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ABSTRACT

Mass Reproducibility and Replicability: A New Hope*

This study pushes our understanding of research reliability by reproducing and replicating claims from 110 papers in leading economic and political science journals. The analysis involves computational reproducibility checks and robustness assessments. It reveals several patterns. First, we uncover a high rate of fully computationally reproducible results (over 85%). Second, excluding minor issues like missing packages or broken pathways, we uncover coding errors for about 25% of studies, with some studies containing multiple errors. Third, we test the robustness of the results to 5,511 re-analyses. We find a robustness reproducibility of about 70%. Robustness reproducibility rates are relatively higher for re-analyses that introduce new data and lower for re-analyses that change the sample or the definition of the dependent variable. Fourth, 52% of re-analysis effect size estimates are smaller than the original published estimates and the average statistical significance of a re-analysis is 77% of the original. Lastly, we rely on six teams of researchers working independently to answer eight additional research questions on the determinants of robustness reproducibility. Most teams find a negative relationship between replicators' experience and reproducibility, while finding no relationship between reproducibility and the provision of intermediate or even raw data combined with the necessary cleaning codes.

JEL Classification: B41, C10, C81

Keywords: reproduction, replication, research transparency, open science, economics, political science

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And from nature we should learn
That all can start again
As the stars must fade away
To give a bright new day.
"Oh My Love" by Riz Ortolani - (feat. Katyna Ranieri)

1 Introduction

Reproducibility and replication efforts contribute in essential ways to the production of scientific knowledge by testing accumulated evidence.[1] Reproductions and replications assess which findings are robust, promoting self-correcting science and affecting policy-making (Vazire (2017)). Importantly, reproductions and replications emphasize that evidence is cumulative and should be assessed holistically. Active research fields appear when previous research fails to be replicated or reproduced. Yet, reproducible and replicable research increases the confidence in scientific communities confidence and our investments and innovations relying on that knowledge. Replications and reproductions in research are also foundational to teaching, ensuring that the knowledge being passed on is accurate and reliable and providing practical experiences for students. For all these reasons, reproductions and replications are considered to be an essential diagnostic tool (King (1995); Maniadis et al. (2017); Moonesinghe et al. (2007); Peterson and Panofsky (2021)) and there is broad agreement that they should be given more visibility (Brandon and List (2015); Freese and Peterson (2017); Maniadis and Tufano (2017); Munafò et al. (2017); Nosek et al. (2022)).

Yet a large literature has documented relatively low data availability and computational reproducibility rates. For around half of the papers published in leading economics journals, the data are not publicly available (Askarov et al. (2023); Brodeur et al. (Forthcoming); Christensen and Miguel (2018); Pérignon et al. (2019)) because of their nature: administrative, proprietary, or copyrighted, data. For many other studies, the required computer code is unavailable or incomplete (Dafoe (2014); Gertler et al. (2018)). Even more puzzling is that some published results cannot be fully computationally reproduced even when all required resources (data, software, hardware, *etc.*) are available (Chang and Li (2022); Pérignon et al. (2023)). Reasons put forward to explain the latter case include: lack of complete documentation, versioning issues for packages, and results which are numerically fragile.[2]

There is also growing evidence on the lack of replicability—*i.e.*, when subsequent attempts to test a hypothesis using new data yield inconsistent results—in the social sciences. A few large-scale systematic replication projects have taken place recently, including one in psychology (Open Science Collaboration (2015)), one in experimental economics (Camerer et al. (2016)) and a social science replication project (Camerer et al. (2018)). Pooling the results of these large replication projects yielded a replication rate of about 50%.

[1] See Section 2.2 for definitions of reproducibility and replicability.
[2] Using varying approaches and definitions of computational reproducibility, Chang and Li (2022), Gertler et al. (2018) and Wood et al. (2018) find, respectively, that between 14% and 43% of published studies were computationally reproducible.

This paper examines reproducibility and replicability rates for a large number of studies recently published in leading economic and political science outlets. Studying more recent studies may shed a different light on the issues discussed above. Journals are increasingly complying with specific guidelines (*i.e.*, TOP Guidelines Nosek et al. (2015)) or conducting internal reproducibility checks (Vilhuber (2020)). Support for posting data and code has increased FAIRness (Ferguson et al. (2023)): easier findability of research, easier accessibility of computational artifacts, greater clarity on how to understand the underlying data and methods, and an increase in the critical re-use of data, code, and methods.

Our project involves mass reproducing and replicating the main claims from studies published from 2022 onwards in nine leading economics outlets and three leading political science outlets. We present the results from our first 110 reproductions/replications in this piece. For each study, we work in small teams and first conduct computational reproducibility checks—the extent to which results in original studies can be reproduced using both the data and code from those studies—and document coding errors and discrepancies between the codes and the article. We then conduct robustness checks, recode the original analysis, or both, using the data provided in the original study's replication folder. Some teams also replicated the original study's findings using new data.

We document a high rate of computational reproducibility using the *Social Science Reproduction Platform*'s (SSRP) 10-point scale on computational reproducibility. This scale ranges from 1 to 10, with 1 indicating an inability to reproduce results due to missing data or code, and 10 indicating the capability to faithfully reproduce results from raw data to final numerical results. Teams assigned reproducibility scores to the papers they reproduced, focusing on the claims they investigated. The results showed a majority (over 85%) of examined results were fully reproducible using the data and code provided by the authors. The remaining 15% included studies with partial availability of analytic code and data or cases where some codes failed to run or produced inconsistent results. These findings contrast with previous studies, which uncovered low rates of computational reproducibility. This is likely influenced by our approach of targeting newer studies and nine (out of 12) outlets internally conducting computational reproducibility checks. See Section 4 for more details.

We then investigate the prevalence of coding errors and discrepancies between the code and article. Except for minor discrepancies (*i.e.*, missing packages or broken pathways), we identified coding errors in approximately one-fourth of the studies, with some studies containing multiple errors. Types of errors include: defining the dependent variable, defining the main independent variable, defining control variables, mis-specification of the estimation/model, inference and the sample. While not all of these coding errors impacted the conclusions of the original studies, we did uncover several significant errors that warrant discussion. These major errors include instances of duplicated observations on a large scale, incomplete interaction in a difference-in-differences model, mislabeling the main treatment variable for a substantial number (or all) of observations, and using a different models, or estimators, than reported in the article.

Our main analysis documents robustness reproducibility rates based on 5,511 re-analyses. The re-analyses involve specification checks such as changing the weighting scheme, changing the choice of control variables, changing estimation methods or using new data. We rely on several

definitions of robustness reproducibility throughout. Our main definition is whether the effect is in the same direction and remains statistically significant at the 5% level. Using this definition, we find a robustness reproducibility and replicability rate of about 70%. Further, we find that half of original point estimates significant at the 10% level (but insignificant at the 5% level) become statistically insignificant at the 10% threshold with our robustness checks. For original estimates significant at the 5% level (but insignificant at the 1% level), more than a quarter of re-analyses become insignificant at the 10% threshold.

We then explore heterogeneity in robustness reproducibility and replicability. We group re-analyses into eight groups. We find that robustness reproducibility rates are markedly lower when replicators change the (coding of the) dependent variable (45%) and the sample (64%). In contrast, replicability rate is the highest for re-analyses that introduced new data (87%). The remaining groups (i.e., changing control variables, estimation method, inference method, main independent variable or weighting scheme) offer robustness reproducibility rates of about 75%.

Last, we use a "many-analysts" approach where six research teams use the re-analysis data to tackle eight additional research questions (in the spirit of Silberzahn et al. (2018) and Huntington-Klein et al. (2021)). We tackle questions ranging from "Does reproducibility/replicability rate depend on replicators' academic experience or experience coding?" to "Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige?" and "Does reproducibility/replicability rate depend on the original authors providing raw data?" Each team receives the same instructions and answers each research question independently. Teams may choose to produce multiple estimates to each question, though we weight the estimates in a way that ensures all teams' results obtain equal weight. We allow full flexibility to all teams and pre-registered this exercise.

We observe a general agreement among analyst teams on the answer to some of these research questions. We provide evidence for a negative relationship between robustness reproducibility and replicators' experience (implying more experience coding leads to lower reproduction probability). A similar finding is found for replicators' academic experience, but not for original authors' experience. For their interaction, the teams find weak evidence that the reproducibility rate increases when authors have high experience relative to the replicators. Prestige (defined independently by each analyst team) has a similar pattern. The last three research questions focused on the relationship between robustness reproducibility and the original authors' degree of provision of data and code. According to the teams, the provision of raw or intermediate data, or the necessary cleaning codes, does not seem to affect the reproducibility of research.

In the course of our 110 reproductions and replications, we always engage with the original authors allowing and encouraging them to respond to our replication reports. Sharing the replication reports creates an opportunity for constructive exchange of ideas and expert feedback, which can lead to mutual learning and improvement in research practices. The vast majority of original authors engaged with our reports and 78% provided feedback to the replicators and/or wrote a formal response. We document the types of remaining disagreements in Section 2.7.

Our project differs from previous efforts in several ways. First, our focus is not solely on labo-

ratory experiments (e.g., Camerer et al. (2016)), but rather on all data types used in economics and political science research. Second, we computationally reproduce *and* conduct robustness reproducibility or replicate research findings, in contrast to a growing literature conducting large-scale computational reproducibility. Conducting sensitivity analysis on a large scale allows us to assess the stability and reliability of empirical findings. Third, we conduct a large range of recoding and specification checks, in contrast to studies focusing on one method/robustness check (e.g., Young (2022)). Fourth, our focus is on a sample of articles recently published which have potentially relied on new open science tools such as pre-analysis plans and registered reports. This is a key difference with previous work that investigated reproducibility and replicability before the open science movement.

One of our contributions is the scale of this ongoing project.[3] We believe mass reproduction and replication of research findings offers the potential to change research norms and researchers' behavior at scale. It may encourage researchers to adopt more rigorous methodologies and perhaps even act as a deterrent to questionable research practices, while also emphasizing the collaborative nature of science. In turn, it may lead to a shift in publication norms, with a strong emphasis on the reliability of results.

The rest of this paper is organized as follows. Section 2 provides a conceptual background and describes our methodology. Section 3 describes our data and provides descriptive statistics. Section 4 documents computational reproducibility rates and the prevalence of coding errors. Section 5 documents robustness reproducibility and replicability rates. Section 6 discusses our main findings and barriers to reproducibility. Section 7 discusses benefits of our approach to replicators. In Section 8, we rely on a many-analysts approach to answer additional research questions. Section 9 concludes.

2 Conceptual Background and Methodology

This section contains a brief overview of the existing literature on replication, provides our specific definitions of reproducibility and replicability, and details our mass-reproducibility methodology (including the construction of the replication reports and communication with the original authors).

