



Reproducibility and Robustness of Economics and Political Science Research

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1 Reproducibility and Robustness of Economics and 2 Political Science Research

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7 Abstract

8 This systematic and large-scale reproduction effort tests the reproducibility and
9 robustness of economics and political science, contributing to a growing literature
10 on research credibility and self-correction in science [1–4]. We reproduced origi-
11 nal analyses and conducted robustness checks of 110 articles recently published
12 in leading economics and political science journals, all of which have mandatory
13 data and code sharing policies [17,18]. We found that over 85% of published
14 claims were computationally reproducible. In robustness checks, our re-analyses
15 led to 72% of statistically significant estimates to remain significant and in the
16 same direction, and the median reproduced effect size is (nearly) the same as the
17 originally published effect size (that is, 99% of the published effect size). Addition-
18 ally, six independent research teams examined 12 pre-specified hypotheses about
19 determinants of robustness. Research teams with more experience found lower
20 levels of robustness, and robustness correlated with neither author characteristics
21 nor data availability.

22 1 Introduction

23 Science aspires to be cumulative. Reproducibility efforts strengthen science by testing
24 the reliability of published findings, promoting self-correction, and informing policy-
25 making [1]. Computational reproductions, whereby independent researchers reproduce
26 the results of published studies, are an essential diagnostic tool [2–10]. Such efforts
27 should have greater visibility [11–16]. However, there has been little social science
28 reproduction and robustness conducted at scale [10, 13, 17–23].

29 This project is a mega-reproduction led by the Institute for Replication (I4R),
30 which evaluates the reproducibility and robustness of 110 published studies in eco-
31 nomics (79) and political science (31). Our focus is on studies published in 12
32 prestigious journals between 2022 and 2023. While each of these journals has a data
33 and code availability policy requiring authors to publicly share their materials upon
34 publication, most (though not all) also appoint a dedicated data editor. This edi-
35 tor is responsible for enforcing the journal’s data and code policy and conducting
36 internal computational reproducibility checks for accepted studies (see Supplementary
37 Materials 11.5).

38 Not all studies from our targeted journals were chosen for reproduction and
39 robustness, and our sample is thus not a random representative sample of studies
40 in economics and political science. Our approach leads to an over-representation of
41 studies using publicly available data ([18]). Another feature of our sample is that the
42 targeted journals have a data availability policy *and* enforce it. This is in contrast to
43 many top field journals in both economics and political science. Our sample should
44 thus be viewed as very selective both in terms of impact and high data and code avail-
45 ability rates, and might present an optimistic upper bound on reproducibility rates.
46 In fact, virtually all papers in our sample include replication packages with cleaned
47 data and code to reproduce the paper’s results, and about 30% also provide the raw
48 data and cleaning code used to generate the analytical data (Extended Data Figure
49 1, Levels 8, 9, and 10).

50 While this project relates to the broader reproducibility movement in psychology,
51 neuroscience, or biomedicine, it distinguishes itself from notable social science repli-
52 cation efforts along four key dimensions [24–26]. First, we are mostly reproducing
53 (non-experimental) studies using the same data as the original authors. Second, we
54 assess computational reproducibility and test the robustness of estimates to alternative
55 specification choices. Because of the unique nature of the underlying studies—largely
56 non-experimental work that uses observational data—we offer the first evidence about
57 the general robustness of economics and political science. Third, we concentrate on
58 recent studies for both economics and political science. Finally, this is an ongoing
59 initiative that aims to expand across disciplines, with the goal of mass reproducing
60 studies and reshaping research norms at scale. This paper reports findings from the
61 first 110 reproductions.

62 2 Definitions

63 We follow [27]’s nomenclature and define a claim **computationally reproducible** if
64 its results can be reproduced using the original study’s data and protocols. A claim
65 is **robust** if its results are robust to alternative reasonable analytical decisions on the
66 same data. Last, a claim is **replicable** if its results can be repeated using new data.

67 3 Teams and Choice of Study

68 The reproductions and replications in this project are generated in one of two streams.
69 First, I4R has a board of editors who recommend potential reproducers. Second, I4R
70 held 11 events called replication games (Games) ([28]). Games are one-day events

71 open to faculty, post-docs, graduate students and other researchers. Participants are
72 assigned to a small team of about 3–5 other researchers all working in the same subfield
73 (*e.g.*, development economics).

74 Participant teams are offered a short list of (average 5) studies in their subfield
75 of interest about three weeks before the games. They are asked to choose a paper as
76 a team, and familiarize themselves with the data and codes publicly posted by the
77 original authors (*i.e.*, replication package) prior to the games. After the Game, teams
78 submit a standardized reproduction report summarizing their results.

79 I4R emphasizes to reproducers that the goal is *not* to show that the results are
80 not reproducible. The goal is instead to test if the claims are reproducible and robust.
81 This is key as some reproducers might engage in reverse specification searching (*i.e.*,
82 selective reporting of insignificant results). I4R stresses the importance of reasonable
83 robustness checks and recoding [29]. Re-analyses are sensible tests of the research
84 question and expected to be statistically valid and theoretically informed.

85 We survey the reasons why teams selected their paper (Extended Data Figure 2).
86 While 13.6% of teams were assigned a study (*i.e.*, did not choose which study to work
87 on), about 45% of teams report “Methods used”, 36% of teams selected “because of
88 the journal of publication” and about 25% due to the “length of time to reproduce
89 results”.

90 If a large portion of reproducers select papers based on the assumption that their
91 findings are questionable, it could skew reproducibility rates downward, as such studies
92 might be more prone to revealing problematic outcomes. However, in this project,
93 only a minimal fraction of teams indicated that they chose their paper because of *ex*
94 *ante* beliefs that main results are (not) replicable (3.6%). We found that selecting a
95 paper due to the reproducers’ belief the paper is not robust is *inversely* correlated
96 with reproducer experience ($\rho = -0.19, p < 0.000$). A few teams (5%) indicated that
97 their choice was based on statistical power/sample size and trust of original authors.

98 4 Data and Computational Reproducibility

99 We find a computational reproducibility rate of 85%. That is, when provided with
100 the original data and code, independent researchers are able to reproduce the pub-
101 lished results in economics and political science studies 85% of the time using either:
102 (1) the raw and analytical data, or; (2) the analytical data when the raw data were
103 not provided. The remaining 15% of cases involved studies with only partial availabil-
104 ity of code or data, or instances where code failed to run or produced inconsistent
105 results (See Supplementary Materials 11.8 and Extended Data Figure 1). Fixing paths,
106 missing packages and software requirements were not considered failures of computa-
107 tional reproducibility. In those instances, we fixed paths, added missing packaged and
108 software requirements.

109 Our findings suggest high rates of computationally reproducible results, but far
110 from perfect for leading journals. Our results are in contrast with several studies
111 documenting low computational reproducibility rates in economics [19]; [13]; [22]. This
112 may in part reflect the effectiveness of editorial policies in journals that have introduced
113 data editors and mandatory sharing of replication packages.

114 To provide context to these findings, we mapped data and code availability in
115 all of our target journals between 2014 and 2023. As discussed in Supplementary
116 Materials 11.11, data and code sharing practices have dramatically improved during
117 this period. We found replication folders are attached to 59% of papers in 2014, while
118 replication folder provision increases to a seemingly stable value close to 90% in 2021–
119 2023 (Extended Data Figures 3, 4, 5 and 6). Additionally, for journals that introduced
120 data editors during this period, much of this improvement occurred during the first
121 year following this change.

122 5 Robustness

123 For robustness, we directly compare original point estimates to the revised point esti-
124 mates. This one-on-one comparison allows us to speak to the robustness of a specific
125 hypothesis test, in addition to the robustness of our entire sample. We are thus looking
126 at several claims within a study and conduct robustness reproducibility and robustness
127 for multiple claims.

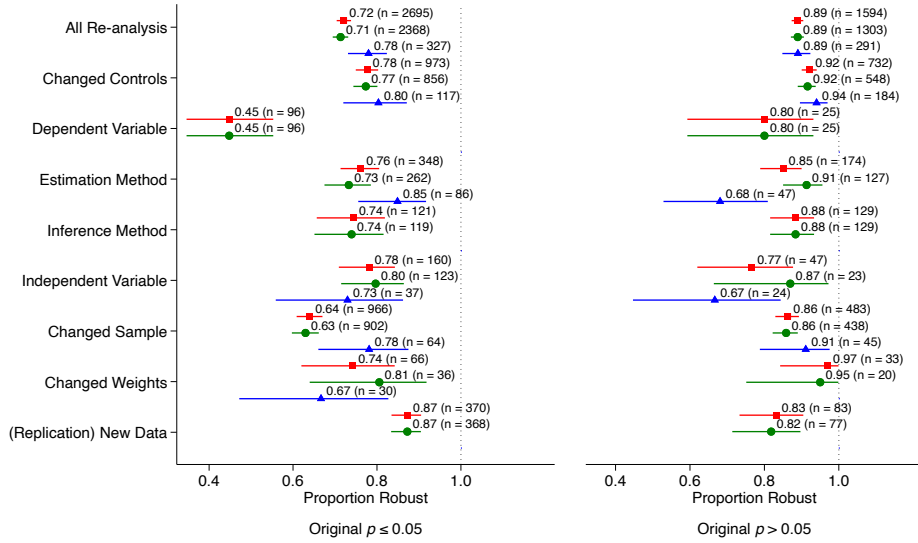
128 Reproducers are then free to conduct any robustness or recoding exercises. They
129 focus on the reproducibility of the claims and have access to the replication package,
130 allowing them to directly test the robustness of the main results. This is a crucial
131 advantage over the traditional review process as reproducers may uncover coding errors
132 and discrepancies between the paper and the codes. They may also uncover coding
133 decisions that were not discussed (or are hard to find) in the article.

134 However, this flexibility also brings some disadvantages. As with the journal review
135 process with reviewers, reproducers spend different amounts of time and effort on their
136 respective replication. Some reproducers are more experienced at coding, while others
137 are more familiar with methods, or simply unable to implement robustness checks due
138 to a lack of raw data (Extended Data Figure 7). This means that reproducibility efforts
139 and type of re-analysis vary across teams. Teams worked on average 13 active days
140 (std. dev. of 24) on the reproductions and robustness, and reports were on average 19
141 pages long (std. dev. of 14).

142 Figure 1 (top of left panel) shows a robustness rate of 72%. This result means that
143 when alternative analytical decisions were made on the same data, 72% of originally
144 statistically significant estimates ($p < 0.05$) remained statistically significant ($p <$
145 0.05) in the original direction.

146 **Figure 1.** Robustness Rate.

Fig. 1: Robustness Rate



Robustness rate for ... **Left panel:** ... originally statistically significant research **Second panel:** ... in economics **Third panel:** ... in political science **Right panel:** ... originally statistically insignificant research **All panels:** Squares, circles, and triangles represent proportions, with 95% Clopper-Pearson confidence intervals presented in whiskers. Red squares represent full sample. Green circles represent economics subsample. Blue triangles represent political science subsample. Each group of three estimates represent different types of re-analysis, non-mutually exclusive. The first 8 groups do not include re-analyses that use new data (replication), while the last one does. The first estimate group contains all types of re-analysis, then all types of re-analysis in economics, then all types of re-analysis in political science. The second represents re-analyses which changed the control variables, e.g., by adding or re-defining them. The third represents re-analyses which changed the dependent variable, e.g., by employing a different standardization or binarization. The fourth represents re-analyses which changed the estimation method, e.g., by adjusting a matching procedure. The fifth represents re-analyses which changed the inference method, e.g., changed the level on which standard errors are clustered. The sixth represents re-analyses which changed the main independent variable, e.g., by taking into account treatment intensity. The seventh represents re-analyses which changed the sample, e.g., by excluding outliers. The eighth represents re-analyses which changed the weights applied, or applied weights for the first time. The last represents replicability rates for re-analyses that introduced new data, e.g., comparable outcomes from more recent survey waves.

