BA	0.01	0.68	0.29	0.34	0.00
BACI CA	CI	0.00	0.04	0.05	0.01	0.00
BACI CA	BA	0.16	0.82	0.33	0.47	0.06
CI	BA	0.16	0.82	0.30	0.47	0.07

Non-randomised						
Design 1	Design 2	No overlap (95% Conf. Ints.)	>100% difference in magnitude (P.E.)	Different significance (95% Conf. Ints.)	Different signs (P.E.)	Significantly different sign (95% Conf. Ints.)
BACI DiD	BACI CA	0.04	0.58	0.31	0.27	0.00
BACI DiD	CI	0.05	0.61	0.28	0.30	0.01
BACI DiD	BA	0.04	0.61	0.22	0.25	0.01
BACI CA	CI	0.00	0.18	0.08	0.08	0.00
BACI CA	BA	0.14	0.74	0.34	0.36	0.03
CI	BA	0.12	0.71	0.33	0.37	0.02

955
956
957
958
959
960

961 Table 2 – Results of hierarchical Bayesian model for randomised and non-randomised
962 datasets. In randomised datasets, BACI and CI terms refer to R-BACI and R-CI designs
963 (denoted by 'R-'). The σ terms are the standard deviations of the bias of each design, so
964 larger σ values correspond to more biased designs. σ_{β} refers to the standard deviation of the
965 true effect across all datasets. Ω represents the within-response correlations between study
966 design estimates, and λ models systematic underestimation ($\lambda > 1$) or overestimation ($\lambda < 1$) of
967 the statistical error using GLM(M)s. See methods for more details on the model. BA =
968 Before-After, CI = Control-Impact, BACI = Before-After-Control-Impact.

Randomised (R-)		
Term	Posterior mean	95% Credible Interval
σ_{β}	0.746	[0.679, 0.813]
λ	1.119	[0.980, 1.276]
σ [BACI DiD]	0.029	[0.005, 0.097]
σ [BACI CA]	0.005	[0.002, 0.008]
σ [CI]	0.005	[0.002, 0.008]
σ [BA]	0.773	[0.699, 0.846]
Ω [BACI DiD, BACI CA]	0.268	[0.152, 0.379]
Ω [BACI DiD, CI]	0.239	[0.122, 0.354]
Ω [BACI DiD, BA]	0.849	[0.770, 0.914]

Ω [BACI CA, CI]	0.995	[0.994, 0.996]
Ω [BACI CA, BA]	-0.168	[-0.332, 0.002]
Ω [CI, BA]	-0.184	[-0.349, -0.015]
Non-randomised		
Term	Posterior mean	95% Credible Interval
σ_{β}	0.700	[0.628, 0.776]
λ	1.822	[1.595, 2.098]
σ [BACI DiD]	0.017	[0.004, 0.049]
σ [BACI CA]	0.049	[0.005, 0.128]
σ [CI]	0.091	[0.008, 0.137]
σ [BA]	0.645	[0.573, 0.720]
Ω [BACI DiD, BACI CA]	0.140	[0.010, 0.263]
Ω [BACI DiD, CI]	0.036	[-0.106, 0.176]
Ω [BACI DiD, BA]	0.798	[0.718, 0.865]
Ω [BACI CA, CI]	0.939	[0.923, 0.954]
Ω [BACI CA, BA]	-0.127	[-0.285, 0.026]
Ω [CI, BA]	-0.229	[-0.397, -0.061]

969

970

971 Table 3 – Definitions used to categorise studies based on the study design they used. See
972 also Figure 1 for visual illustration and comparison of designs. Reviews from the database
973 were not included.

Study design	Controlled?	Sampling before impact occurs?	Randomised allocation of replicates to the impact group and control group?
After	No	No	No
Before-After (BA)	No	Yes	No
Control-Impact (CI)	Yes	No	No
Before-After Control-Impact (BACI)	Yes	Yes	No
Randomised Control-Impact (R-CI)	Yes	No	Yes
Randomised Before-After Control-Impact (R-BACI)	Yes	Yes	Yes

974