2.1 Existing Literature and Incentives to Reproduce and Replicate

Concerning experimental data, several extensive replication initiatives have occurred in various fields recently. Notable examples include a project in psychology (Open Science Collaboration (2015)), one in experimental economics (Camerer et al. (2016)), and a social science replication project (Camerer et al. (2018)).[4] In this context, replication involves selecting the primary significant

[3] For economics, we are currently conducting robustness reproducibility or replicability for about 250 studies per year, or about 25% of all empirical studies published in our targeted journals. We hope to soon expand the scale of our project to include more journals, fields and types of data (e.g., hard-to-access administrative data).

[4] Other large replication projects include Errington et al. (2021) and Marcoci et al. (2023). See Nosek et al. (2022) for a review.

result from the original study and conducting the study anew on a fresh sample using comparable methods and tests (referred to as "direct replication"; see the following section for definitions). Combining the outcomes of these large-scale replication projects revealed an overall replication rate of approximately 50%.[5]

The low replicability rates for experiments can be due to many factors, including low statistical power (Arel-Bundock et al. (2022); Maniadis et al. (2014)). These factors are also present for non-experimental work. Indeed, many observational studies have been performed on small sample sizes, possibly implying low statistical power. Ioannidis et al. (2017) surveyed 159 empirical economics literatures and found that the median statistical power is 18% or less. Moreover, there are typically many ways of testing a hypothesis, giving researchers many "degrees of freedom" in their analysis. Specification searching (or "p-hacking") and publication bias have also been found to be a problem (e.g., Doucouliagos and Stanley (2011); Havránek and Sokolova (2020)). Numerous studies indicate that the prevalence of p-hacking is lower in papers employing Randomized Controlled Trials (RCTs) compared to those utilizing alternative methods of causal inference (Brodeur et al. (2016), Brodeur et al. (2020), and Vivalt (2019)). These results potentially imply that prioritizing mass reproducibility and replicability might be of greater significance for non-experimental work.[6]

Last, researchers might be tempted to select their hypotheses after the results are known (called "HARKing") on the basis of whether they yield significant results (Kerr (1998)). All these factors make it hard to disentangle true results from false positive and false negative ones.

In addition to the technical and logistical hurdles that prevent researchers from reproducing past evidence, the current publication incentives remain unfavorable to reproductions and replications (Ankel-Peters et al. (2023a); Brodeur et al. (2024); Clemens (2017); Coffman et al. (2017); Mueller-Langer et al. (2019)). Publication outlets tend to favor novel conceptual insights over new tests of a published idea, regardless of what these tests find. Another reason why journals potentially do not publish replications is that comments are hard to review and do not get a lot of citations (Ankel-Peters et al. (2023b)). Furthermore, researchers aiming to publish reproductions and replications as standalone projects may face incentives to fish for results that do not reproduce or replicate, implying that replication efforts might also suffer from p-hacking, selective reporting and other questionable research practices (Bryan et al. (2019)).

2.2 Definitions of Reproducibility and Replicability

Several definitions of reproducibility and replicability have been posited and examined (Hamermesh (2007) and Clemens (2017)). Indeed, the authors of this study do not always rely on the same definitions in their reproduction/replication as there is no consensus in the literature.[7] Dreber and

[5] A growing literature is currently developing tools and techniques for predicting replicability of research. See, for instance, Altmejd et al. (2019), Camerer et al. (2018), Fraser et al. (2023) and Yang et al. (2020).

[6] In Section 8, we document many determinants of robustness reproducibility and replicability, such as replication packages' completeness and authors' characteristics. We will further investigate determinants such as comparing articles using RCTs and those using non-experimental methods in a follow-up project.

[7] For instance, "replication" as used by many authors of this study (and researchers in economics and political science) encompasses both "reproduction" and "replication" in the conceptual framework of Dreber and Johannesson (2023).

Johannesson (2023) have recently introduced definitions and indicators which we rely on throughout this paper.[8]

Reproducibility, as delineated by Dreber and Johannesson (2023), is the examination of whether the results and conclusions of original studies can be duplicated using the original studies' data, while **replicability** is defined as using data other than what was used in the original studies.

Reproducibility is further delineated into three categories. **Computational reproducibility** gauges the extent to which results in original studies can be reproduced using both the data and code from those studies. **Recreate reproducibility** assesses the extent to which results can be reproduced using the information in the original studies without access to the processed data set and/or the analysis code. **Robustness reproducibility** explores the extent to which results in original studies remain robust to alternative plausible analytical decisions, utilizing the same data as in the original studies.

Replicability is also classified into two types. **Direct replicability** evaluates the extent to which results in original studies can be repeated on new data using the original studies' research design and analysis. This classification is further subdivided based on whether data from the same population, a similar population, or a different population is employed. **Conceptual replicability** employs new data to assess the extent to which results in original studies can be repeated; however, this type of replication involves an alternative research design and/or alternative analysis to test the same hypothesis as in the original study. Conceptual replicability is also further subdivided into the same three categories based on populations which are the same, similar or different.

There are key definitions which we will use throughout the paper that better explains the different roles of people in relationship to the Institute for Replication (henceforth I4R) and this paper.[9]

> **Original Author(s)** are the individual(s) who have published a paper in one of the targeted journals.
>
> **Original Paper** refers to the paper published in one of the targeted journals by the original authors.
>
> **Replicator(s)** are the individual(s) who have conducted a reproduction or replication of a paper written by an original author.
>
> **Replication Report** is the written report by the replicators documenting their findings while conducting the reproduction/replication of the original paper by the original authors.

2.3 Methodology

For this paper, our focus is on the following nine economic journals: (1) *American Economic Review*, (2) *American Economic Review: Insights*, (3) *American Economic Journal: Applied Economics*, (4) *Amer-*

[8] For a more comprehensive understanding of the definitions and proposed indicators of reproducibility and replicability, readers are directed to the work of Dreber and Johannesson (2023). They discuss the intricacies of each type and provide detailed indicators to be reported in conjunction with the respective reproducibility and replicability assessments.

[9] See https://i4replication.org/ for more details.

ican Economic Journal: Economic Policy and (5) *American Economic Journal: Macroeconomics*, (6) *The Economic Journal*, (7) *Journal of Political Economy*, (8) *Quarterly Journal of Economics* and (9) *Review of Economic Studies*. For political science, our focus is on three journals: (1) *American Journal of Political Science*, (2) *American Political Science Review* and (3) *Journal of Politics*. These journals were selected for multiple reasons. First, all of these journals are considered leading outlets in their respective disciplines. Second, they all have a data and code availability policy. Third, most of these journals conduct computational reproducibility checks for most accepted articles. The computational reproducibility is conducted internally by a data editor and his/her team.[10] The journals which do not conduct computational reproducibility checks are the *American Political Science Review*, the *Journal of Political Economy* and the *Quarterly Journal of Economics*. Data editors also enforce their journal data and code availability policy and enhance the completeness of the replication package. About 77% of articles in our sample were published by a journal with a data editor or a group conducting computational reproducibility such as the Odum Institute.[11]

Our sample of journals should thus be seen as highly selective. We focus on journals which enforce their data and code availability policy and are high impact. Moreover, we focus solely on studies published since 2022. Our aim is to reproduce and replicate studies as soon as they are published, as to achieve at least two goals: (i) provide a rapid assessment of the credibility of new claims and (ii) make original authors more engaged. The high response rate from original authors is perhaps indicative that focusing on more recent work make them more engaged. We come back to this point later in Section 2.7, and the representativity of our sample in Section 2.8.

2.4 Reproduction and Replication Process

Assessments of reproducibility and replicability may unfortunately gravitate towards binary judgments that declare an entire paper as "irreplicable". Our approach is similar in spirit to Hoces de la Guardia et al. (2024) as we focus on the reproducibility and replicability of each hypothesis test rather than the entire study. For our empirical analysis, we directly compare original point estimates to the revised point estimates. This one-on-one comparison allows us to speak to the reproducibility and replicability of a specific hypothesis test, in addition to the reproducibility and replicability of our entire sample. Our strategy differs from large-scale replications such as Camerer et al. (2016) along (at least) one crucial dimension; we are looking at several claims within a study and conduct robustness reproducibility or replicability for each claim.

Replicators first identify the main claims, check for computational reproducibility, and are then free to conduct any robustness or recoding exercises. This flexibility is very important as each study is different and allows for different re-analyses. For instance, some studies provide the raw data, while others only provide the final data set. Furthermore, the type of sensitivity analysis and

[10] The *American Journal of Political Science* does not have a data editor. Instead, the computational reproducibility was carried out by the staff at the Odum Institute for Research in Social Science, at the University of North Carolina, Chapel Hill.

[11] Some of the articles published in those outlets are not computationally reproduced by the data editor for a variety of reasons, including not having access to the restricted data or software. These reasons and the share of articles computationally reproduced vary across journals.

recoding that are relevant for an applied microeconomics paper using a difference-in-differences method might be different from a political science study using a regression discontinuity. We do our best to match replicators' skills and fields of expertise with papers from similar fields. Replicators reproduce or replicate a study in their primary field of interest. We provide summary statistics by types of re-analyses and field of study in the next subsections.

This flexibility in choosing which re-analyses to conduct has advantages and disadvantages. One key advantage is that we can document reproduciblity and replicability rates for different types of re-analysis.[12] Another advantage is that replicators act as "super" reviewers. They do not make a recommendation to the editor, nor do they comment on the contribution to the literature. Instead, they focus on the reproducibility of the claims and have access to the replication package, allowing them to directly test the sensitivity of the main results. This is a crucial advantage over the traditional review process as replicators may uncover coding errors and discrepancies between the paper and the codes. They may also uncover coding decisions that were not discussed (or are hard to find) in the article.

However, this flexibility also brings some disadvantages. As with the journal review process with reviewers, replicators spend different amounts of time and effort on their respective replication. Some replicators are more experienced at coding, while others are more familiar with methods. This means that not all replication reports are of the same quality. We come back to the discussion of quality in Section 6.

2.5 Generating Reproductions and Replications

We have two streams to generate reproductions and replications. All replicators are coauthors on this paper.

I4R's Board. First, I4R has a board of editors (https://i4replication.org/people.html) who recommend potential replicators. All board members are nominated by the lead author, A.B. He then reaches out to the board for suggestions of replicators who could be a good fit for the studies in the targeted journals.

Replication games. Our second stream to generate reproductions and replications is the replication games (RGs). RGs are one-day meet-ups open to faculty, post-docs, graduate students and other researchers. Participants join a small team of about 3–5 researchers all working in the same subfield (*e.g.*, development economics).[13,14]

[12] Once our sample size becomes larger, we will also be able to document replicability rates by field and method. One of our goals is to compare the importance of different robustness checks and recoding by method (e.g., removing outliers for instrumental variable estimation versus a difference-in-differences estimation).