147 We find large differences by re-analysis type. The re-analysis type that has the
 148 highest robustness rate (78%) is changing the independent variable measure (exam-
 149 ples include log transformations, discretization, etc.). The re-analysis type that has
 150 the lowest robustness rate (45%) is any which included changing the dependent vari-
 151 able measure (e.g., categorizing the variable or log-transforming). When a replication
 152 (addition of new data, e.g., from more recent survey waves or an alternative source)
 153 is applied, the replication rate is 87%.

154 The average robustness rate is 71% and 78% for economics and political science,
 155 respectively, where the 6.7% difference is statistically significant (two-sample difference
 156 in proportions $z = -2.52$, $p = 0.012$, $n_1 = 2368$, $n_2 = 327$). The general pattern of the
 157 robustness rates is similar between economics and political science (with the exception
 158 of dependent variable and inference method, which were not applied by any of the

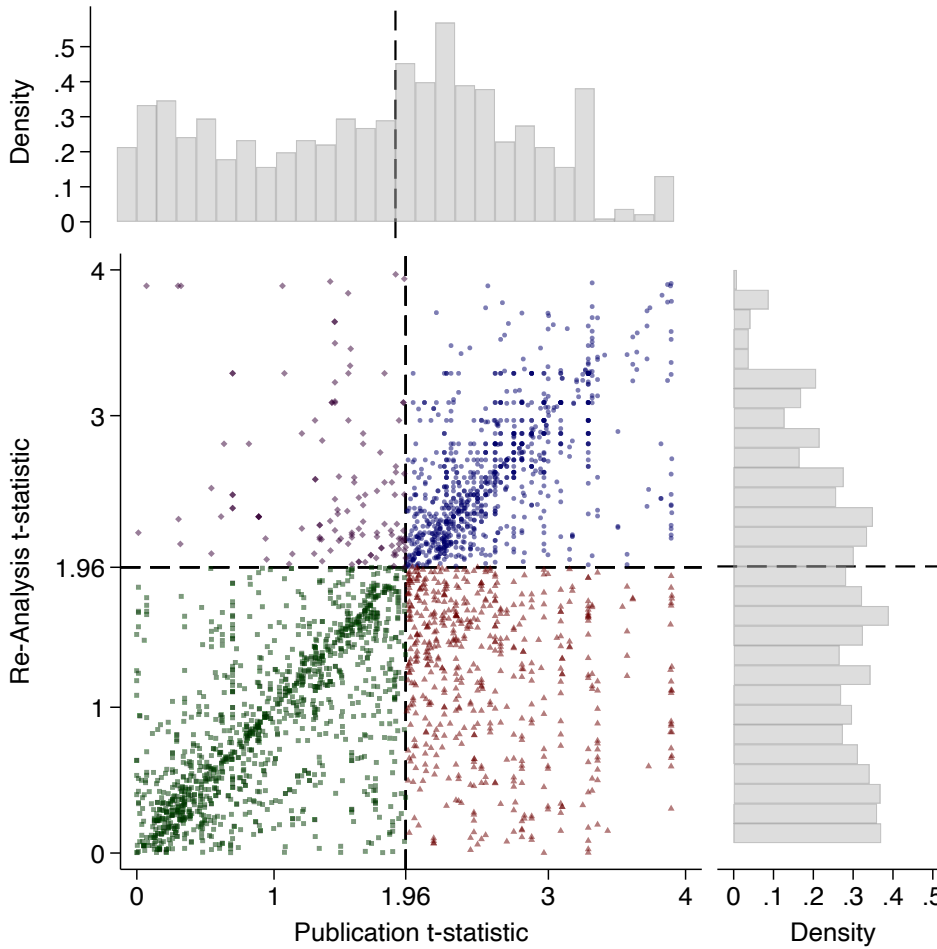
159 political science re-analyses). Focusing on robustness rates for originally statistically
160 *insignificant* findings, we find a robustness rate of 89%.

161 Supplementary Materials Appendix Table 1 shows shifts in statistical significance
162 between all significance regions. We find that 7.44% of re-analyses find an effect with
163 the opposite sign as the original result. In contrast, of the 62.84% of original analyses
164 that were statistically significant, most remained significant and of the same sign
165 (44.33%/62.84% = 70.5%). Of particular note is the 15.06% of re-analyses that find a
166 statistically *insignificant* result for originally statistically significant analyses.

167 We illustrate in Figure 2 the distribution of test statistics for the original point
168 estimates and the re-analyses. We find that 53% of the originally published test statis-
169 tics are statistically significant (to the right of the statistical significance threshold).
170 In contrast, 43% of re-analyses are statistically significant (above the statistical signif-
171 icance threshold in the vertical axis histogram). The simple difference in proportions
172 is statistically significant (difference of 10.4%, McNemar's $\chi^2 = 264.11$, $p < 0.001$,
173 $n = 4750$).

174 **Figure 2.** Statistical Significance of Publication and Re-analysis.

Fig. 2: Statistical Significance of Publication and Re-analysis



Top histogram: Distribution of publication tests of significance. T-statistics over 4 truncated for exposition. The histogram's bars are of width 0.14, with exactly 14 bars between 0 and the statistical threshold of $t = 1.96$ (corresponding to statistical significance at the 5% level). **Right histogram:** Distribution of re-analysis tests of significance. T-statistics over 4 truncated for exposition. **Scatterplot:** Each marker is a pair of test statistics, an originally published test statistic (horizontal value) and an associated re-analysis test statistic (vertical value). If the original and re-analysis test statistics were identical, this scatterplot would follow the 45 degree line. As either axis represents statistical significance, we have bifurcated each with a line at $t=1.96$, representing statistical significance at the 5% threshold. **Blue circles** indicate an originally statistically significant statistic that is also statistically significant under re-analysis. Represents 50% of sample. **Red triangles** indicate originally significant test statistics that are no longer statistically significant under re-analysis. Represents 14% of sample. **Green squares** indicate originally statistically insignificant test statistics that are the same under re-analysis. Represents 27% of sample. **Purple diamonds** indicate originally statistically insignificant test statistics that become statistically significant under re-analysis. Represents 3% of sample. **Not displayed** Not displayed are the 6% of test statistics that represent a sign reversal between the originally estimated effect and the effect estimated under re-analysis.

175 When expressed as t-statistics, the average originally published t-statistic is 1.797
176 whereas the average re-analysis t-statistic is 1.544. The difference between the pairs of
177 original study estimates and re-analysis estimates is statistically significant (Wilcoxon
178 signed-rank test $z = 15.477$, $p < 0.001$, $n = 3151$). Indeed, we reject the null hypoth-
179 esis of a two-sample Kolmogorov–Smirnov test that the two distributions come from
180 the same probability distribution ($p < 0.001$). Here, we also note the large increase in
181 test statistic density immediately after the statistical significance threshold (Extended
182 Data Figures 8 and 9), which offers strong evidence of publication bias in origi-
183 nally published research ([30, 31]). In contrast, this increase at the significance level
184 threshold is missing from the vertical axis histogram depicting the distribution of
185 re-analyses.

186 When expressed as p-values, the average originally published p-value is 0.167
187 whereas the average re-analysis p-value is 0.219; the difference is statistically significant
188 (Wilcoxon signed-rank test $z = -16.007$, $p < 0.001$, $n = 4063$).

189 In this project, we conduct multiple re-analyses per original study, and so it is
190 possible that much of the differences between original studies and their re-analyses
191 are driven or characterized by large changes in a small subset of studies rather than
192 indicative of more general shifts between original and re-analysis. In fact, we find
193 evidence of general shifts. The proportion of original studies that have at least one
194 statistically significant result is 95.3% whereas for the corresponding re-analyses this
195 is 92.9% (difference of 2.4%, McNemar’s $\chi^2 = 1.00$, $p < 0.625$, $n = 86$). Only 3.6%
196 of articles did not lose any statistical significance under replication, and the average
197 replication lost statistical significance for 29% of replication tests (median of 22%). In
198 only three original studies that reported statistically significant results, the reanalysis
199 found that all test statistics were not statistically significant.

200 6 Determinants of Robustness

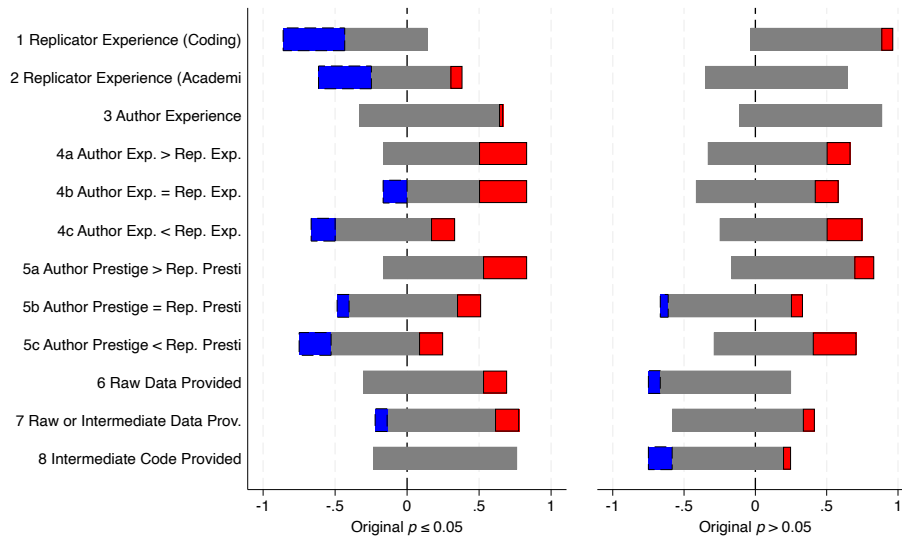
201 This section examines what, if any, characteristics of the authors, reproducers, or the
202 original articles are informative of the robustness rate.

203 While this analysis is merely exploratory, this project applied both a pre-
204 registration and many-analysts approach [32–36]. By pre-specifying which research
205 questions would be examined, and averaging the responses to those research questions
206 over multiple independent teams, the results here are guarded against specification
207 searching and confirmation bias.

208 About 110 co-authors were invited to participate regarding the proposed determi-
209 nants of robustness. We received answers from 10 individuals and ended up forming
210 six many-analysts teams. Each team answered several research questions. The results
211 are displayed in Figure 3.

212 **Figure 3.** Robustness Rate Determinants.

Fig. 3: Robustness Rate Determinants



Six independent teams answered twelve questions of the re-analysis database. Each bar represents a different question. **Left panel:** “Does reproducibility of an originally statistically significant result depend on...” **Right panel:** “Does reproducibility of an originally statistically *insignificant* result depend on...” **Both panels:** where the first bar represents “... the reproducers’ experience at coding.” **Blue, patterned outline** indicates the proportion of teams that indicated a negative and statistically significant relationship, in whichever manner the team defined so in their analysis. **Gray, no outline** indicates the proportion of teams that indicated a statistically insignificant relationship, where left of the zero line indicates negative and right of the zero line indicates positive. **Red, solid outline** indicates the proportion of teams that indicated a statistically significant and positive relationship. All teams equally weighted.

213 They began by analyzing originally statistically significant results and answer-
 214 ing the first question “Does reproducibility/replicability rate depend on reproducers’
 215 experience coding?” Specifically, most of the teams estimated a negative coefficient
 216 in a regression with reproducibility as the dependent variable and a measure of their
 217 choosing for reproducers’ experience as the primary independent variable, that is, the
 218 relationship is far more likely to be negative than positive. We interpret this result
 219 to mean that reproducers who are more experienced (broadly defined, as each of the
 220 many analysts defined experience independently) are better able to detect non-robust
 221 results in their chosen paper; likening the notion of the ‘trained eye’ of a detective
 222 finding subtle clues the untrained eye may miss at the scene. The remaining 11 pre-
 223 specified hypotheses that the analysts tested were whether reproducibility is associated
 224 with: (2) reproducers’ experience in academia, (3) the original authors’ experience in
 225 academia, whether authors have (4a) more, (4b) similar, or (4c) less experience than
 226 reproducers, (5a) more, (5b) similar, or (5c) less prestige (their institution, defined
 227 independently by the analysts) than reproducers, and whether (6) raw data was pro-
 228 vided (7) raw or intermediate data was provided, and (8) whether cleaning code was
 229 provided.

230 Among results that were originally statistically significant, the first hypothesis
231 yielded the clearest finding: the more experience a reproducer team had, the lower the
232 robustness rate they found. One plausible interpretation of our main results therefore
233 is that robustness in our full sample would likely have been lower if equally highly
234 qualified replicator teams had been assigned to each paper. However, according to
235 the results presented in the main text (Determinants of Robustness), the provision of
236 raw or intermediate data, or the necessary cleaning codes, does not seem to affect the
237 robustness of research.

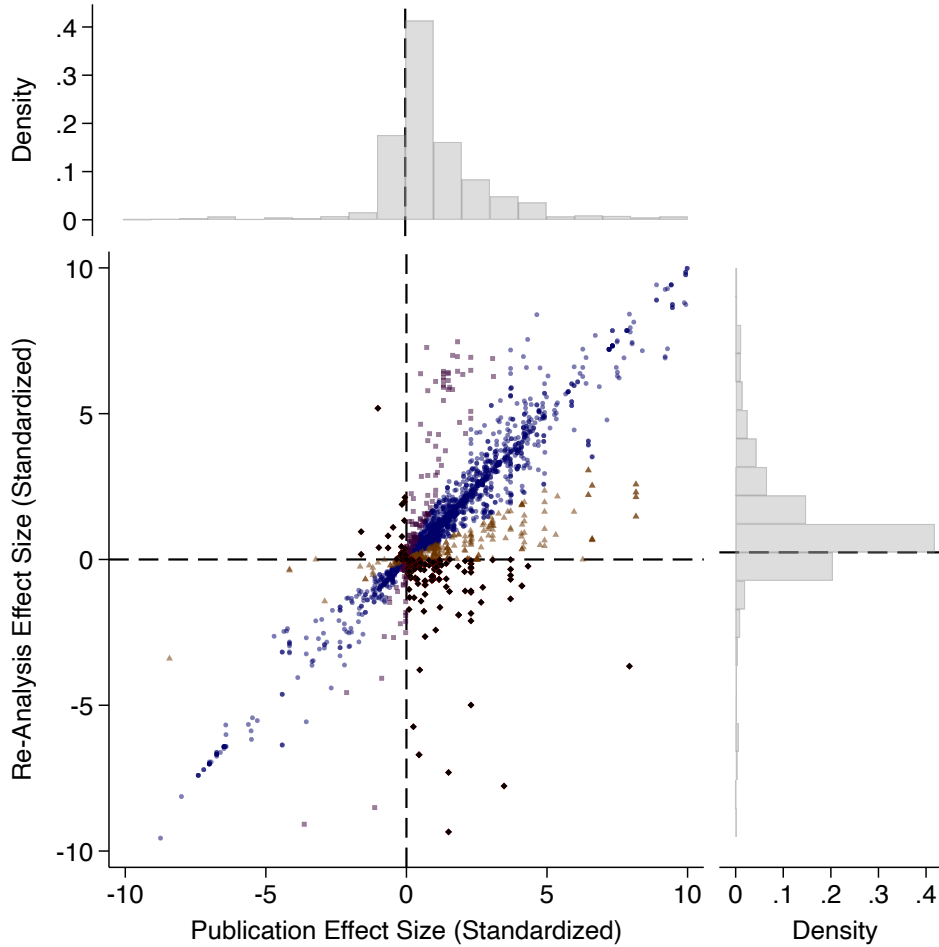
238 When analysts examined these same 12 hypotheses for originally statistically
239 *insignificant* results, the relationships are far more likely to be positive than negative,
240 but (as indicated by the proportion in gray) the relationships are often not statistically
241 significant.

242 7 Effect Size

243 Figure 4 displays publication and re-analysis effect sizes. In economics and political
244 science, effect sizes are largely reported as non-unit-less regression coefficients, whereas
245 in other sciences, effect sizes are often reported using more comparable measures such
246 as Cohen's-d. Because raw effect sizes vary widely between original studies, each of
247 the markers are standardized by the within-article average published effect size (e.g.,
248 estimated effects of 2, 4, and 6 are standardized within publication to be 0.5, 1.0, and
249 1.5).

250 **Figure 4.** Effect Size of Publication and Re-analysis.

Fig. 4: Effect Size of Publication and Re-analysis



Top histogram: Distribution of originally published effect size standardized by the average effect size within a published article. **Right histogram:** Distribution of re-analysis published effect size standardized by the average effect size within a published article. **Scatterplot:** Each marker is a pair of effect sizes, the originally published effect size (horizontal value) and an associated re-analysis effect size (vertical value). If an originally estimated and re-analysis effect size were of similar magnitude (and sign), the markers would gather tightly around the 45 degree line passing through the origin **Blue circles** indicate effect sizes which are similar (between 50% to 200% of original effect size) under re-analysis. Represents 69% of sample. **Red diamonds** indicate effect size estimates which switch sign under re-analysis. Represents 6% of sample. **Orange triangles** indicate effect size estimates which are 50% or less their original magnitude under re-analysis. Represents 9% of sample. **Purple squares** indicate effect size estimates which are double or larger than their original magnitude under re-analysis. Represents 16% of sample.

251 We find that, on average, the median effect size of a re-analysis is equivalent to
252 the published effect size (i.e., 99% the size of the published effect), while the mean
253 replicated effect is 9% larger than the original. Extended Data Figure 10 illustrates
254 the distribution effect sizes of re-analyses. This result is in stark contrast to previous
255 projects focused on replication with new data in psychology or social science exper-
256 iments uncovering replication rates ranging from 50 to 66% [24–26]. Three major
257 differences between our project and these replication efforts are that we focus on
258 robustness as opposed to replication with new data, our focus is on recent articles, and
259 that our sample is composed mostly of non-experimental studies using secondary data.

260 8 Coding Errors and Recoding

261 We investigate the prevalence of coding errors and discrepancies between the code
262 and article. Computational reproducibility pertains to the provided replication folder’s
263 ability to reproduce the exhibits and statistics displayed in the research (manuscripts,
264 appendices, *etc.*). Reproducers may be able to reproduce all exhibits exactly as they
265 appear (computationally reproducible), but the exhibits may have been constructed
266 with coding errors or discrepancies.

267 Except for minor inconveniences (*i.e.*, missing packages or broken pathways), we
268 identify coding errors in approximately 25% of the studies, with some studies contain-
269 ing multiple errors (Supplementary Materials 11.10). The prevalence of coding errors is
270 larger for economics (26%) than political science (16%). Types of errors include: defin-
271 ing the dependent variable, defining the main independent variable, defining control
272 variables, mis-specification of the estimation/model, inference or the sample. While not
273 all of these coding errors impacted the conclusions of the original studies, we uncover
274 several significant errors that warrant discussion. These major errors include instances
275 of duplicated observations on a large scale, incomplete interaction in a difference-in-
276 differences model, mislabeling the main treatment variable for a substantial number
277 (or all) of observations, and using different models, or estimators, than reported in
278 the article.

279 It is important to note that this 25% figure likely underestimates the true preva-
280 lence of coding errors. Reproducers may have missed some errors, and many replication
281 packages do not include raw data or data-cleaning code, limiting the ability to detect
282 additional issues.

283 A number of reproducers also recoded the analysis using a different statistical
284 software. Out of 23 recoding exercises, we find major differences for three studies and
285 minor differences for 10 studies. Two of the major differences were uncovered when
286 using a different software and looking at the authors’ code. Additionally, one team who
287 computationally reproduced the results using a different *version* of the software used
288 by the authors uncovered noteworthy differences in the magnitude and significance of
289 the estimates (Supplementary Materials 11.9).

290 9 Communication with Original Authors

291 I4R shares completed reproduction reports with original authors before public release
292 ([28]). Reports are reviewed typically by A.B. or another board member mainly for

293 tone and structure. I4R then disseminates the report and any author response simul-
294 taneously (see SI for the full list of reports). Reproducers may revise their reports
295 after receiving feedback from original authors.

296 About 95% of contacted authors responded (including one case where an author
297 was unreachable after leaving academia). Among respondents, 11% provided only brief
298 notes or indicated they could not respond, 59% offered informal feedback, and 30%
299 supplied a formal response. For comparison, [37] report that roughly 25% of authors
300 in their sample provided a formal response.

301 Roughly two-thirds of reproducers indicated that interactions with original authors
302 improved their reports, often by clarifying variables or procedures, supplying data or
303 data-access instructions, or helping adjust tone. In one case, original authors conducted
304 additional robustness checks in their non-public files at the reproducers' request.

305 Lastly, we assess agreement between authors and reproducers. Authors' final
306 responses were coded for whether disagreements remained after mediation; only
307 23% of articles showed any remaining disagreement. Further details appear in the
308 Supplementary Materials.

309 10 Discussion

310 A substantial information asymmetry exists between authors and the broader aca-
311 demic community, including reviewers and editors ([30]). Reviewers rarely see the
312 underlying data and code and may be unaware of crucial coding decisions, even as
313 journals routinely request multiple robustness checks. This limited visibility means
314 major errors or inconsistencies can go undetected.

315 Large-scale reproducibility initiatives offer a promising way to address these chal-
316 lenges in the social sciences and beyond. Our project provides a systematic, scalable
317 approach to evaluating reproducibility and robustness, with the goal of increasing
318 transparency and improving the credibility of published research. While stronger
319 incentives to conduct reproducibility and robustness remain necessary, we do not
320 attempt to evaluate which specific incentives would be most effective, as doing so
321 would require speculation beyond the scope of our data. Identifying the most effective
322 incentives is an important research question that we hope future work will address.

323 Given the low prevalence of diagnostic replication in published work [38], the
324 scale of this ongoing effort could shift research norms. By encouraging more rigorous
325 methodologies, deterring questionable research practices, and emphasizing collabo-
326 ration, it may help place greater weight on the reliability of results in publication
327 decisions.

328 Although our journal sample is selective, the findings are encouraging and suggest
329 a high level of computational reproducibility. These patterns—and the existence of a
330 large-scale, community-driven effort—may strengthen trust in published results.

331 We also asked reproducers about the quality of the replication packages they exam-
332 ined. Over 40% reported gaining a more optimistic view of the discipline, while only
333 about 5% developed a more negative opinion. This suggests that mass reproduction
334 of studies accompanied by replication packages can directly enhance researchers' trust
335 in scientific findings.

336 The initiative’s success and scalability have been driven by the intrinsic motivation
337 of participating researchers to support open science and improve their technical skills.
338 By late 2025, I4R had organized 80 replication games involving over 3,500 researchers,
339 with events held every other week. These efforts show that the skilled labor needed for
340 large-scale reproduction can emerge organically from an engaged research community.

341 The project also has the potential to advance science and improve equity. Publicly
342 posting data and code facilitates learning, speeds methodological diffusion, and enables
343 independent verification. Reproducing analyses in open-source software can also help
344 level the playing field for researchers who lack access to expensive licenses.

345 Our results have limitations. Only a small number of economics and political
346 science journals currently require data and code [17]; [18], and even fewer check
347 reproducibility [39]. Thus, our findings largely reflect leading journals with strong
348 data-sharing norms. Future research should assess reproducibility more broadly by
349 examining a random sample of papers from journals with and without data availability
350 policies.