[13] So far, teams have been as small as one individual or as large as seven.

[14] The location of RGs are chosen based on (i) local interests, (ii) geography, (iii) possibility to have the RGs as part of a major conference, and (iv) EDI considerations. See here for a list of events: https://i4replication.org/games.html. As of December 2023, we have held 15 RGs with over 700 participants in nine countries.

Participants are offered a short list of (about 5) studies in their field of interest about three weeks before the games. They are asked to choose a paper as a team, read it and familiarize themselves with the replication package prior to the games. (See Section 2.8 for the determinants of study selection.)

Teams are asked to develop a game plan for the games; each team member should know what they are supposed to do during the games.[15] Teams then have to write a (templated - https://osf.io/8dkxc/) replication report summarizing their work and results in the following months. Of note, virtually all teams kept working on their replication after the games and some even started the re-analysis prior to the games.

Participants are offered the possibility to virtually attend RGs. In our sample of completed reports, about 68% of participants attended the games in-person, while 32% virtually attended the events.[16,17]

2.6 Replication Reports

Teams have on average worked 13 active days on their reproduction or replication (std. dev. of 24). Appendix Figure 5 shows the distribution of days across reports, trimmed at over 100 days.[18] About half the teams worked from 5 to 20 days on their replication report. Most of the remaining teams worked between 25 to 85 active days.[19] Replication reports are on average 19 pages long, with a standard deviation of 14.

The goal for all replicators is clearly stated; testing whether the main claims are reproducible and robust. I4R emphasizes to replicators that the goal is NOT to show that the results are not reproducible. The goal is instead to test if the results are reproducible to recoding and/or robustness checks. This is key as some replicators might engage in reverse specification searching (i.e., selective reporting of insignificant results). Moreover, we ask replicators from I4R's Board stream to provide a pre-reanalysis plan. The game plan acts as a pre-reanalysis plan for the second stream.[20]

[15] A.B. assigns each participant to a team of about 3–5 participants based on research interests. A group of researchers may come as a pre-defined team, but this is not required. We do our best to team up graduate students with faculty members and senior researchers, ensuring a mix of junior and more senior economists in each team. A virtual meeting with the organizers before the games allows each team to ask questions and discuss a game plan. During the games, A.B., D.M. or one of I4R's co-directors, provide live assistance to each team.

[16] Most teams are fully virtual or in-person, with only a small share of teams having a mix of virtual and in-person participants. Mixed teams are typically due to a variety of reasons (*e.g.*, canceled flight for one participant), or late registrations.

[17] We asked a subset of RGs participants the following question: "Why did you choose to participate in the Replication Games?" We offered seven potential options, with an empty box to provide additional reasons. We find that a majority of respondents chose the responses "Learn about academic replications and reproductions", "Expand your network", and "Contribute to Open Science". Other popular responses include "Improve your ability to program and code" and "Improve your ability to conduct research".

[18] In terms of retention for the Replication Games, over 90% of registered participants ended up participating in the event. Furthermore, within one year of completing the first two replication games (October and November 2022), 85% of teams had completed a replication report. We hope to get to 90% completed reports over the next few months.

[19] A very small fraction worked less than 5 days. This is due to the replicators not being able to conduct robustness checks. In contrast, about 8% of teams worked more than 100 days. This is typically due to uncovering major coding errors or issues with the original study and having to engage in multiple rounds of back and forth with the original authors. There is also the potential for people to have spent many days on their paper even if the number of hours were low.

[20] In practice, some teams in both streams did not write a pre-reanalysis plan and virtually all teams that did write one

For both streams, I4R stresses the importance of reasonable robustness checks and recoding (Simonsohn et al. (2020)). Reasonable re-analyses need to be sensible tests of the research question and expected to be statistically valid. This explains why replicators were asked to focus on studies in their own field and using methods they are familiar with.[21]

2.7 Communication with Original Authors

Once a replication report is completed, A.B. reviews it if it falls within his expertise. Otherwise, someone else on I4R's board reviews the report. This review involves checking the tone and structure of the report. A.B. then shares the report with the original authors.[22] I4R's policy is to share the replication report with the original authors prior to publicly disseminating the report (Brodeur et al. (2023)). I4R then disseminate the replication report and the original authors' response simultaneously. Note that the replicators may change their replication report after receiving the original authors' response, allowing them to include their feedback. This is especially important if a re-analysis was judged unreasonable. I4R then allows the original authors to change their response as well. Of note, the replicators may remain anonymous. In practice, about 11% of replicators have decided to remain anonymous.

Original authors have been incredibly fast at providing a response, perhaps since papers being reproduced and replicated have just been published. Overall, about 95% of original authors that A.B. reached out to responded to his email.[23] Of those that responded, 11% provided a very short note (e.g., thanking replicators) or mentioned they could not respond (e.g., due to personal reasons or ongoing conflict in their country), 59% provided feedback without a formal response and 30% provided a formal response.[24] See Appendix Table 7 for a breakdown by discipline.[25]

How often do replicators and original authors agree? This is a key question as replicators have freedom to conduct any recoding or sensitivity analysis. This freedom might lead to disagreement on the validity of some re-analyses. We document (dis)agreements in multiple ways. First, authors' final responses (i.e., post-mediation) were coded as whether there remained disagreements between authors and replicators.[26] Overall, we find that there are remaining disagreements for only 23% of articles in our sample.[27] Disagreements are mostly due to the validity of the re-analyses. There

ended up deviating from it. The latter is because it is very unclear from only reading the original paper what is the range of re-analyses that is feasible. Replicators had to carefully look at the replication package provided by the authors to gauge whether specific robustness checks were implementable given data availability. Our re-analyses should thus all be considered as not pre-registered. We come back to barriers to robustness reproducibility in Section 6.1.

[21] The discussion between original authors and replicators also helped, in some instances, to resolve issues raised by the reviewers. Similarly, original authors have occasionally pointed out issues with re-analyses conducted by the replicators. See 6.2 for more information.

[22] A.B. emailed all the original authors unless there were more than 5 authors. A reminder was sent a few months later if the original authors did not respond to the initial email. If the authors did not respond to the reminder, the report was released after 6 months.

[23] This includes one author that was unreachable as he left academia.

[24] In some instances, original authors requested to see the replicators' replication package, which we provided.

[25] As a benchmark, Fišar et al. (2023) also offered original authors the possibility to provide a short formal response. Approximately 25% of authors in their sample provided a formal response.

[26] The coding was done by A.B. and three ambiguous cases were discussed at length with D.M.

[27] This percentage goes up to over 75% if we restrict the sample to articles for which the original authors wrote a formal

were no remaining disagreements on the presence of coding errors, but authors and replicators sometimes disagreed on their importance. Disagreements on the scope of the re-analyses and definition of reproducibility were quite rare, and there were also disagreements involving the tone or interpretation of the re-analyses/errors.

Overall, we observed a general lack of adversariality between original authors and replicators (Clark and Tetlock (2023)). The broad lack of adversariality is potentially due to the high rates of reproducibility and replicability, but also perhaps on the institutionalization of replications and the fact that discussion between original authors and replicators is mediated by the Institute for Replication (I4R). Moreover, original authors may feel less targeted by our replicators as our aim is to mass-reproduce and replicate studies published in leading economic and political science outlets.[28]

2.8 Study Selection

Not all studies from our targeted journals have been reproduced or replicated.[29] This brings the questions: "Which studies are being reproduced/replicated and why?"

Our approach (discussed in Section 2.3) leads to an over-representation of studies using publicly available data, and articles using either third-party surveys and own-collected data.[30] Another feature of our sample is that the targeted journals have a data availability policy *and* enforce it. This is in contrast to many top field journals in both economics and political science.[31] Our sample should thus be viewed as very selected both in terms of impact and high data and code availability rates. In fact, approximately 45% of replication packages in our sample included raw data and complete cleaning code (Appendix Figures 6 and 7). An additional 13.5% provided partial cleaning code.

We explore in the team survey the reasons why teams selected their paper. All teams answered the following question: "For what reasons did you select your specific paper to reproduce and/or replicate from the list of papers provided?" 12 options were offered, including *Other (please specify)*. Options were not mutually exclusive, so any one team could provide multiple reasons for why they selected their paper. Appendix Figure 8 summarizes the percentage of teams who selected each

response, suggesting that the majority of formal responses we obtained include some sources of disagreements.

[28] The Institute for Replication aims to replicate about 25% of all empirical studies published in eight leading economic outlets as of 2024.

[29] I4R's short run goal is to reproduce or replicate over 250 studies per year for economics and between 50-100 for political science. The 110 replication reports included in this study were the first to be completed.

[30] Brodeur et al. (Forthcoming) document for over 1,000 articles from 13 economic journals with a data availability policy that only 13% of administrative data are in articles which provide access to data and code for replication in comparison to 24% for third-party surveys and 55% for own-collected data.

[31] A.B. investigated whether studies published at the *Journal of Development Economics* (JDE) using publicly available data complied with the journal's mandatory data sharing policy. He manually checked the presence of a replication package on JDE's website for all articles published in four volumes in 2022. Out of 75 studies, 47 did not provide a replication package or mentioned that data and codes will be made available upon request. The remaining 28 studies can be categorized as follows: 13 report relying on confidential data; 14 provided a link to a replication package; and one provided only Stata codes and information on how to obtain the data. He then contacted (through I4R's email) all authors who did not provide a replication package. Seven ended up providing a package. Some authors mentioned that they did not know that the policy existed. A few mentioned that they shared the replication materials with JDE and were surprised that it was not posted. Status for each study is available here: https://i4replication.org/Journals/JDE/JDE.html.

category. Of note 13.6% of teams were assigned a study (*i.e.*, did not choose which study to work on), so they did not answer this question. About 45% of teams report "Methods used", 36% of teams selected because of the journal of publication, about 25% due to the "Length of time to reproduce results" and about 19% due to the "Size of replication package". This is in line with our provided guidelines for choosing a study (Appendix A.2).[32]

If a large portion of replicators select papers based on the assumption that their findings are questionable, it could skew reproducibility rates downward, as there's a tendency to pick studies more prone to revealing problematic outcomes. However, in this project, such a scenario does not apply. Only a minimal fraction of teams indicated that they chose their paper because of *ex ante* beliefs that main results are (not) robust/replicable (3.6%). A small share also indicated that their choice was based on statistical power/sample size (4.6%) and/or trust of original authors (6.4%). These responses suggest that the choice of paper is mostly based on methods and ease of reproduction rather than trust in the main results.