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689 (University of Calgary), Christoph Huber (Aalto University), Andreas Kotsadam

692 Figure Legends

693 Figure 1: Robustness Rate. Legend: Robustness rate for ... **Left panel:** ... originally
694 statistically significant research **Second panel:** ... in economics **Third panel:** ... in
695 political science **Right panel:** ... originally statistically *insignificant* research **All**
696 **panels:** Squares, circles, and triangles represent proportions, with 95% Clopper-
697 Pearson confidence intervals presented in whiskers. Red squares represent full sample.
698 Green circles represent economics subsample. Blue triangles represent political science
699 subsample. Each group of three estimates represent different types of re-analysis,
700 non-mutually exclusive. The first 8 groups do not include re-analyses that use new
701 data (replication), while the last one does. The first estimate group contains all
702 types of re-analysis, then all types of re-analysis in economics, then all types of
703 re-analysis in political science. The second represents re-analyses which changed the
704 control variables, e.g., by adding or re-defining them. The third represents re-analyses
705 which changed the dependent variable, e.g., by employing a different standardization
706 or binarization. The fourth represents re-analyses which changed the estimation
707 method, e.g., by adjusting a matching procedure. The fifth represents re-analyses
708 which changed the inference method, e.g., changed the level on which standard errors
709 are clustered. The sixth represents re-analyses which changed the main independent
710 variable, e.g., by taking into account treatment intensity. The seventh represents re-
711 analyses which changed the sample, e.g., by excluding outliers. The eighth represents
712 re-analyses which changed the weights applied, or applied weights for the first time.
713 The last represents replicability rates for re-analyses that introduced new data, e.g.,
714 comparable outcomes from more recent survey waves.

715
716 Figure 2. Statistical Significance of Publication and Re-analysis. Legend: **Top**
717 **histogram:** Distribution of publication tests of significance. T-statistics over 4
718 truncated for exposition. The histogram's bars are of width 0.14, with exactly 14
719 bars between 0 and the statistical threshold of $t = 1.96$ (corresponding to statistical
720 significance at the 5% level). **Right histogram:** Distribution of re-analysis tests of
721 significance. T-statistics over 4 truncated for exposition. **Scatterplot:** Each marker
722 is a pair of test statistics, an originally published test statistic (horizontal value) and
723 an associated re-analysis test statistic (vertical value). If the original and re-analysis
724 test statistics were identical, this scatterplot would follow the 45 degree line. As either
725 axis represents statistical significance, we have bifurcated each with a line at $t=1.96$,
726 representing statistical significance at the 5% threshold. **Blue circles** indicate an
727 originally statistically significant statistic that is also statistically significant under
728 re-analysis. Represents 50% of sample. **Red triangles** indicate originally significant
729 test statistics that are no longer statistically significant under re-analysis. Represents
730 14% of sample. **Green squares** indicate originally statistically insignificant test
731 statistics that are the same under re-analysis. Represents 27% of sample. **Purple**
732 **diamonds** indicate originally statistically insignificant test statistics that become

733 statistically significant under re-analysis. Represents 3% of sample. **Not displayed**
734 Not displayed are the 6% of test statistics that represent a sign reversal between the
735 originally estimated effect and the effect estimated under re-analysis.

736

737 Figure 3. Robustness Rate Determinants. Legend: Six independent teams
738 answered twelve questions of the re-analysis database. Each bar represents a different
739 question. **Left panel:** “Does reproducibility of an originally statistically significant
740 result depend on...” **Right panel:** “Does reproducibility of an originally statistically
741 *insignificant* result depend on...” **Both panels:** where the first bar represents “...
742 the reproducers’ experience at coding.” **Blue, patterned outline** indicates the pro-
743 portion of teams that indicated a negative and statistically significant relationship, in
744 whichever manner the team defined so in their analysis. **Gray, no outline** indicates
745 the proportion of teams that indicated a statistically insignificant relationship, where
746 left of the zero line indicates negative and right of the zero line indicates positive.
747 **Red, solid outline** indicates the proportion of teams that indicated a statistically
748 significant and positive relationship. All teams equally weighted.

749

750 Figure 4. Effect Size of Publication and Re-analysis. Legend: **Top histogram:**
751 Distribution of originally published effect size standardized by the average effect size
752 within a published article. **Right histogram:** Distribution of re-analysis published
753 effect size standardized by the average effect size within a published article. **Scatter-**
754 **plot:** Each marker is a pair of effect sizes, the originally published effect size (horizontal
755 value) and an associated re-analysis effect size (vertical value). If an originally esti-
756 mated and re-analysis effect size were of similar magnitude (and sign), the markers
757 would gather tightly around the 45 degree line passing through the origin **Blue cir-**
758 **cles** indicate effect sizes which are similar (between 50% to 200% of original effect
759 size) under re-analysis. Represents 69% of sample. **Red diamonds** indicate effect
760 size estimates which switch sign under re-analysis. Represents 6% of sample. **Orange**
761 **triangles** indicate effect size estimates which are 50% or less their original magni-
762 tude under re-analysis. Represents 9% of sample. **Purple squares** indicate effect size
763 estimates which are double or larger than their original magnitude under re-analysis.
764 Represents 16% of sample.

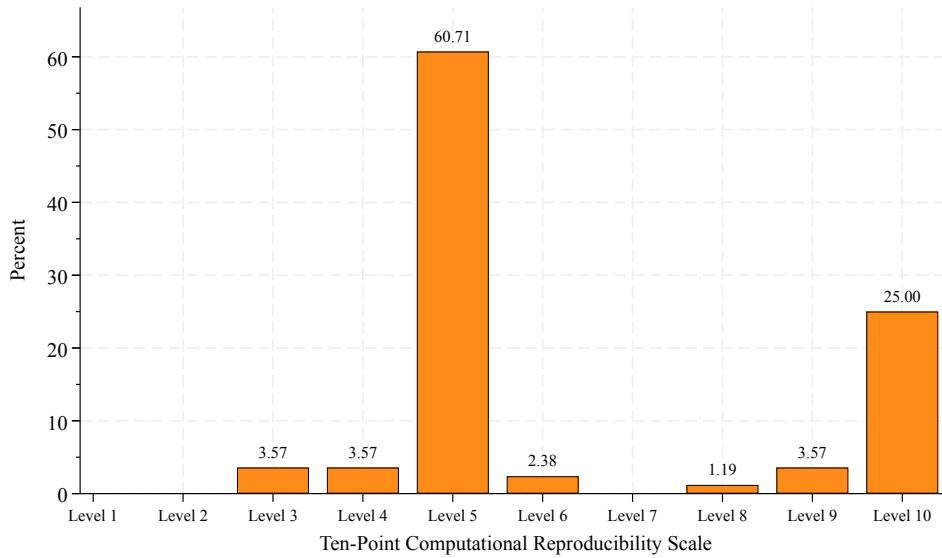
765 Extended Data Figure Legends

766 Extended Data Figure 1: 10-Point Computationally Reproducibility Score. Legend:
767 Each team assigned a reproducibility score on a scale of one to ten to the paper
768 reproduced. See Supplementary Materials for a description of each score. Level 10
769 (L10) means that all necessary materials are available and produce consistent results
770 with those presented in the paper, while level 5 (L5) means that analytic data sets
771 and analysis code are available and they produce the same results as presented in the
772 paper.

773

774 Extended Data Figure 2: Reasons Select Paper? (Select all which apply). Legend:
775 Data collected *via* survey of our reproducers after completing their reports. This

Fig. 5: 10-Point Computationally Reproducibility Score



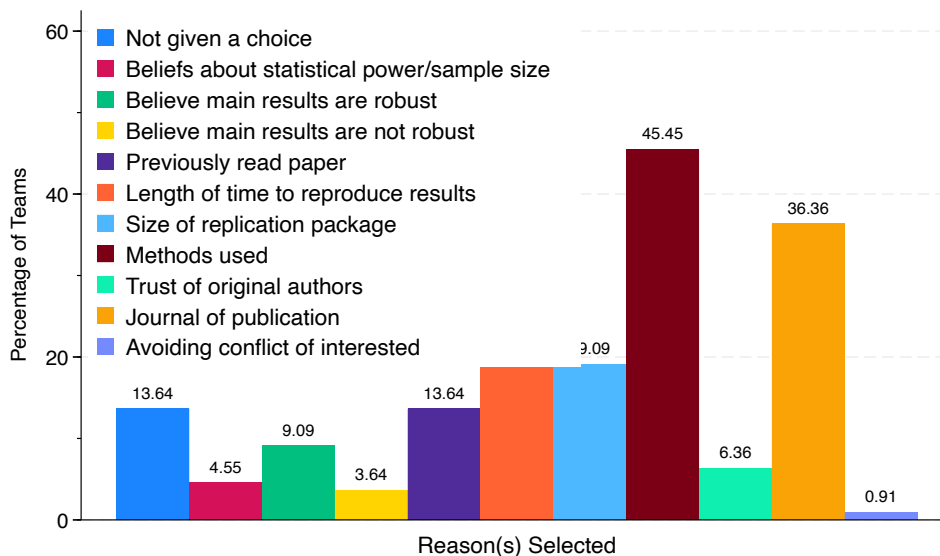
Notes: Each team assigned a reproducibility score on a scale of one to ten to the paper reproduced. See Supplementary Materials for a description of each score. Level 10 (L10) means that all necessary materials are available and produce consistent results with those presented in the paper, while level 5 (L5) means that analytic data sets and analysis code are available and they produce the same results as presented in the paper.

776 figure illustrates the responses to the question: “For what reasons did you select your
 777 specific paper to reproduce and/or replicate from the list of papers provided?
 778

779 Extended Data Figure 3: Percentage of Papers with a Replication Folder. Legend:
 780 The total sample is 1150 papers with 120 papers per year from 2019 to 2023 and 110
 781 papers per year from 2018 to 2014. Each journal has 10 papers per year except *Ameri-*
 782 *can Economic Review: Insights* which only formally became a journal in 2019 (and
 783 are omitted in earlier years). The journals sampled over correspond to those used in
 784 the manuscript’s main analysis, three from political science and nine from economics.
 785 The political science journals include: *American Journal of Political Science*, *Ameri-*
 786 *can Political Science Review*, and *Journal of Politics*. The economics journals include:
 787 *American Economic Review*, *Quarterly Journal of Economics*, *Review of Economic*
 788 *Studies*, *Journal of Political Economy*, *American Economic Journal: Macroeconomics*,
 789 *American Economic Journal: Applied Economics*, *American Economic Journal: Eco-*
 790 *nomic Policy*, *American Economic Review: Insights*, *Economic Journal*.
 791

792 Extended Data Figure 4: Percentage of Papers with a Replication Folder by
 793 Discipline. Legend: Panel (a) is for papers published in economics journals where

Fig. 6: For what reasons did you select your specific paper to reproduce and/or replicate from the list of papers provided? (Select all which apply)



Notes: Data collected *via* survey of our reproducers after completing their reports. This figure illustrates the responses to the question: “For what reasons did you select your specific paper to reproduce and/or replicate from the list of papers provided?”

794 Panel (b) is for papers published in political science. The total sample is the same
 795 as Extended Data Figure 3 is 1150 papers, where 850 papers are in the economics
 796 sample and 300 papers are in the political science sample.

797

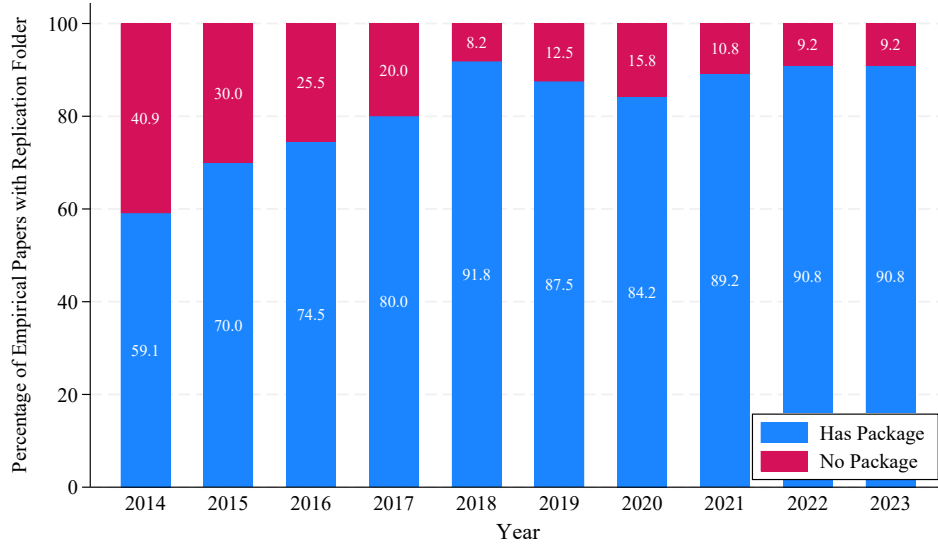
798 Extended Data Figure 5: Percentage Replication Folders’ with Contents Condi-
 799 tional on they Should Have a Replication Folder. Legend: Each subfigure represents
 800 the proportion of the replication folders which affirmatively (“Yes”) contained the
 801 variable (displayed as the title). The “Not Yes” in the legend corresponds to those
 802 replication folders which did not affirm (“No”) or had only “Some” of the required
 803 contents. Each sample is over those observations where categories are applicable (*i.e.*
 804 not all replication packages require the same contents).

805

806 Extended Data Figure 6: Percentage Replication Folders’ with Contents Condi-
 807 tional on they Should Have a Replication Folder. Legend: Each subfigure represents
 808 the proportion of the replication folders which affirmatively (“Yes”) contained the
 809 variable (displayed as the title). The “Not Yes” in the legend corresponds to those
 810 replication folders which did not affirm (“No”) or had only “Some” of the required
 811 contents. Each sample is over those observations where categories are applicable (*i.e.*
 812 not all replication packages require the same contents).

813

Fig. 7: Percentage of Papers with a Replication Folder



The total sample is 1150 papers with 120 papers per year from 2019 to 2023 and 110 papers per year from 2018 to 2014. Each journal has 10 papers per year except *American Economic Review: Insights* which only formally became a journal in 2019 (and are omitted in earlier years). The journals sampled over correspond to those used in the manuscript’s main analysis, three from political science and nine from economics. The political science journals include: *American Journal of Political Science*, *American Political Science Review*, and *Journal of Politics*. The economics journals include: *American Economic Review*, *Quarterly Journal of Economics*, *Review of Economic Studies*, *Journal of Political Economy*, *American Economic Journal: Macroeconomics*, *American Economic Journal: Applied Economics*, *American Economic Journal: Economic Policy*, *American Economic Review: Insights*, *Economic Journal*.

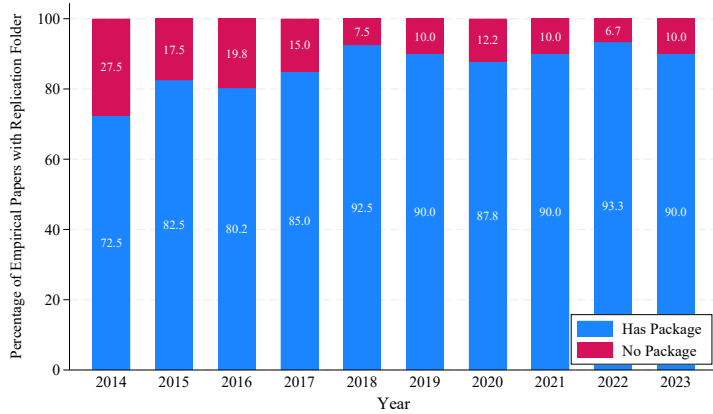
814 Extended Data Figure 7: Reasons Unable Conduct Robustness Checks. Legend:
 815 This Figure illustrates the share of teams who were unable to perform robustness
 816 checks (top-left), replications (top-right), key variable recodes (bottom-right) or
 817 extensions (bottom-left) for various reasons represented by the different coloured bars.

818
 819 Extended Data Figure 8: Distributions of t-Statistics for Original Studies and Re-
 820 Analyses. Legend: The top panels display a histogram of test statistics for $t \in [0, 5]$,
 821 with bins of width 0.1. The top left panel includes all original studies in our data set.
 822 The top right panel includes all re-analysis estimates in our data set. Vertical refer-
 823 ence lines are displayed at conventional two-tailed significance levels. We superimpose
 824 an Epanechnikov kernel (which includes renormalization at 0). The bottom figures
 825 display histograms of test statistics for p-values $\in [0.0025, 0.1500]$, with bins of width
 826 0.0025, among original studies and those from re-analyses, respectively.

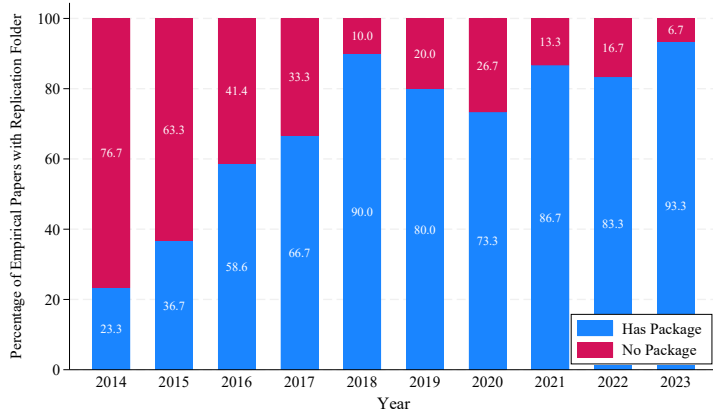
827

Fig. 8: Percentage of Papers with a Replication Folder by Discipline

(a) Economics



(b) Political Science

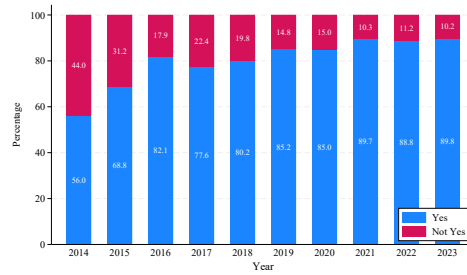


Panel (a) is for papers published in economics journals where Panel (b) is for papers published in political science. The total sample is the same as Figure 7 is 1150 papers, where 850 papers are in the economics sample and 300 papers are in the political science sample.

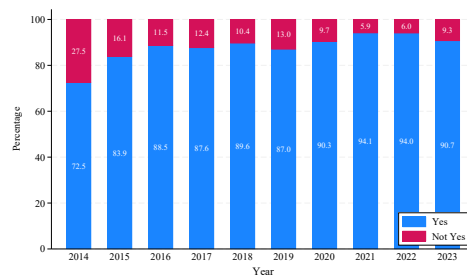
828 Extended Data Figure 9: Distributions of t -Statistics and p -values by Field.
 829 Legend: We restrict the sample to articles published in the indicated field, journals.
 830 Top panels display histograms of test statistics for $t \in [0, 5]$, with bins of width 0.1
 831 respectively. Vertical reference lines are displayed at conventional two-tailed significance
 832 levels. We superimpose an Epanechnikov kernel density curve (which includes
 833 renormalization at 0). Bottom panels display histograms of test statistics for p -values
 834 $\in [0.0025, 0.1500]$, with bins of width 0.0025.
 835

Fig. 9: Percentage Replication Folders' with Contents Conditional on they Should Have a Replication Folder

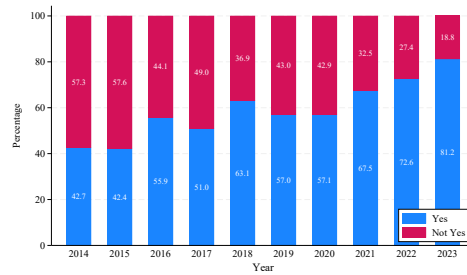
(a) README



(b) Analysis Code



(c) Cleaning Code

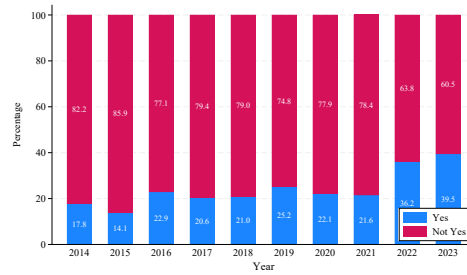


Each subfigure represents the proportion of the replication folders which affirmatively (“Yes”) contained the variable (displayed as the title). The “Not Yes” in the legend corresponds to those replication folders which did not affirm (“No”) or had only “Some” of the required contents. Each sample is over those observations where categories are applicable (*i.e.* not all replication packages require the same contents).

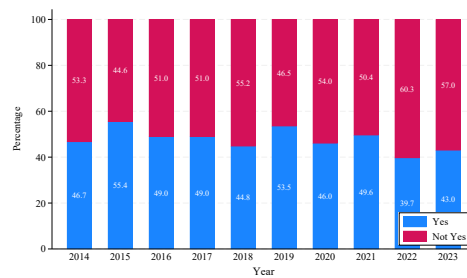
836 Extended Data Figure 10: Relative Reproduced Effect Size. Legend: 48% of rel-
 837 ative effect sizes are exactly equal to or greater than 1. This figure illustrates the
 838 ratio of re-analysis estimates and original estimates. The standardized effect sizes are
 839 normalized so that 1 equals the original effect size. A positive value indicates that the

Fig. 10: Percentage Replication Folders' with Contents Conditional on they Should Have a Replication Folder