Appendix Table 8 explores if our sample is representative of all subfields within economics. We compare JEL Codes of economic papers that we reproduced or replicated relative to those of a random sample of representative journal articles published in the top 100 journals in Economics (as ranked by IDEAS/RePec). This comparison benchmark comes from Hoces de la Guardia et al. (2024). A comparison of the two samples suggest that some subfields are under-represented. Our sample under-represents, among other fields, C-Mathematical and Quantitative Methods, G-Financial Economics and F-International Economics. In contrast, the most popular JEL Codes in our sample are D-Microeconomics, J-Labor and Demographic Economics, O-Economic Development, Innovation, Technical Change, and Growth, and I-Health, Education, and Welfare.

3 Meta Database and Descriptive Statistics

In what follows, we describe the Meta Database and provide summary statistics. The main objective of this paper is to document computational reproducibility and coding errors, and robustness reproducibility/replicability in our sample. For robustness, we need to directly compare the point estimates from the original studies to the new point estimates provided in the replication reports. To do so, we build a Meta Database. The Meta Database is mainly built from three sources of raw data: (1) replication reports; (2) surveys for individual replicators; and (3) surveys for teams of replicators. We also collected information from publicly available *curricula vitae* of all original authors and replicators.

3.1 Replication Reports

Two of the lead authors (A.B. and D.M.) and research assistants read replication reports and copied test statistics into an Excel file. We also coded and grouped robustness reproducibility and repli-

[32] See Appendix Figure 9 for the percentage of teams who selected their paper based on the journal for each journal separately.

cability exercises, and information on computational reproducibility and coding errors. The work being entered by RAs was checked by A.B. or D.M. for completeness and accuracy. If any part of any entry was unclear, they were checked again and discussed.

Only a subset of results was usually considered suitable for our research. Examples of analysis not included are extensions of the original authors' research, effects by heterogeneity, or mediation analysis. These examples correspond to situations where there are no "original" estimates for which we can reasonably compare the replicators' estimates. Most often, replicators included tables and figures which were the output of a computational reproduction using the original authors' replication package. These are almost always left out.[33]

After being checked, replicators would then be contacted with their subset of the Meta Database and asked to confirm our transcribing of their reports into the Meta Database. In instances where replicators disagreed with our coding, we would exchange emails or discuss over Zoom to clarify the differences. Any issues with transcribing would be changed. We also used this interaction to confirm any results which we thought *could* be included but were not sure without further input.

We report some additional information in the Meta Database. We collect information on the journal, year of publication, number of Google Scholar citations at the time of entry into the Meta Database, the research field, the position of the test in the original article and the number of original authors and replicators.[34] We also collect information from *curricula vitae* of all the original authors and replicators. We obtained information on their academic affiliation at the time of publication, their position at the main institution and year the PhD was earned. In addition, we gather for each author and replicator the following information (at the time of completing the replication): the total number of Google Scholar citations and whether they had published in a Top-5 economic journal, a leading political science journal, and/or one of the other economic journals we are reproducing/replicating.[35]

3.2 Surveys

Replication reports and publicly available information did not completely give us the information necessary to answer all of our research questions and hypotheses. We asked all replicators to fill out an individual survey. We also asked one author per replication report to fill out a team survey. Both surveys gave additional information on the academic and programming experience of replicators, how long their report took to create and the completeness of the original authors' replication package, and whether they improved it. Teams were invited to answer the surveys following the

[33] Exceptions include p-values which were not originally presented in published paper, clear coding errors, or discrepancies between original authors' values in their published paper compared to what their replication package produces. See Section 4.4.

[34] Where some researchers do not have Google Scholars, we sometimes collect the information by hand with their corresponding publications on their *curriculum vitae*. In other circumstances, we use citations counts from *Research Gate*.

[35] The Top-5 economic journals are the *American Economic Review*, *Econometrica*, the *Journal of Political Economy*, the *Quarterly Journal of Economics* and the *Review of Economic Studies*. The leading political science journals considered here are the *American Journal of Political Science*, *American Political Science Review* and *Journal of Politics*.

completion of transcribing their report.[36]

The team survey provides additional information on data availability, computational reproducibility, the reasons the paper to be reproduced/replicated was chosen, how long it took to run the code provided in the replication package, reasons they were unable to conduct specific robustness exercises, *etc*. We also asked whether there was any communication with the original authors for clarifications and how it improved the quality of the report.

The individual survey also provides us information about whether the replicators participated in the RGs, whether they virtually attended, why they participated in the RGs and their general experience, and how it improved their networking and coding skills. We conclude the individual survey with subjective questions such as "How does the quality of the replication package affect your view of the discipline as a whole?" We provide the full set of questions to both surveys in the Appendix.

3.3 Descriptive Statistics

The Meta Database described above provides 6,583 re-analyzed test statistics from 103 replication reports. (Seven reports did not include robustness checks.) The other test statistics are estimates obtained by re-coding the analysis. We come back to those tests in Section 4.3.

Table 1 provides summary statistics for the full sample and by journal. In total, 83 replication reports were completed through RGs in comparison to 27 through the editorial board stream. 79 replication reports are for the field of economics against 31 for political science. The outlets for which we have the largest number of reports are the *Economic Journal*, the *American Economic Review*, the *American Journal of Political Science*, the *American Economic Journal: Economic Policy* and the *Journal of Politics*. This is due in large part to the higher availability of publicly available data for those journals and the presence of a data editor.

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

> **First Dependent Variable**: dummy variable indicating whether the re-analysis is statistically significant at 5% level and same sign. For this dependent variable, we only keep original estimates statistically significant at the 5% level.
>
> **Second Dependent Variable**: dummy variable indicating whether the re-analysis is statistically significant at 10% level and same sign. For this dependent variable, we only keep original estimates statistically significant at the 10% level.
>
> **Third Dependent Variable**: dummy variable indicating whether the re-analysis remains not

[36] This was also typically delayed if original authors and replicators were still having a back-and-forth about a replication report. For earlier papers, replicators were only contacted after their report was made available to the public.

statistically significant at 5% level. For this dependent variable, we only keep original estimates statistically insignificant at the 5% level.

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

The average number of re-analyzed test statistics per article is about 60. The standard deviation is very high (73), with a maximum of 421. This is unsurprising given that some teams, for instance, focused most of their attention to (blindly) recoding using the raw data (either provided by the authors or re-downloaded by the replicators), while other teams have focused solely on conducting robustness checks for multiple central hypotheses.[37] As a robustness check, we deal with this issue by adjusting the weight of each test statistics by the number of such statistics in the replication report such that each replication report has the same weight.[38]

Table 2 provides descriptive statistics. The articles in our sample are all recently published with a relatively small number of Google Scholar citations (44 on average) as of the completion of a replication report. The original authors are more experienced than replicators with 11 years of experience (*i.e.*, years since completing their Ph.D.) against 3. Original authors have on average 4,269 Google Scholar citations in comparison to 478 for replicators. Those differences are mostly driven by the larger share of graduate students among replicators than for original authors (49% against 6%). There are about 2.6 original authors per article in comparison to 3.2 for replicators.

We also collect additional information for replicators. About 15% of replicators have recently published in a Top 5 or one of the three leading political science journals in our sample. Approximately 30% have published in those journals or in one of the other journals in our sample (*e.g.*, *The Economic Journal*).

While replicators have less academic experience than original authors on average, their level of expertise as a programmer is quite advanced. Appendix Figure 10 shows about 10%, 48% and 33% of replicators report that their level of expertise is "Expert", "Proficient" and "Competent," respectively. Moreover, about 55% of replicators had already produced a replication package for their own work or journal publication (Appendix Figure 11).

3.4 Types of Re-Analyses

One of our main objectives is to document the relative importance of several robustness checks and re-analyses in impacting the magnitude and significance of the original point estimates.[39] We group

[37] As an illustrative example, imagine that an original article has three main outcome variables and relies on two main specifications. If the replicators conduct five different robustness checks for each outcome variable and specification, then this would lead to 30 re-analyzed test statistics.

[38] Another potential issue for studies documenting the extent of p-hacking and publication bias is the coarse rounding of test statistics (e.g., taking coefficient and standard error ratios as if they follow an asymptotically standard normal distribution). This is not an issue for our investigation as replicators compute p-values or t-statistics using the original authors' code.

[39] A medium run objective will be to document the impacts of each of those robustness checks by field and method. Our sample is currently too small to investigate these patterns.

the robustness checks and coding exercises conducted by the replicators into eight groups: (i) alternative control variables, (ii) changing the sample, (iii) changing the dependent variable (e.g., rescaling or using an alternative), (iv) changing the main independent variable (e.g., scaling or introducing an alternative definition), (v) changing the estimation method/model (e.g., from ordinary least squares to a probit when the outcome was a binary variable), (vi) changing the method of inference (most commonly the level of clustering but also randomization inference), (vii) change weighting scheme and (viii) replication using new data. Appendix A.5 provides a description and examples for each group. Replicators often make coding decisions which involve multiple categories simultaneously. For instance, a team of replicators may change the dependent variable, which also leads to a change in the sample size as the new dependent variable might have missing or additional values.

In practice, many replicator teams performed multiple robustness checks *simultaneously* in a single robustness exercise, or, combined two independent robustness checks into a new, third robustness check. We tracked all the changes replicators made when comparing to an original estimate and coded accordingly. In our sample, about 809 re-analyses fall into at least two categories of simultaneous robustness checks.

Table 3 provides a decomposition of reports and test statistics by type of re-analyses. The most popular re-analyses involve using alternative control variables and changing the sample. In contrast, only 14 reports had any robustness check which changed the weighting scheme and only 15 replication reports had any robustness checks which used new data.

The types of re-analyses are quite similar for economics and political science. Using alternative control variables, changing the sample and changing the estimation method/model are among the most popular re-analyses for both fields. One noticeable difference is that replicators are more likely to change the method of inference for economic articles than in political science.

4 Computational Reproducibility and Coding Errors

In this section, we first discuss replicators' expectations. We then document computational reproducibility rates and the prevalence of coding errors.

4.1 Replication Packages and Expectations

In an assessment of replicators' expectations regarding the quality of replication packages, we ask replicators the following question in the individual survey: "Which of the following best describes how the replication package aligned with your expectations". Appendix Figure 12 shows the distribution of responses. We find that more than half of replicators report that the replication package aligned reasonably with expectations, and an additional 26% of replicators indicated that the replication packages exceeded their initial expectations. Less than 10% report that the replication package was worse than expected, possibly indicating that for this small proportion of replicators, the provided materials did not meet the anticipated quality standards or may have lacked certain

elements critical for an effective replication process. Overall, we find it encouraging that most replicators found that the provided materials exceeded or aligned well with their initial expectations.