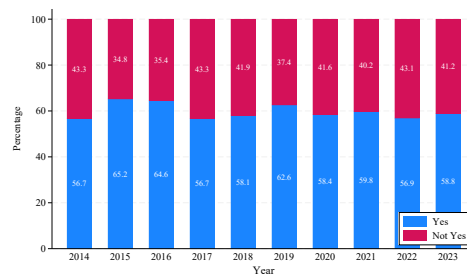
(a) Raw Data



(b) Final Data



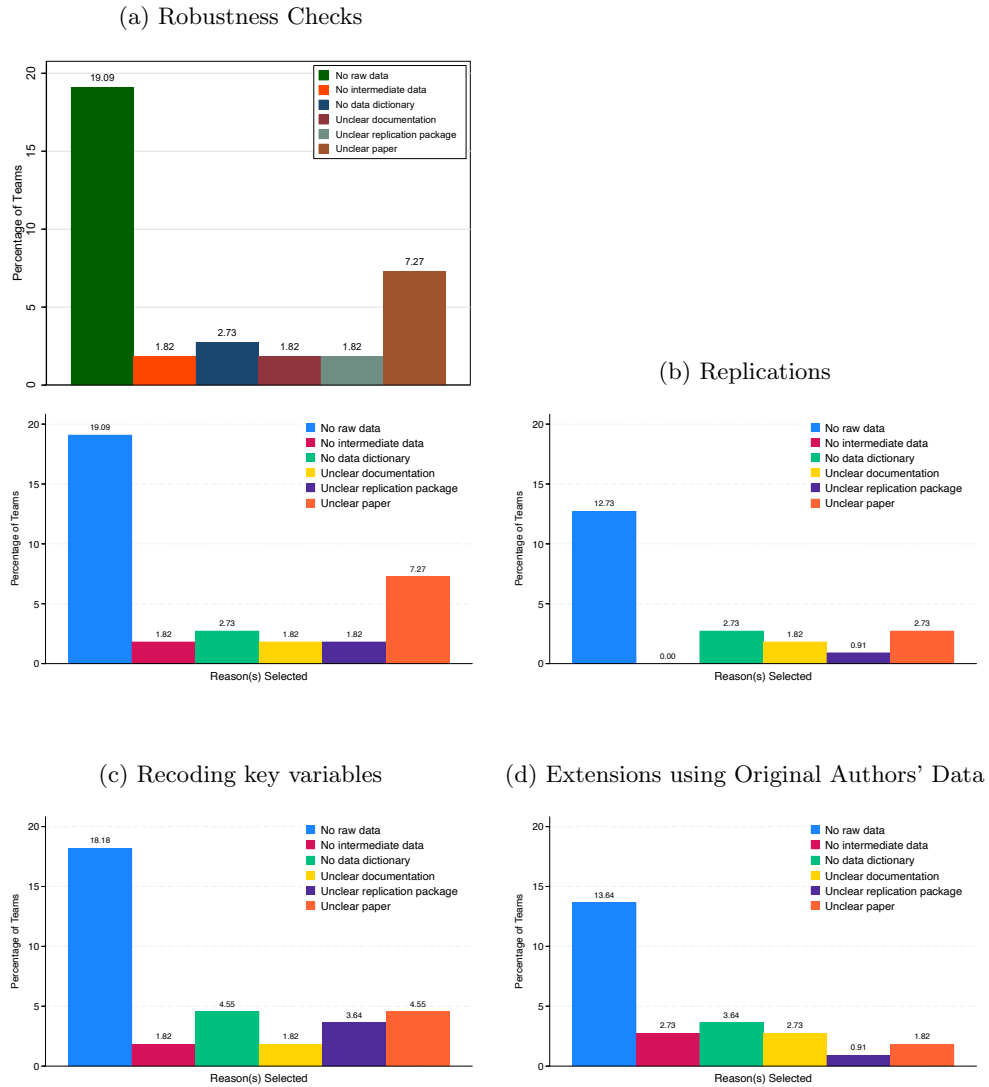
(c) Final Data + Raw or Intermediate Data with Cleaning Code



Each subfigure represents the proportion of the replication folders which affirmatively (“Yes”) contained the variable (displayed as the title). The “Not Yes” in the legend corresponds to those replication folders which did not affirm (“No”) or had only “Some” of the required contents. Each sample is over those observations where categories are applicable (*i.e.* not all replication packages require the same contents).

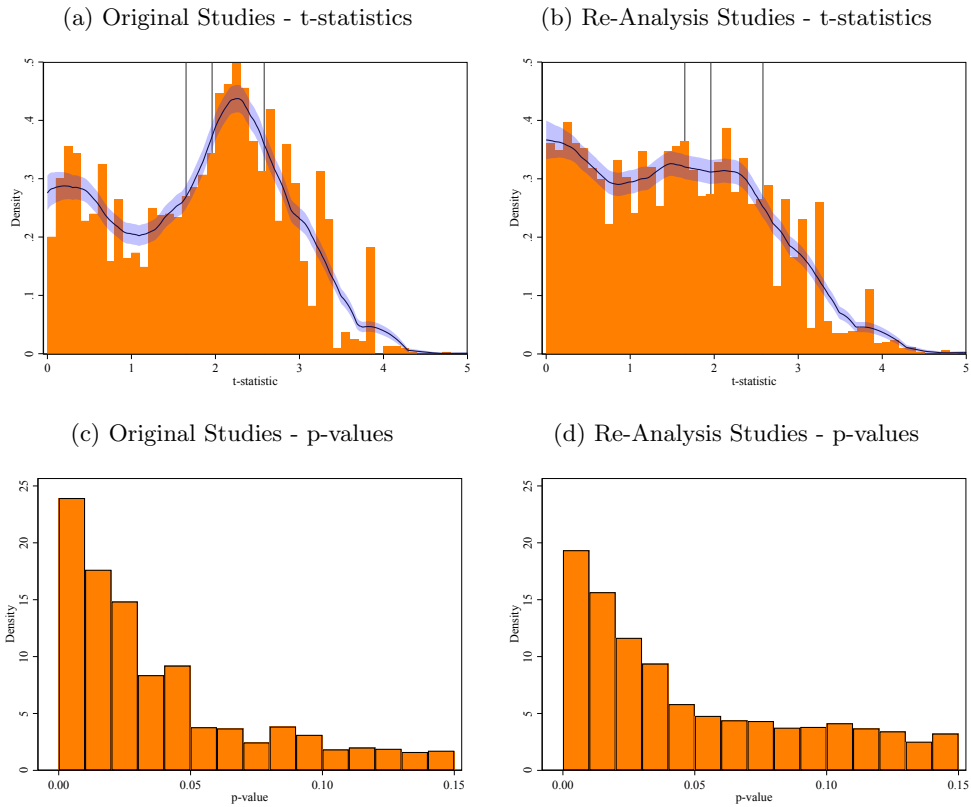
840 re-analysis estimate is in the same direction as in the original study. A negative value
 841 indicates that the re-analysis estimate is not in the same direction as in the original

Fig. 11: For which of the following reasons were you unable to conduct robustness checks, recoding exercises, extensions, or a replication using new data, prior to communications with the original authors? (Select all which apply)



Notes: This Figure illustrates the share of teams who were unable to perform robustness checks (top-left), replications (top-right), key variable recodes (bottom-right) or extensions (bottom-left) for various reasons represented by the different coloured bars.

Fig. 12: Distributions of t-Statistics for Original Studies and Re-Analyses



Notes: The top panels display a histogram of test statistics for $t \in [0, 5]$, with bins of width 0.1. The top left panel includes all original studies in our data set. The top right panel includes all re-analysis estimates in our data set. Vertical reference lines are displayed at conventional two-tailed significance levels. We superimpose an Epanechnikov kernel (which includes renormalization at 0). The bottom figures display histograms of test statistics for p-values $\in [0.0025, 0.1500]$, with bins of width 0.0025, among original studies and those from re-analyses, respectively.

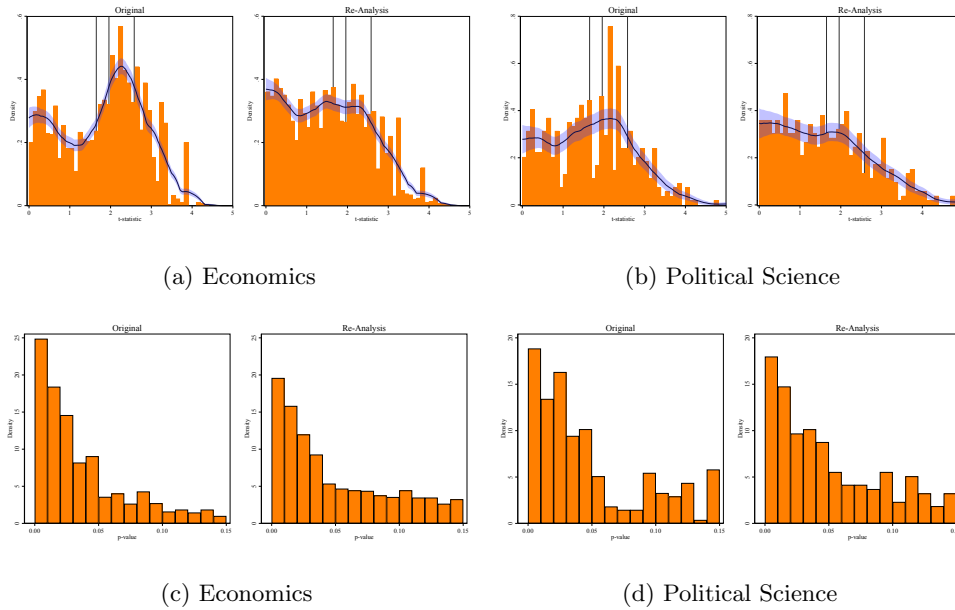
842 study. Outliers (3%) are excluded for visibility.

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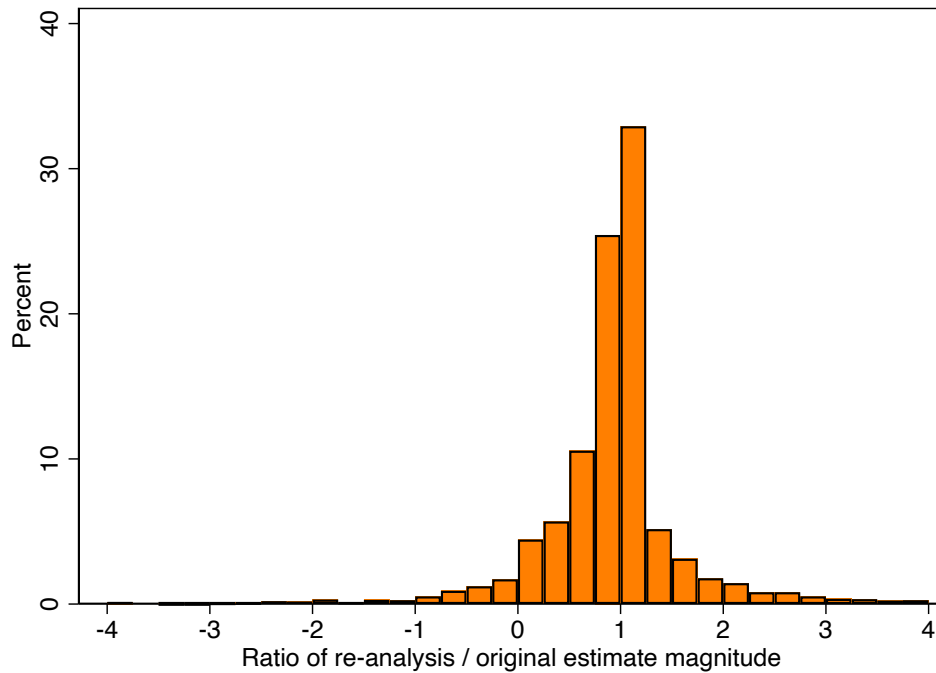
Fig. 13: Distributions of t -Statistics and p -values by Field



Notes: We restrict the sample to articles published in the indicated field. journals. Top panels display histograms of test statistics for $t \in [0, 5]$, with bins of width 0.1 respectively. Vertical reference lines are displayed at conventional two-tailed significance levels. We superimpose an Epanechnikov kernel density curve (which includes renormalization at 0). Bottom panels display histograms of test statistics for p -values $\in [0.0025, 0.1500]$, with bins of width 0.0025.

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863
864

Fig. 14: Relative Reproduced Effect Size



Notes: 48% of relative effect sizes are exactly equal to or greater than 1. This figure illustrates the ratio of re-analysis estimates and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. A positive value indicates that the re-analysis estimate is in the same direction as in the original study. A negative value indicates that the re-analysis estimate is not in the same direction as in the original study. Outliers (3%) are excluded for visibility.

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 935 [1257/pandp.110.764](https://www.aeaweb.org/articles?id=10.1257/pandp.110.764) (May 2020).

936 11 Methods

937 Our focus is on 12 journals. The journals are the following for economics: *Amer-*
 938 *ican Economic Review*, *American Economic Review: Insights*, *American Economic*
 939 *Journal: Applied Economics*, *American Economic Journal: Economic Policy*, *Ameri-*
 940 *can Economic Journal: Macroeconomics*, *The Economic Journal*, *Journal of Political*
 941 *Economy*, *Quarterly Journal of Economics*, *Review of Economic Studies*. For political
 942 science, the journals are: *American Journal of Political Science*, *American Political*
 943 *Science Review*, and *Journal of Politics*.