4.2 Computational Reproducibility

We first evaluate computational reproducibility in our sample. We rely on the Social Science Reproduction Platform (SSRP)'s 10-point scale to document computational reproducibility. This scale is useful as it is standardized and offers more details than a simple indicator for whether the results are computationally reproducible (Visit here for more details on SSRP and this scale). On this scale, a rating of 1 signifies the incapacity to reproduce results due to the absence of data or code, while a rating of 10 indicates the capability to faithfully reproduce results from the raw data (unaltered files obtained by the authors from the sources cited in the paper) to the final numerical results as published in the paper. Appendix Table 9 and Appendix A.3 provide a concise overview of this assessment approach, including a detailed description of each level of reproducibility.

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

The results are presented in Figure 1. This figure shows the variation across papers, with the highest concentration of scores concentrated at levels 10 and 5. Indeed, over 85% of results examined in our sample were fully reproducible using either: (1) the raw and analytical data, or; (2) the analytical data when the raw data were not provided. Level 10 (L10) means that all necessary materials are available and produce consistent results with those presented in the paper. Level 5 (L5) means that analytic data sets and analysis code are available, and they produce the same results as presented in the paper. In other words, L5 indicates that the replicators successfully (computationally) reproduced the numerical results using the analytical data, but the raw data were not provided, while L10 indicates that the replicators successfully (computationally) reproduced the numerical results using the raw data and cleaning and analytical codes.

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

Our results are in stark contrast with several studies documenting low computational reproducibility rates (Chang and Li (2022); Gertler et al. (2018); Wood et al. (2018)). This is perhaps unsurprising given that most of the articles in our sample were already computationally reproduced by data editors. This highlights the open science movement has improved computational reproducibility of research findings in leading economics and political science journals. Our approach is also different as we are targeting newer studies and only articles for which (at least) analytical data were available to the teams of replicators. A more comparable (and recent) study is Fišar et al. (2023) which assess the reproducibility of nearly 500 articles published in the journal *Manage-*

ment Science. They find that more than 95% of articles could be reproduced if data accessibility and software requirements were not an obstacle for reviewers.

Our approach also involves interacting with original authors who often help replicator teams computationally reproduce their results. We come back to this interaction between authors and replicators in Section 6.2.

4.3 Recoding

We now turn to recoding exercises conducted by a subset of teams. Those teams either recoded using a different software language or used the same software without looking at the original authors' code. In total, 19 teams of replicators engaged in computationally reproducing and checking for coding errors using a different statistical software than the original authors. This may be due to replicators being more comfortable in another software language or the availability of specific commands (to run a robustness check).[40] Five teams also recoded the empirical analysis without looking at the authors' code/programs.

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

Appendix Table 10 shows our results. Out of 24 recoding exercises, we find major differences for three studies and minor differences for 10 studies. Two of the major differences were uncovered when using a different software and looking at the authors' code.

Additionally, one team who computationally reproduced the results using a different *version* of the software used by the authors uncovered noteworthy differences in the magnitude and significance of the estimates. About half the main claims were no longer reproducible (i.e., same sign and statistically insignificant or different sign) due to a change in the defaults used by base R when generating random numbers starting in version 3.6.0.[41] This is the only instance where using a different version of the software led to major differences in the size and significance of the estimates.

These results suggest that most teams who recoded using a different software language or without looking at the authors' code could obtain similar or very similar results.

[40] Recoding in a different software also opens up the ability for others to benefit and understand the empirical foundations of published articles in ways that the original authors may not have been able to convey. For instance, while the fact that many papers in economics are in Stata or Matlab may not be too constraining within the North American and European academic sphere, it can be severely constraining only a few steps down the academic ladder. Thus, verifying reproducibility by translating it into R or Python makes the study itself accessible to many more researchers.

[41] This change is described in R version 3.6.0 release notes: https://stat.ethz.ch/pipermail/r-announce/2019/000641.html.

4.4 Coding Errors and Discrepancies

Having briefly described computational reproducibility in our sample, we now turn to documenting the prevalence of coding errors and discrepancies between the code and the published article. Of note, a paper might be fully reproducible, but the programs may contain coding errors. For example, a study may have a large number of observations erroneously duplicated, but the programs run and perfectly reproduce what is reported in the article. Similarly, there might be important discrepancies between what the article states and what the programs compute, while remaining computationally reproducible. For instance, a study may report clustering the standard errors at the state level, but clustering at a different level in the programs. Those errors would not be identified during the review process as reviewers (very) rarely have access to the data and codes during the peer review process.

In what follows, we do not document trivial coding errors such as versioning issues and missing packages/paths. Those coding errors are typically easily fixed by the replicators. We instead focus on coding errors which could have had an impact on claims and conclusions of articles.

We uncover minor or major coding errors in 26 of the 110 studies in our sample, with some studies containing multiple errors. The errors can be broadly categorized into errors of the dependent variable (4 articles), main independent variable (5), control variables (10), estimation (2), inference (2), sample/observations (8) and other (5).[42] While not all coding errors lead to changes in the conclusions of the original study, we uncovered several major coding errors worth discussing. Some examples of major errors include: a very large number of duplicated observations, failing to fully interact a difference-in-differences regression specification, miscoding the treatment variable for a large number of (or all) observations, and clear model misspecification.[43]

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

5 Changes in Statistical Significance, Effect Size, and the Reproducibility and Replicability Rate

In this section, we compare the re-analysis estimates in the meta-database against their originally published counterparts. First, we compare statistical significance both visually and with a suite of state-of-the-art tests of publication bias. Second, we compare the relative effect size of re-analysis estimates. Third, we detail how originally published estimates 'move' from statistical significance to insignificance (and vice versa) during the re-analysis process. We then identify which types of re-analysis have the best (and worst) replication rates.

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

[43] Some of these major coding errors have been 'verified' in the sense that they are now published as comments in an article's journal. Another issue not discussed here is the lack of sufficient information to re-create key dependent and independent variables for several studies.

5.1 Statistical Significance

Before visually examining a distribution of the statistical significance of re-analysis estimates, it is worth thinking about what we might expect the distribution to look like absent any distortions (such as publication bias or p-hacking). There are two common ways to present the distribution: a histogram of the associated t-statistics or a histogram of p-values. At present, there is merit to both visualizations. The t-statistic distribution has the advantage that highly statistically significant results are comparatively much less 'bunched' up in the right tail than they are in the compressed left mass of the p-curve. In contrast, the p-curve has been shown to have testable properties under a hypothesis of no p-hacking and publication bias.[44] The formal tests of publication bias and p-hacking (discussed later) make continuity and differentiability assumptions of the t-statistics distribution (e.g., the calipers of Gerber and Malhotra (2008)) and the p-curve (Elliott et al., 2022). These assumptions provide the rationale behind the discontinuity or caliper tests, where the absence of publication bias implies the absence of specific clusters of significance tests just above (in the case of t-statistics) or just below (in the case of p-values) arbitrarily defined statistical significance thresholds.

We present both t-statistics and p-curves in Figure 2. The top left panel provides the distribution of t-statistics from the *originally* published estimates. We restrict the visualization to $t \in [0, 5]$, present bins of width 0.1, and present an Epanechnikov kernel (with standard errors in blue) which softens valleys and peaks. We provide reference lines at the conventional two-tailed significance levels. To contextualize the mass of test statistics, approximately 40% of test statistics are statistically insignificant at the 10% significance level. Roughly 60%, 50%, and 25% of test statistics are significant at the 10%, 5% and 1% levels, respectively. We note especially that the distribution exhibits a peak (global maximum) just above the two-star statistical significance threshold of $t = 1.96$ and a valley before the one-star statistical significance threshold between $t = 1.0$ and $t = 1.65$. We take this as our first piece of evidence that the original studies in our sample suffer from (marginal) p-hacking and publication bias. The bottom left panel provides the equivalent p-curve for p-values $\in [0.0025, 0.1500]$, with bins of width 0.0025. We have removed $p < 0.0025$ (for a two-tailed test this is roughly $t = 3$) for illustrative purposes only, as inclusion of that mass in the left-most bar of the p-curve leads the resolution of the remaining bars to be quite low. We note that, much like the peak after $t = 1.96$ and the valley just before, the p-curve exhibits a too-tall bar just to the left of the $p = 0.05$ threshold. We also note that the bar to the left of the too-tall one is shorter, a violation of the non-increasingness which should be present in the absence of both publication bias and p-hacking (Elliott et al., 2022). Whether interpreted through the t-statistic or p-curve, we consider this to be our first piece of evidence that the sample of original studies suffers from some form of p-hacking and publication bias. We formally document the extent of p-hacking and publication bias in the original articles in Appendix A.4 which applies a suite of state-of-the-art methods for detecting p-hacking and publication bias in the presence of either. For instance, using Andrews and Kasy

[44] Several authors provide predictions based on statistical theory. For instance, Elliott et al. (2022) demonstrate that, irrespective of the distribution of true effects, the p-curve should exhibit a non-increasing and continuous pattern under the assumption of no p-hacking or publication bias (or both) across a wide range of circumstances.

(2019)'s method, we document that a not statistically significant test statistic is only 17% as likely as a (very) statistically significant test statistic to be observed (published).

We present t-and-p-curves using data from Brodeur et al. (2020) in the right panels to serve as a benchmark with which to compare the original studies.[45] The top right panel presents the distribution of t-statistics associated with hypothesis tests from articles published in 25 leading economics journals in 2015 and 2018. These articles rely on one of four popular identification methods (i.e., difference-in-differences, instrumental variable, randomized controlled trials, and regression discontinuity design). Overall, the distribution from our original studies sample is similar to that in Brodeur et al. (2020), although with visually markedly more bunching around the 5% significance threshold. This could be due to at least three reasons. First, the extent of p-hacking and publication bias might be larger in our sample. Second, replicators might focus on the most central claim(s) in original studies, while Brodeur et al. (2020) focus on all claims. Arguably, the central claim(s) could be more p-hacked or suffer from more publication bias. Third, replicators might choose to reproduce studies finding an effect or focus on replicating claims that reject the null hypothesis.

Figure 3 directly compares the distribution of test statistics for original studies and our re-analyses. Just as in Figure 2, the top panels present t-distributions while the bottom panels present p-curves, and the left panels present the original studies while the right panels now present statistical significance for the re-analyses.[46] We use this visual analysis to test whether re-analyses are less likely to reject the null hypothesis than their original counterparts. If they are, we would expect to see less of a peak (global maximum) just beyond the 5% statistical significance threshold and a shift in the mass of test statistics leftward to the statistical insignificance region, i.e., if re-analyses 're-distribute' the mass of test statistics without (or with less of) the distorting effects of publication bias or p-hacking.

Our findings are striking. Moving from left to right in the top panels - from the original to the re-analysis test statistics - there is a large shift in the mass of test statistics from the *just* statistically significant at the 5% level region to the statistically insignificant and 10% significance regions ($[0.10 > p > 0.05)$). We note this following the global maximum has shifted in mass into where the valley was, and noting also the much greater mass where $t = 0$. This visual result suggests that re-analyses decrease the statistical significance of many originally published test statistics. This is confirmed by a Kolmogorov–Smirnov test which rejects the null of equality of distributions ($p < 0.000$). A similar result emerges from visual inspection of the bottom panels which display the same statistical significance distributions using p-values. An over-abundance of just statistically significant results here is reflected in a particularly large bar just to the left of $p = 0.05$. Under the assumption of no p-hacking and publication bias the p-curve should be non-increasing - this particularly large bar is too large. We note that, in the same manner as the t-statistics no longer displaying a marked peak once they have been re-analyzed, the p-curve resulting from re-analysis is much better characterized as non-increasing (particularly at the statistical significance thresholds).

[45] See Appendix Figure 13 for another benchmark exercise using Brodeur et al. (2016).

[46] See Appendix Figures 14 and 15 for the weighted distributions. For the re-analyses, we use the inverse of the number of test statistics presented in the replication report to weigh observations.

Appendix Figures 16 and 17 reproduce the top panel of Figure 3 for economics and political science whereas Appendix Figures 18 and 19 reproduce the bottom panel for economics and political science. A reduction in the peak of t-statistics or a reduction of the p-value bar just to left of $p = 0.05$ can be seen for both economics and political science.

Appendix Figure 20 extends the visual analysis by offering a direct comparison of the statistical significance of an original estimate and its corresponding re-analysis. Depicted is a histogram of $(p_{\text{replication}} - p_{\text{original}})$ with bars of width 0.05. Interpretation of this difference-statistic is as follows. If the original estimate and its reanalysis have very similar p-values, then the difference-statistic will be close to zero. If the re-analysis p-value is high (indicating statistical insignificance) while the original p-value is low (indicating statistical significance), then this difference-statistic will add to the right tail of the distribution. Notably, this is what we see—a large proportion of re-analyses find similar p-values as the original (represented by both tall bars just above and just below zero), while we also see that the right tail (which indicates re-analyses finding a lower statistical significance on average) being much thicker than the left tail (which indicates an original study finding a lower statistical significance than its re-analysis). This trend is robust to weights (Appendix Figure 21), and is present in economics (Appendix Figure 22) as well as in political science (Appendix Figure 23).

So far, we have not distinguished between re-analyses that find an effect in the same versus opposite direction as the original estimate. This is potentially problematic if a large fraction of re-analyses finds a significant effect in the opposite direction. In Appendix Figures 24 and 25, we make this distinction. Whenever the re-analysis estimates an effect that is in the opposite direction, we assign this t-statistic (in the case of Appendix Figure 24) or p-value (in the case of Appendix Figure 25) a negative value. From both we see that the statistical significance of an original estimate with a re-analysis with an oppositely-signed effect are often statistically significant, but are also not the only drivers of the reduction in statistical significance when moving from original to re-analysis either as the positive t-statistics still exhibit the mass peak's disappearance when moving from original to re-analysis.

Overall, our graphical analysis suggest that re-analyses can lead to both increases and decreases in statistical significance, although the average effect is a reduction. In all cases, there appears to be a downward shift of an over-abundance of just marginally significant test statistics at the 5% level to the less and not statistically significant regions.

Table 4 explicitly presents the change in statistical significance from the original to a re-analysis at the test-statistic level.

For example, the first row indicates that of those original test statistics that were not statistically significant, 13.61% reversed sign during re-analysis while the majority (75%) remained statistically insignificant. Very few became more statistically significant at conventional levels, with roughly 5, 4, and 3 percent becoming statistically significant at the 10%, 5%, and 1% significance thresholds, respectively. The Total column indicates that the sum of the row values is normalized to 100%.[47]

[47] Appendix Table 11 does not make this normalization, which shows that statistical insignificance represents 31.09% of all observations. Appendix Table 12 displays test-statistic frequencies instead of proportions.

The most striking result comes from the second row (the $(0.05 < p < 0.10]$ region) for which we find that almost half (45.45%) of re-analyses become statistically insignificant while an additional 6.91% flip sign and only 28.00% remain the same level of significance. This result suggests that estimates just marginally significant at the 10% level are the most likely to lose significance.

In the third row (the $(0.01 < p < 0.05]$ region) more than a quarter (27.89%) of re-analyses become statistically insignificant, 12.06% become just marginally significant at the 10% level, 41.08% remain significant at the 5% level, and a small share (16.21%) becomes statistically significant at the 1% level.

In the fourth row (the $[0 < p < 0.01]$ region), 12.89% of re-analyses become statistically insignificant, with another 4.43% of re-analyses remaining only marginally significant at the 10% level. 8.07% fell from this highest level of statistical significance to the two-star level, while almost 70% remained statistically significant at the original level.

5.2 Relative t-Statistics

As an alternative measure of robustness reproducibility, we rely on relative t-statistics. As there can be multiple re-analysis estimates per original estimate, we first take the average of the re-analysis estimates by original estimate and take the ratio.[48] Then, in order for all re-analyses to have the same effect, we average those ratios at the re-analysis level.[49]

In the movement from original to re-analysis statistical significance, we find that on average a re-analysis finds a statistical significance around 77% the size of the original (at the paper level, 95% CI [0.72,0.83], significantly different from 100%, $p < 0.000$). This average number no doubt conceals considerable heterogeneity, which we display in Appendix Figure 26. Displayed is the distribution of the relative t-statistic between the re-analysis and original estimates (but only if the originally published estimate was statistically significant at the 5% level).

5.3 Relative Effect Size

We now turn from a comparison of statistical significance to a comparison of effect sizes between the original studies and their re-analyses (only if originally published estimates were statistically significant at the 5% level). A direct comparison is possible for most types of re-analyses, for example when replicators change the control variables, or the weighting scheme applied by the original study. We have 5,511 tests (rows/observations) which are directly comparable *and* have statistics (coefficients and p-values). Due to the freedom afforded to replicators in their re-analyses, a direct comparison is not possible for about 15.6% ($\approx 1072/6583$) of re-analyses.[50] The following analysis

[48] This aggregation provides advantages from reporting multiple correlated observations from the same claim/article (without distinguishing them from independent observations) and allows for straightforward calculation of confidence intervals.

[49] For example, if a re-analysis reproduces two original estimates with statistical significance $t_1 = 1.5$ and $t_2 = 2.0$ to find $t_1^1 = 1.3$, $t_1^2 = 1.2$, $t_2^1 = 1.8$, and $t_2^2 = 1.7$, then we would calculate a relative t-statistics of 0.833 for t_1 and 0.875 for t_2 and 0.854 overall.

[50] 423 rows have coefficients and p-values *but* are not comparable. Examples include tests where the replicators might have standardized the dependent variable, leading to the original and re-analysis coefficients being incomparable. 398

includes only those tests which are directly comparable and have coefficients and p-values.

For those re-analyses for which a direct comparison is possible (and we have statistics), we present the relative effect sizes in Figure 4 (see Appendix Figure 27 for the weighted version). By construction, the relative effect sizes are normalized so that a value of one equals the original effect size. A positive value indicates that the re-analysis estimate is in the same direction as in the original study (93%), while a negative value indicates that the re-analysis estimate is not in the same direction (7%). In our sample, the median relative effect size of the re-analyses is 0.98 and the mean is 0.95 (when winsorizing about and below by 5%). There is a large mass around one,[51] with about 17% of re-analyses smaller or equal to 0.5 and a remarkable 48% of re-analyses reporting a ratio greater than one, suggesting that many original authors were potentially conservative when publishing their point estimates.[52]

Appendix Figures 28 and 29 reproduce our Figure 4 for economics and political science to find similar patterns; the median relative effect size for economics is 0.98 against 1.00 for political science. Appendix Figure 30 presents relative effect sizes at the level of the paper to find similar results as the test-statistic level.

Overall, we find large heterogeneity in relative effect size. The evidence suggests that a large share of original authors are being conservative, while over 15% of re-analyses lead to coefficients less than half the size of the original estimate.

5.4 Robustness Reproducibility and Replicability Rate

We now turn to our analysis of the reproducibility and replicability rates. Here, we rely on four distinct definitions of reproducibility and replicability (reflecting the four dependent variables listed in Section 3.3). We begin with the definition of replicability as whether (or not) a re-analysis estimate is in the same direction as its associated original estimate and remains statistically significant at the 5% level. The second definition is similar but applies to the 10% statistical significance level. In both, we exclude original estimates that were not statistically significant at the 5% (or 10%) level.

Table 5 presents our reproducibility rates (see Appendix Table 13 for the number of re-analyses in addition to the rates and Appendix Table 14 for article weighted rates).

In the first row, we show that for the full sample 71% of original results that were significant at the 5% level had a re-analysis estimate that was of the same sign and retained statistical significance at the 5% level. In the second column, which includes re-analyses that *at least* changes the control variables, this rate increases to 76%. The type of robustness check which offers the lowest replication rate was changing the dependent variable (where only 45% of originally significant estimates survived) in comparison to the approximately 75% seen for most other types of robustness checks. The third row finds a similar result for estimates originally significant at the 10% level.

rows do not have statistics *but* are comparable. Examples include figures (see Section 5.6 on **Non Comparable Re-Analysis**) or non-estimated target parameters which contribute importantly to the arguments in the original paper. In this case it is clear to the replicators, A.B and D.M, that results are the same or differing. In 251 rows, results are neither comparable *nor* are there statistics.

[51] A two-tailed t-test comparing the relative effect sizes to a hypothesized mean of one returns $p = 0.114$.

[52] At the article level, the average relative effect size is 97% (95% CI [0.89,1.07] not statistically different from 1, $p = 0.6172$)

In the second row, we show that for the full sample 88% of original results that were not significant at the 5% level had a re-analysis estimate that was of the same sign and retained statistical *in*significance at the 5% level. We now see that for originally statistically insignificant results, the replicability rate seems to be around 90% (as compared to the mid 70's of statistically significant ones), regardless of the type of robustness check applied (even the dependent variable, which reduced the replication rate of statistically significant original results by almost half). This trend continues in the fourth row which examines re-analysis and originally not statistically significant results at the 10% level - again with replication rates around 90%. While this means that the remaining approximately 10% of re-analyses become statistically significant, we note with interest the very different replication rates between originally statistically significant results, and statistically insignificant results.

Our rates of robustness reproducibility and replicability are relatively high in comparison to previously published replications (e.g., economics laboratory experiments using new data Camerer et al. (2016)). We provide a more direct comparison to the literature in the next subsection by splitting our re-analyses by group, including re-analyses which incorporate new data. Nonetheless, we take as a positive sign for the re-analyzed literature that the re-analysis rate is as high as it is.