944 We have two streams to generate reproductions.

945 ***I4R’s Board.***

946 First, I4R has a board of editors who recommend potential reproducers. All board
 947 members are nominated by the lead author, A.B. He then reaches out to the board
 948 for suggestions of reproducers who could be a good fit for the studies in the targeted
 949 journals.

950 ***Replication Games.***

951 Our second stream to generate reproductions and replications is the replication games
 952 (Games). Games are one-day meet-ups open to faculty, post-docs, graduate students
 953 and other researchers. Participants join a small team of about 3–5 researchers all
 954 working in the same subfield (*e.g.*, development economics).

955 11.1 Types of Re-Analyses

956 We group re-analyses into eight groups: (i) alternative control variables, (ii) change
 957 the sample, (iii) change (coding of) the dependent variable, (iv) change (coding of)
 958 the main independent variable, (v) change estimation method, (vi) change inference
 959 method, (vii) change weighting scheme and (viii) replication using new data.

960 11.2 Robustness for Figures

961 While the bulk of our analysis compares coefficients and statistical significance from
962 the original study and the work of reproducers, many results in papers are also dis-
963 played in figures. For those which are plots of coefficients (i.e., event studies) we
964 encouraged reproducers to give the underlying statistics used to create the graph. This
965 was often at the discretion of the reproducers: it could be taxing to write new code
966 to compare and extract those values. In one example, the underlying programs which
967 were written by the original authors were too complicated to modify with robustness
968 checks. Excepting anecdotal examples, many teams found it feasible to reproduce a
969 figure as part of a robustness check or direct replication. In those circumstances, we
970 (A.B. and D.M.) tried to subjectively describe if we believed the results were the same.
971 This was usually taken with the discussion of the reproducers and reading the original
972 paper. We find that 189 out of 263 figures—71.9 percent—we believe to have display
973 the same result as the original paper and can be reasonably compared.

974 11.3 Non Comparable Re-Analyses

975 As mentioned earlier, a direct comparison is not possible between the original analysis
976 and the reproducers' analysis for about 15% of re-analyses. In applied microeconomics
977 and politics papers, this may be due to a change in the estimator or a change in
978 the scale of the dependent or main independent variable. There are also scenarios
979 where the original paper uses methods where coefficient estimates and p-values are
980 not the objective of the analysis. This is apparent in a few empirical macroeconomics
981 papers teams looked at. A common "robustness check" would be to adjust parameters
982 which enter a model, possibly using accepted values in the field or estimated from an
983 alternative dataset.

984 82 articles have at least one non-comparable estimate. Only a small proportion
985 (10 re-analyses) were not directly comparable for all reported re-analysis estimates.
986 For not directly comparable re-analyses, we report the proportion that reproducers
987 indicated were of the same statistical significance as the original and same sign. For our
988 four definitions of reproducibility and replication rates these are: When the original
989 estimate is statistically significant at the 5% level, 85% of those we considered not
990 directly comparable indicated their re-analysis was of the same significance (93% for
991 the 10% level). When the original estimate was not statistically significant at the 5%
992 level, 88% of those we considered not directly comparable indicated their re-analysis
993 was of the same (non)significance (92% for the 10% level).

994 11.4 Study Selection

995 Not all studies from our targeted journals have been reproduced or replicated. Our
996 approach leads to an over-representation of studies using publicly available data.
997 Another feature of our sample is that the targeted journals have a data availability
998 policy *and* enforce it. This is in contrast to many top field journals in both economics
999 and political science. Our sample should thus be viewed as very selected both in terms
1000 of impact and high data and code availability rates. In fact, approximately 45% of

1001 replication packages in our sample included raw data and complete cleaning code. An
1002 additional 13.5% provided partial cleaning code.

1003 11.5 Journal Policy

1004 The *American Journal of Political Science* does not have a data editor. Instead, the
1005 computational reproducibility is carried out by the staff at the Odum Institute for
1006 Research in Social Science, at the University of North Carolina, Chapel Hill. The jour-
1007 nals which do not conduct reproducibility checks are the *American Political Science*
1008 *Review*, the *Journal of Political Economy* and the *Quarterly Journal of Economics*.
1009 The other journals conduct computational reproducibility internally.

1010 Data editors make sure that the replication packages include the data and codes,
1011 and that the documentation (e.g., Readme) is complete. In the event that the authors
1012 cannot share some or all the data, they request that information is provided on how
1013 other researchers could obtain the data set(s). Their teams also run the codes and
1014 make sure that the output is similar to what is reported in the article. They do not
1015 look for coding errors nor run robustness checks.

1016 11.6 Many-Analysts Approach

1017 Our approach and research questions, which we detail below, were pre-registered. Our
1018 pre-analysis plan was pre-registered here: <https://osf.io/8wsqx/>. The pre-analysis plan
1019 was pre-registered prior to sharing the Meta Database with analysts. See the SI for
1020 more information on the Meta Database.

1021 The six analyst teams tackled the following eight questions:

- 1022 1. “Does reproducibility/replicability rate depend on replicators’ experience coding?”
- 1023 2. “Does reproducibility/replicability rate depend on replicators’ academic experi-
1024 ence?”
- 1025 3. “Does reproducibility/replicability rate depend on the authors’ experience?”
- 1026 4. “Does reproducibility/replicability rate depend on the interaction of the authors’
1027 experience and replicators’ experience?” In particular:
 - 1028 (a) Are reproducibility/replicability rate higher when authors’ experience is
1029 high, and replicators’ experience is low (in comparison to similar levels)?
 - 1030 (b) Are reproducibility/replicability rate higher when authors’ experience
1031 and replicators’ experience is similar (in comparison to dissimilar
1032 levels)?
 - 1033 (c) Are reproducibility/replicability rate higher when authors’ experience is
1034 low, and replicators’ experience is high (in comparison to similar levels)?
- 1035 5. “Does reproducibility/replicability rate depend on the interaction of the authors’
1036 prestige and replicators’ prestige?” In particular:
 - 1037 (a) Are reproducibility/replicability rate higher when authors’ have high
1038 prestige, and replicators’ experience have low prestige (in comparison
1039 to similar levels)?
 - 1040 (b) Are reproducibility/replicability rate higher when authors’ and replica-
1041 tors’ prestige is similar (in comparison to dissimilar levels)?

- 1042 (c) Are reproducibility/replicability rate higher when authors' have low
1043 prestige, and replicators' experience have high prestige (in comparison
1044 to similar levels)?
- 1045 6. "Does reproducibility/replicability rate depend on the original authors providing
1046 raw data?"
- 1047 7. "Does reproducibility/replicability rate depend on the original authors providing
1048 raw or intermediate data?"
- 1049 8. "Does reproducibility/replicability rate depend on the original authors providing
1050 cleaning code?"

1051 **11.6.1 Data for Analysts**

1052 Analysts were not given access to raw data (database, team leader surveys, individual
1053 surveys). Rather, they were given access to intermediate/analytical data which was
1054 cleaned and merged in a manner which would be consistent for their analysis. Giving
1055 researchers a downstream dataset allowed A.B. and D.M. to make restrictions on
1056 what the analysts could do. The clearest example of this would be defining dependent
1057 variables which were not allowed to be changed - providing a consistent definition
1058 between analysts. Asking certain research questions also restricted the data given to
1059 the analysts. These restrictions were done in ways so that any analysis done would be
1060 more comparable.

1061 The backbone of the data provided to analysts was the Meta Database, of which
1062 questions from the team leader surveys and individual surveys were added. Much of
1063 the information from the individual surveys were aggregated to the report level.

1064 The data given to the analysts changed as reproduction reports, team leader and
1065 individual surveys were completed. In total, we provided 13 updated databases for
1066 analysts between November 6th, 2023 and February 12th, 2024. We did this to give
1067 analysts time to create scripts which would work with partial datasets as we worked
1068 to gather reports and surveys. This allowed analysts to expedite their analysis once
1069 the full dataset was constructed.

1070 The goal was to have each team answer each research question independently.
1071 Each team received the same instructions and data. We allowed full flexibility to all
1072 teams. Teams were allowed to use any statistics package, statistical model, inference,
1073 weighting scheme, *etc.* Teams were free to choose the independent variables and how
1074 to code them. Teams were also free to construct their own derived variables from the
1075 dataset given to them.

1076 We provided the four dependent variables and the database to all teams. They
1077 were allowed to use any of the provided variables and new data. The only restriction
1078 imposed on teams is that they needed to use our four main dependent variables.

1079 **11.6.2 Team Construction**

1080 We asked a subset of coauthors on this paper (reproducers) if they would like to help
1081 analyze our database. We informed them that we would "have different teams inde-
1082 pendently working together at answering the same research questions (e.g., what is
1083 the reproducibility/replicability rate for each specific type of robustness checks/re-
1084 coding)." The subset of coauthors who received an invitation to volunteer were: (1)

1085 contacted between September 21st and October 8th 2023 *and* (2) had completed,
1086 or were near completion of, their reproduction report. We sent invitations (a simple
1087 sign-up form) in an email which also asked the reproducers to respond to individ-
1088 ual and team leader surveys which formed parts of our previous analysis. About
1089 110 co-authors were invited between September 21st and October 8th. 10 individuals
1090 ultimately signed-up as “many-analysts.”

1091 In our request for volunteers, we asked volunteers if they: (1) had a team who
1092 wanted to do research on the project; (2) wanted to be added to a team; (3) wanted to
1093 work on the analysis alone. No one joined as teams, most people wanted to be added
1094 to a team, and the remainder wanted to work alone. For those that wanted to work
1095 together, we assembled teams as best we could so they were close enough in timezones.
1096 We had two teams of three, one team of two, and two individuals. A.B. and D.M.
1097 also acted as a team of two, yielding six teams in total. No members of any teams left
1098 during the analysts phase.

1099 Although the PI ultimately provided each volunteer with a payment of \$3,000
1100 CAD, this compensation was not disclosed or anticipated at the time they agreed to
1101 participate.

1102 11.7 Database: Sample Composition

1103 The database described above provides 6,583 re-analyzed test statistics from 103 repro-
1104 duction reports. (Seven reports did not include robustness checks.) The other test
1105 statistics are estimates obtained by re-coding the analysis.

1106 Supplementary Materials Appendix Table 11 provides summary statistics for the
1107 full sample and by journal. In total, 83 reproduction reports were completed through
1108 Games in comparison to 27 through the editorial board stream. 79 reproduction reports
1109 are for the field of economics against 31 for political science.

1110 There is no universally agreed upon criterion for reproduction. As a first criterion,
1111 we follow much of the literature and define reproducibility as obtaining a statistically
1112 significant effect in the same direction (positive or negative) as the original study.
1113 Throughout, we rely on four main dependent variables:

1114 **First Dependent Variable:** dummy variable indicating whether the re-analysis
1115 is statistically significant at 5% level and same sign. For this dependent variable,
1116 we only keep original estimates statistically significant at the 5% level.

1117 **Second Dependent Variable:** dummy variable indicating whether the re-analysis
1118 is statistically significant at 10% level and same sign. For this dependent variable,
1119 we only keep original estimates statistically significant at the 10% level.

1120 **Third Dependent Variable:** dummy variable indicating whether the re-analysis
1121 remains not statistically significant at 5% level. For this dependent variable, we
1122 only keep original estimates statistically insignificant at the 5% level.

1123 **Fourth Dependent Variable:** dummy variable indicating whether the re-analysis
1124 remains not statistically significant at 10% level. For this dependent variable, we
1125 only keep original estimates statistically insignificant at the 10% level.

1126 The average number of re-analyzed test statistics per article is about 60. The stan-
1127 dard deviation is very high (73), with a maximum of 421. This is unsurprising given

1128 that some teams, for instance, focused most of their attention to (blindly) recoding
1129 using the raw data (either provided by the authors or re-downloaded by the repro-
1130 ducers), while other teams have focused solely on conducting robustness checks for
1131 multiple central hypotheses. As an illustrative example, imagine that an original arti-
1132 cle has three main outcome variables and relies on two main specifications. If the
1133 reproducers conduct five different robustness checks for each outcome variable and
1134 specification, then this would lead to 30 re-analyzed test statistics.

1135 As a robustness check, we deal with this issue by adjusting the weight of each test
1136 statistics by the inverse number of such statistics in the reproduction report such that
1137 each reproduction report has the same weight.

1138 Supplementary Materials Appendix Table 2 provides descriptive statistics. The
1139 articles in our sample are all recently published with a relatively small number of
1140 Google Scholar citations (44 on average) as of the completion of a reproduction report.
1141 The original authors are more experienced than reproducers with 11 years of experi-
1142 ence (*i.e.*, years since completing their Ph.D.) against 3. Original authors have on
1143 average 4,269 Google Scholar citations in comparison to 478 for reproducers. Those dif-
1144 ferences are mostly driven by the larger share of graduate students among reproducers
1145 than for original authors (49% against 6%). There are about 2.6 original authors per
1146 article in comparison to 3.2 for reproducers. About 15% of reproducers have recently
1147 published in a Top 5 or one of the three leading political science journals in our sample.
1148 Approximately 30% have published in those journals or in one of the other journals
1149 in our sample.

1150 While reproducers have less academic experience than original authors on average,
1151 their level of expertise as programmers is quite advanced. About 10%, 48% and 33%
1152 of reproducers report that their level of expertise is “Expert”, “Proficient” and “Com-
1153 petent,” respectively. Moreover, about 55% of reproducers had already produced a
1154 replication package for their own work or journal publication.

1155 11.8 Computational Reproducibility

1156 We rely on the Social Science Reproduction Platform (SSRP)’s 10-point scale to docu-
1157 ment computational reproducibility. This scale is useful as it is standardized and offers
1158 more details than a simple indicator for whether the results are computationally repro-
1159 ducible (Visit <https://bitss.github.io/ACRE/assessment.html#score> for more details
1160 on SSRP and this scale). On this scale, a rating of 1 signifies the incapacity to repro-
1161 duce results due to the absence of data or code, while a rating of 10 indicates the
1162 capability to faithfully reproduce results from the raw data (unaltered files obtained
1163 by the authors from the sources cited in the paper) to the final numerical results as
1164 published in the paper.

1165 The following is a direct reproduction from the Guide for Accelerating Computa-
1166 tional Reproducibility in the Social Sciences.

1167 **Level 1 (L1):** No data or code are available. Possible improvements include adding:
1168 raw data, analysis data, cleaning code, and analysis code.

1169 **Level 2 (L2):** Code scripts are available (partial or complete), but no data are
1170 available. Possible improvements include adding: raw data and analysis data.

1171 **Level 3 (L3):** Analytic data and code are partially available, but raw data and
1172 cleaning code are missing. Possible improvements include: completing analysis data
1173 and/or code, adding raw data, and adding analysis code.

1174 **Level 4 (L4):** All analytic data sets and analysis code are available, but the code
1175 fails to run or produces results inconsistent with the paper (not CRA). Possible
1176 improvements include: debugging the analysis code or obtaining raw data.

1177 **Level 5 (L5):** Analytic data sets and analysis code are available and they produce
1178 the same results as presented in the paper (CRA). The reproducibility package may
1179 be improved by obtaining the original raw data.

1180 Note: This is the highest level that most published research papers can attain
1181 currently. Computational reproducibility from raw data is required for papers that
1182 are reproducible at Level 6 and above.

1183 **Level 6 (L6):** Cleaning code scripts are available (partial or complete), but raw
1184 data is missing. Possible improvements include: adding raw data.

1185 **Level 7 (L7):** Cleaning code is available and complete, and raw data is partially
1186 available. Possible improvements: adding raw data.

1187 **Level 8 (L8):** All the materials (raw data, analytic data, cleaning code, and analy-
1188 sis code) are available. However, the cleaning code fails to run or produces different
1189 results from those presented in the paper (not CRR) or the analysis code fails to run
1190 or produces results inconsistent with the paper (not CRA). Possible improvements:
1191 debugging the cleaning or analysis code.

1192 **Level 9 (L9):** All the materials (raw data, analytic data, cleaning code, and anal-
1193 ysis code) are available. The analysis code produces the same output as presented
1194 in the paper (CRA). However, the cleaning code fails to run or produces differ-
1195 ent results from those presented in the paper (not CRR). Possible improvements:
1196 debugging the cleaning code.

1197 **Level 10 (L10):** All necessary materials are available and produce consistent
1198 results with those presented in the paper. The reproduction involves minimal effort
1199 and can be conducted starting from the analytic data (CRA) and the raw data
1200 (CRR). Note that Level 10 is aspirational and may be unattainable for most
1201 research published today.

1202 Each team was asked to assign a reproducibility score on a scale of one to ten to
1203 the paper reproduced. This involved documenting the completeness of the data and
1204 code, and whether the materials produce results consistent with those in the article.
1205 Their focus for computational reproducibility is only for the claims that they have
1206 investigated rather than all exhibits in the article.

1207 The results are presented in Extended Data Figure 1. This figure shows the varia-
1208 tion across papers, with the highest concentration of scores concentrated at levels 10
1209 and 5. Indeed, over 85% (Levels 5 and 10) of results examined in our sample were fully
1210 reproducible using either: (1) the raw and analytical data, or; (2) the analytical data
1211 when the raw data were not provided. Level 10 (L10) means that all necessary mate-
1212 rials are available and produce consistent results with those presented in the paper.
1213 Level 5 (L5) means that analytic data sets and analysis code are available, and they
1214 produce the same results as presented in the paper. In other words, L5 indicates that
1215 the reproducers successfully (computationally) reproduced the numerical results using

1216 the analytical data, but the raw data were not provided, while L10 indicates that the
1217 reproducers successfully (computationally) reproduced the numerical results using the
1218 raw data and cleaning and analytical codes.

1219 The remaining 15% includes studies for which analytic code and data are partially
1220 available and studies for which some of the codes (cleaning or analytic) fail to run or
1221 produce results inconsistent with the paper. These findings suggest very high rates of
1222 computationally reproducible results.

1223 Our results are in stark contrast with several studies documenting low compu-
1224 tational reproducibility rates ([13, 19, 40]). This is perhaps unsurprising given that
1225 most of the articles in our sample were already computationally reproduced by data
1226 editors. This highlights the open science movement has improved computational repro-
1227 ducibility of research findings in leading economics and political science journals. Our
1228 approach is also different as we are targeting newer studies and only articles for which
1229 (at least) analytical data were available to the teams of reproducers. A more compa-
1230 rable (and recent) study is [37] which assess the reproducibility of nearly 500 articles
1231 published in the journal *Management Science*. They find that more than 95% of arti-
1232 cles could be reproduced if data accessibility and software requirements were not an
1233 obstacle for reproducers.

1234 11.9 Recoding

1235 We now turn to recoding exercises conducted by a subset of teams. Those teams either
1236 recoded using a different software language or used the same software without looking
1237 at the original authors' code. In total, 19 teams of reproducers engaged in computa-
1238 tionally reproducing and checking for coding errors using a different statistical software
1239 than the original authors. This may be due to reproducers being more comfortable in
1240 another software language or the availability of specific commands (to run a robust-
1241 ness check). Five teams also recoded the empirical analysis without looking at the
1242 authors' code/programs.

1243 Recoding in a different software opens up the ability for others to benefit and
1244 understand the empirical foundations of published articles in ways that the original
1245 authors may not have been able to convey. For instance, verifying reproducibility
1246 by translating it into R or Python makes the study itself accessible to many more
1247 researchers.

1248 Recoding also helps to assess the importance of differing assumptions embedded
1249 within programming languages (e.g., different types of Random Number Generations,
1250 rounding rules and numerical precision). We categorized recoding exercises done by
1251 reproducers into three categories: (i) identical numerical results, (ii) minor differences,
1252 and (iii) major differences. Minor differences involve small numerical discrepancies
1253 between the authors' estimates and those obtained by the reproducers. Those dif-
1254 ferences do not lead to important changes in significance or magnitude. In contrast,
1255 major differences lead to major differences in one or multiple claims.

1256 11.10 Coding Errors and Discrepancies

1257 We now turn to documenting the prevalence of coding errors and discrepancies between
1258 the code and the published article. Of note, a paper might be fully reproducible,
1259 but the programs may contain coding errors. Similarly, there might be important
1260 discrepancies between what the article states and what the programs compute, while
1261 remaining computationally reproducible.

1262 We do not document trivial coding errors such as versioning issues and missing
1263 packages/paths. Those coding errors are typically easily fixed by the reproducers.
1264 We instead focus on coding errors which could have had an impact on claims and
1265 conclusions of articles.

1266 We uncover minor or major coding errors in 26 of the 110 studies in our sample,
1267 with some studies containing multiple errors. The errors can be broadly categorized
1268 into errors of the dependent variable (4 articles), main independent variable (5), control
1269 variables (10), estimation (2), inference (2), sample/observations (8) and other (5).
1270 While not all coding errors lead to changes in the conclusions of the original study,
1271 we uncovered several major coding errors worth discussing. Some examples of major
1272 errors include: a very large number of duplicated observations, failing to fully interact
1273 a difference-in-differences regression specification, miscoding the treatment variable
1274 for a large number of (or all) observations, and clear model misspecification.

1275 The prevalence of coding errors is larger for economics (26%) than political science
1276 (16%). A plausible explanation is that replication packages from economic articles
1277 have more lines of code than those in political science, mechanically increasing the
1278 likelihood of at least one coding error.

1279 We also uncovered transcription issues for 13 studies, typically involving small
1280 numerical differences or rounding errors not impacting the claims or conclusions of
1281 the article.

1282 11.11 Time Trends in Data and Code Availability

1283 To document time trends in data and code availability in economics and political
1284 science between 2014 and 2023, we randomly sampled 10 empirical articles per year
1285 for each of our 12 target journals. We define an article as empirical if it relies on real
1286 or simulated data at any point in the text. Thus, a theoretical article that is motivated
1287 with a descriptive analysis of labor market trends, or an econometric paper showing
1288 properties of an estimator on synthetic data would both be classified as empirical for
1289 the purposes of our study.

1290 To randomly select papers, we proceeded as follows: First, we noted the number
1291 of issues per journal per year. Second, we drew ten issues (with replacement) for each
1292 year. Third, for each issue, we generated a random permutation of numbers between
1293 1 and 35, giving us the order in which papers from a given issue should be considered.
1294 So, for example, if the first issue drawn was 4, and the first number in our permutation
1295 sequence was 10, we would consider the tenth article in the fourth issue for coding.
1296 We skipped an article and proceeded with the next number in the permutation if the
1297 article in question a) was not empirical, b) was not a standard article (we excluded
1298 comments, replies and corrections, retraction notices, and editor notes, even if they

1299 were empirical in nature), c) was a duplicate that had already been considered (e.g.,
1300 issue number one, article number five, was drawn twice in a row), or d) did not exist
1301 (our chosen journals typically publish around ten articles per issue, so higher numbers
1302 in the permutation often went unused).

1303 In our coding we considered whether the journal website or the article pdf contain a
1304 link to a replication package, whether this package is accessible, and what the contents
1305 of the package are. We tracked the availability of a Readme file, cleaning and analytical
1306 code, and raw, intermediate, and final data. Note that our coding of code availability
1307 is optimistic in the sense that we only note whether a particular type of code exists;
1308 we did not verify its completeness or correctness. However, when authors explicitly
1309 indicated that a code was incomplete, we noted this information.

1310 Of note, the *American Economic Review: Insights* only formally became a journal
1311 in 2019. For the five years earlier, we did not collect for this journal, leading to 10
1312 fewer papers per year.

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