5.5 Robustness Reproducibility for Figures

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

5.6 Non Comparable Re-Analysis

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

alternative dataset.

The total unique articles that have been re-analyzed is 104, and while 82 articles have at least one non-comparable estimate, we find that only a small proportion (10 re-analyses) were not directly comparable for all reported re-analysis estimates.[53]

For not directly comparable re-analyses, we report the proportion that replicators indicated were of the same statistical significance as the original and same sign. For our four definitions of reproducibility and replication rates these are: When the original estimate is statistically significant at the 5% level, 85% of those we considered not directly comparable indicated their re-analysis was of the same significance (93% for the 10% level). When the original estimate was not statistically significant at the 5% level, 88% of those we considered not directly comparable indicated their re-analysis was of the same (non)significance (92% for the 10% level).[54]

5.7 Types of Re-analyses

Replicators are afforded freedom in their re-analyses. From what was ultimately submitted, we categorize each re-analysis estimate into one (or more) of seven types (we discuss that categorization in detail in Appendix A.5).

In this section, we investigate the differences in robustness reproducibility by *type* of re-analysis. We begin with statistical significance, where we split Figure 20 into its components in Appendix Figures 31-37 and offer an additional analysis in Appendix Figure 38 (which includes re-analyses that introduced new data, which is not quite as directly comparable as the remainder of those we discuss at length). We then continue onto relative effect sizes, where we split Figure 4 into its components in Appendix Figures 39-44 to illustrate relative effect sizes by type of re-analysis (but only when effect sizes are directly comparable - e.g., not when changing the dependent variable since that would make comparison of effect sizes dubious at best).

We find striking patterns for statistical significance. For context, while the average difference in p-values depicted in Appendix Figure 20 is 0.053, this average masks considerable heterogeneity apparent in the figure (for example, 22% of $p_{\text{rep}} - p_{\text{orig}}$ are greater than 0.10 which guarantees a loss of statistical significance regardless of original statistical significance level). In Appendix Figure 33 we present the type of re-analysis that has the most striking distribution of the p-value difference. The mean difference is 0.15, representing an average shift of 15 percentage points *less* statistically significant (towards one) following re-analysis. Unsurprisingly, this large shift is composed of shifts as large as 0.25, 0.5, and close to 1, representing a statistically insignificant re-analysis regardless of the level of significance of the original result. A total of 32% of re-analyses that change the dependent variable result in a shift greater than 0.10, enough to ensure loss of statistical significance

[53] A simple t-test of mean p_{rep} by whether the re-analysis was not comparable (or included elsewhere in the analysis) reveals an average difference of 0.045, where those excluded p_{rep} were *more* statistically significant (had lower values on average) than the ones included. This has the fortunate effect that the comparisons we have identified as not-incomparable will likely represent under-estimates of the reproducibility rates for estimates originally statistically significant.

[54] Following up on the previous section, we also have a subset of figures which cannot be reasonably compared. Of those which cannot be reasonably compared, we find a reproduction rate of about 79.2% (72 out of 91).

regardless of original statistical significance level. The remaining average increase in p-values range from 0.022 (changing estimation method) to 0.085 (changing sample).

We also find striking patterns for relative effect size. For context, the average relative effect size was approximately one (see Figure 4 for the test-level and Appendix Figure 30 for the paper level). There is significant heterogeneity in the relative effect size by type of reanalysis. The type of re-analysis with the lowest relative effect size is when the dependent variable is changed, with an average of only 29.8% (not depicted, as comparisons of effect sizes when the dependent variable changes are - at best - dubious). The type of re-analysis with the lowest relative effect size is when the main independent variable is changed, with an average of only 81.1% (though this too could be dubious as the reported coefficient's units may have changed). The type of re-analysis with the lowest relative effect size that we considered to be valid is when changing inference method (at 91% and depicted in Appendix Figure 43). In contrast, some types of re-analysis provided an average relative effect size that was *greater* than the originally published estimates (for example when changing the sample (136%) and changing the estimation method (124%)).

Table 5 provides robustness reproducibility and replicability rates by type of re-analysis for the four definitions of reproducible and replicability. We highlight here three key results. First, robustness reproducibility rates are almost always lower for originally statistically significant results when compared to their complement. Second, within a definition (for example in the first row) reproducibility rates vary widely from 45% to 87%. Third, robustness reproducibility rates are markedly lower when replicators change the (coding of the) dependent variable (45%) and the sample (64%). In contrast, replicability rate is the highest for re-analyses that introduced new data (87%).

These findings highlight the relative importance of different types of specification checks in confirming the robustness of originally published claims. Nonetheless, this by-type analysis suffers from numerous shortcomings, which we briefly highlight. First, the re-analyses are potentially categorized into types that are 'too broad.' Going forward, additional observations will allow for finer categorization and perhaps more nuanced discussion by type of re-analysis (for instance, differentiating between increasing or decreasing the sample or differentiating between changes in time or geographical units of analysis). Additional observations may even allow for productive discussion of reproducibility rates by research field and identification method. Second, replicators did not systematically implement these types of re-analyses (nor could they have been aware of these potential categorizations, since we conceived of them only after viewing their submissions), but rather had freedom to chose which (if any) to implement, and so selection along many (perhaps unobservable dimensions) is no doubt present. Third, many of the re-analyses are implemented simultaneously, making it hard to disentangle their relative importance.

In summary, we believe the patterns displayed here point to several optimistic results for the re-analyzed body of research. While remaining aware that replicators were free to choose which types of re-analysis to attempt, the most striking result of around one third of original p-values becoming non-significant also says that two-thirds remained statistically significant - a proportion higher than seen in many previous mass replication efforts. Turning away from statistical significance, we find even more optimistic results for relative effect size by type of re-analysis. Replicators, with some

exceptions, find effect sizes that are, on average, approximately the same (and often larger) than those originally published regardless of the type of re-analysis. Last, and in the terms of our four definitions of robustness reproducibility and replicability rates, many rates (with some exceptions) are in the mid-70's at their lowest, while others reach beyond a remarkable 90% reproducibility rate under re-analysis.

6 Discussion

We aim for high-quality replication reports and believe our process contributes positively to the scientific community for at least four reasons. First, original authors are allowed to respond and may point out flaws in the replicators' work. In practice, original authors and replicators do not disagree on the completeness of the replication package (e.g., whether raw data is provided) nor on the presence of major coding errors. Disagreements are almost always about the validity of robustness reproducibility and replicability. Second, A.B. or a co-director at I4R checks the tone of both the original authors' response and replicators' report.[55] Third, while replicators may make mistakes, so do reviewers and editors. Our replicators have the advantage of having access to the replication package. They may identify coding errors and uncover coding decisions which may not be discussed in the main body of the article.[56] For example, multiple studies in our sample do not mention the use of a weighting scheme for their main analysis. This coding decision is obvious to a replicator, but not to an editor or reviewer. Relatedly, our teams of replicators spent on average 13 active days working on their reproducibility and replicability. This may compare favorably to a typical referee report, which is not prepared with peers and may involve subjectivity about the contribution of the paper to the literature.[57] Fourth, replicators learn throughout the process and benefit from this experience. (See Section 7.) This, in itself, is a positive contribution.

6.1 Barriers to Reproducibility and Replicability

We ask the following question in the team survey: "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)" Figure 45 provides a summary of the responses for these four categories. Out of 110 teams, 64 did not respond to the question. This suggests that the majority of teams felt their replication packages contained enough to create a replication report for I4R. That said, the lack of raw data restricted most what replicators could do when analyzing a paper across all four categories. Raw data inhibited 19% of teams when trying to do robustness checks and 18% of teams wanting to recode key variables. 12% of teams also believed the lack of raw data inhibited their ability to perform a replication and

[55] A.B. and D.M. virtually meet with original authors and replicators upon request.

[56] Or buried in a footnote.

[57] As an example, the *Canadian Journal of Economics* writes in its instructions to reviewers that the "amount of time taken with a paper can vary enormously - anything from a couple of hours to a couple of days of full-time effort. A typical report should probably take 3 or 4 hours." See https://www.economics.ca/cpages/cje-referees. Obviously, the journals in our sample are higher ranked and we only focus on published manuscripts.

13% of teams believed it inhibited their ability to perform extensions.[58] The remaining reasons for potential hurdles replicators could have faced (like no intermediate data, no data dictionary, unclear documentation, and/or unclear replication package) only affected more than 5% of teams in one category. About 7% of teams felt the original paper was unclear to the point of not being able to perform robustness checks. We thus see a lack of raw data provided in a replication package as a significant barrier to reproducibility and replicability, even in our selected sample of journals which have data and code availability policies.

6.2 Communication with Original Authors

As mentioned, once a Replication Report is completed, it is shared with the original authors. But replicators may want to contact original authors for clarifications. We asked replicators whether their team or I4R contacted, or attempted to contact, the original authors for clarifications. About 40% responded "yes". About 10% reached out because the replication package was unclear, while 17% needed help to computationally reproduce the original authors' results. Another 17% were unable to access the original authors' data. Other reasons include verifying coding errors, clarifications about the design model parameters or other coding decisions.

Interestingly, about two-thirds of replicators mentioned that interacting with the original authors improved the quality of their report. Reasons provided include providing missing information on variables and procedure and providing data or instructions on how to obtain the data. Some teams also reported that original authors rightfully helped them adjust the tone of their report.[59] Another benefit of exchanging with the authors was to identify and confirm coding errors in the original study's or replication's codes.

7 On the Benefits for Replicators

A growing literature documents that reproductions and replications are not well-cited and that "negative" replications do not lead to a decrease in citations for original studies (e.g., Ankel-Peters et al. (2023b); Coupé and Reed (2022); Schafmeister (2021); Serra-Garcia and Gneezy (2021); von Hippel (2022)). We contribute to this literature by documenting other benefits of conducting reproductions and replications. We ask the following question in the individual survey: "Please indicate the degree to which your experience with I4R has contributed to your improvement in the following areas." We offer six choices: (i) Networking, (ii) coding skills, (iii) capacity to write a good replication package, (iv) learning difference between reproduction and replication, (v) further ability as a researcher and (vi) communicate issues with a paper to others. Appendix Table 15 provides a breakdown of the responses. We find that about 70% of replicators responded that their experience with I4R contributed either a lot or moderately to their: (1) capacity to write a good replication package

[58] Fišar et al. (2023) also provide evidence that non-reproducibility for the journal *Management Science* is due to non-availability/accessibility of data.

[59] One set of original authors also performed at the request of the replicators additional robustness checks in their anonymous (non-public) data files.

and (2) learning the difference between reproduction and replication. Replicators further said their experience with I4R contributed at least moderately to furthering their ability as a researcher (about 53%) and their ability to communicate issues with a paper to others (about 60%).[60]

While the (perceived) benefits of producing a reproduction or replication might be limited, we nonetheless document other benefits which have not been documented. We hope these findings receive as much (or more) attention as (than) the lack of citations for replications going forward.

8 Many-Analysts Approach: Authors' Experience and Prestige, and Data and Code Availability

In this section, we tackle additional research questions using a "many-analysts" approach where six research teams use our Meta Database to answer the same research questions. A many-analysts approach may be less vulnerable to specification searching and may mitigate the influence of individual-researcher biases, such as confirmation bias by the proponent of a theory (Hoogeveen et al. (2023)).

Our approach and research questions, which we detail below, were pre-registered.[61] See Section A.6 for more information on the methodology and illustrative examples on how a few teams coded their analysis.

8.1 Research Questions

The six meta-analyst teams tackled the following eight questions:

1. "Does reproducibility/replicability rate depend on replicators' experience coding?"
2. "Does reproducibility/replicability rate depend on replicators' academic experience?"
3. "Does reproducibility/replicability rate depend on the authors' experience?"
4. "Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience?" In particular:
   (a) Are reproducibility/replicability rate higher when authors' experience is high, and replicators' experience is low (in comparison to similar levels)?
   (b) Are reproducibility/replicability rate higher when authors' experience and replicators' experience is similar (in comparison to dissimilar levels)?
   (c) Are reproducibility/replicability rate higher when authors' experience is low, and replicators' experience is high (in comparison to similar levels)?

[60] Networking is especially important for those attending RG in person.
[61] Our pre-analysis plan was pre-registered here: https://osf.io/8wsqx/. The pre-analysis plan was pre-registered prior to sharing the Meta Database with analysts.

5. "Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige?" In particular:
   (a) Are reproducibility/replicability rate higher when authors' have high prestige, and replicators' experience have low prestige (in comparison to similar levels)?
   (b) Are reproducibility/replicability rate higher when authors' and replicators' prestige is similar (in comparison to dissimilar levels)?
   (c) Are reproducibility/replicability rate higher when authors' have low prestige, and replicators' experience have high prestige (in comparison to similar levels)?
6. "Does reproducibility/replicability rate depend on the original authors providing raw data?"'
7. "Does reproducibility/replicability rate depend on the original authors providing raw or intermediate data?"
8. "Does reproducibility/replicability rate depend on the original authors providing cleaning code?"

8.2 Data for Meta-Analysts

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

The backbone of the data provided to meta analysts was the Meta Database, of which questions from the team leader surveys and individual surveys were added. Much of the information from the individual surveys were aggregated to the report level.[62]

8.3 Method

As in Botvinik-Nezer et al. (2020), Breznau et al. (2022), Huntington-Klein et al. (2021), Menkveld et al. (Forthcoming) and Silberzahn et al. (2018), the goal is to have each team answer each research question independently. Each team received the same instructions and data. We allow full flexibility to all teams. Teams are allowed to use any statistics package, statistical model, inference, weighting scheme, *etc*. Teams were free to choose the independent variables and how to code them. Teams were also free to construct their own derived variables from the dataset given to them.

[62]The data given to the meta analysts changed as replication reports, team leader and individual surveys were completed. In total, we provided 13 updated Meta Databases for meta analysts between November 6th, 2023 and February 12th, 2024. We did this to give meta analysts time to create scripts which would work with partial datasets as we worked to gather reports and surveys. This allowed analysts to expedite their analysis once the full dataset was constructed.

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

8.4 Results

Each row in Table 6 represents one of the eight research questions. The four columns represent four broad categories regarding research teams' coefficient estimate(s) to the research question: (1) negative and statistically significant, (2) negative and not-statistically significant, (3) positive and not statistically significant and (4) positive and statistically significant. The left-to-right order of the column categories corresponds to where the associated analyst t-statistic would fall on the real number line. While the dependent variable (which does not change in this table) is the same for each team, each team chooses their own primary independent variable. Each cell represents the proportion of analyst-estimated relationships by category. The cells are team-weighted so that if a many-analyst team presents three estimates and another team presents a single estimate, the first team's estimates enter the proportion as 1/3 each.

The cell in the first row and first column tells us that 42.8% of results from the many-analysts find a negative and statistically significant relationship between the coding experience of a replicator and the reproducibility rate for estimates that were originally statistically significant at the 5% level (i.e., lower reproducibility rate for more experienced replicators).[63] From the second column, it becomes clear that, if there is a relationship between replicators experience coding and the reproducibility rate, it seems to be almost definitively negative with a combined proportion of 86% of results returned as negative and statistically significant or negative and not statistically significant at the 5% level. Only 14% of estimates find a positive relationship between the replicators experience coding and the reproducibility rate - of which none of the estimated positive relationships estimated were statistically significant. (The associated row in Appendix Table 16, which looks at the replication for the 10% threshold finds the same pattern.) This result potentially suggests that replicators with more experience coding are better suited to detecting and correcting less-than-robust estimations - possibly because of having greater expertise with the methods used.

For the second research question, a somewhat similar albeit less starkly negative result is found with some proportion moving into the positive and statistically significant category. That said, the ratio of negative-and-significant results to positive-and-significant results remains above 4 to 1. The associated row in Appendix Table 16, which looks at the replication for the 10% threshold finds the same pattern, although with 75% of many-analysts results being negatively signed.

For the third research question - whether the replication rate depends on the author's experience seems to be centered on the null. Combined, the negative and not statistically significant and the positive and not statistically significant cells contain 97.2% of results. The null hypothesis dominates

[63] Table 6 presents results where the dependent variable takes a value of one if an originally 5% statistically significant result was reproduced by a replicator also at the 5% level. Appendix Table 16 has the same structure, but uses the 10% threshold. Appendix Table 17 then examines whether an originally *not* 5% statistically significant result was reproduced, while Appendix Table 18 continues this with the 10% threshold.

in Appendix Tables 16, 17, and 18 (which examine reproducibility rates for originally statistically significant at the 10% level, not statistically significant at the 5% level, and not statistically significant at the 10% level, respectively) as well.

For the fourth research question, (which has three sub-questions depending on the relative hierarchy of replicator and original author experience) there seems to be a positive relationship when authors have more or the same level of experience as the replicator (research question 4a and 4b). This relationship, however, weakens to a likely null when authors have comparatively less experience than their replicators. Appendix Tables 16, 17, and 18 find similar patterns.

For the fifth research question, which has the same comparative structure as the fourth while focusing now on the relative prestige of the authors and replicators, the same (albeit weaker) pattern is found. When authors have more prestige than their replicator, there is a very positive relationship with replication rate. When original authors and replicators have similar prestige levels, this relationship becomes much more likely to be a null (since the middle two columns so outsize the outer two columns). When the authors have less prestige than the replicators, then the relationship seems to be negative: 22% finding a negative and statistically significant relationship. In Appendix Table 16, we see the same pattern. When examining replication rate of originally statistically insignificant results, the null hypothesis dominates.

The null hypothesis seems to dominate for the final three research questions, with statistical significance not being achieved in either direction for more than one-sixth of the teams' analyses. This means that replication rate does not seem to have a relationship for whether the authors provided raw data (research question 6), both raw and intermediate data (research question 7) or cleaning codes (research question 8).[64,65] This result may reflect that our focus is on journals with a data and code availability policy. The provision (or not) of raw data, intermediate data, or cleaning codes, may thus be due to data type rather than selective data/code provision by original authors.[66]

To sum up, we provide evidence suggesting a negative relationship between replicators' experience and robustness reproducibility, while provision of raw or intermediate data, or the necessary cleaning codes, does not seem to affect the reproducibility of research.

9 Conclusion

> *False facts are highly injurious to the progress of science, for they often long endure*
> The Descent of Man (1871), Vol. 2, 385. by Charles Darwin

Substantial information asymmetry exists between the authors of an article and the rest of the

[64]The null hypothesis clearly dominates for these final research questions in Appendix Tables 16, 17, and 18 as well.

[65]In Table 19, we reproduce the analyses in Table 6 and Appendix Tables 16, 17, and 18 while only including estimates if the analyst team indicated that, in their opinion, the estimated effect size was economically meaningful. Results are broadly consistent as those described above without the restriction.

[66]Our results are consistent with Brodeur et al. (Forthcoming) who document no relationship between the presence of a data and code availability policy and the incidence of p-hacking, including for research leveraging harder-to-access (e.g., administrative) data. They also document a statistically insignificant relationship between voluntary provision of data by authors on their homepages and selective reporting.

academic community (Brodeur et al. (2016)). This leads reviewers and editors to require several robustness checks prior to acceptance. Unfortunately, reviewers may not be aware of important coding decisions and do not have access to the codes and data for their review. A related concern is that some manuscripts' programs contain major coding errors or discrepancies between the codes and the articles.

We see mass reproducibility and replicability as a new hope for the social sciences, partly dealing with the concerns highlighted above. Our paper describes a new initiative and methods to reaching the goal of mass reproducibility and replicability. While our initiative is just starting, we document several important patterns using a sample of 110 replication reports.

In terms of impact, the scale of this ongoing project has the potential to change research norms and researchers' behavior through the adoption of more rigorous methodologies and deterring questionable research practices.

While our sample of journals is selective, our results are optimistic. They suggest a high level of reproducibility and a low prevalence of major coding errors. We argue that these results and this project may have a positive effect on trust in scientific results. We ask all replicators in the individual survey about the quality of the replication package they reproduced and their views of the discipline. We find that just over 40% report that the quality of the replication package gave them a more optimistic view of the discipline (Appendix Figure 46). About 45% report that the quality of the replication package did not affect their views of the discipline. These results suggest that mass reproduction may significantly increase trust in scientific results among scientists.

Equally important, our project has the potential to advance science and improve equity issues. The posting of data and code and its re-analysis are likely to advance science not only through course correction but also through learning and understanding new approaches more quickly. Reproducing the original authors' work in another (open source) software also has the potential to level the playing field by allowing researchers from lower-level universities, those in developing nations, and others who cannot afford expensive licenses to learn from elite scholars.

Our results suffer from several limitations. To this date and despite some recent progress on the matter, only a small number of economics and political science journals request data and codes (Askarov et al. (2023); Brodeur et al. (Forthcoming)), and a very small fraction check whether the results are reproducible (Vilhuber et al. (2020)). This is even though this has been a long-standing issue; in fact, Ragnar Frisch wrote as early as 1933 that "In statistical and other numerical work presented in Econometrica the original raw data will, as a rule, be published, unless their volume is excessive. This is important to stimulate criticism, control and further studies." (Introductory editorial to Econometrica). Our results should thus be seen as describing patterns for leading journals in the field of open science and data sharing. Future research should aim to draw conclusions about reproducibility and replicability more broadly by reproducing and replicating a random sample of papers from journals that do and do not have a data availability policy.

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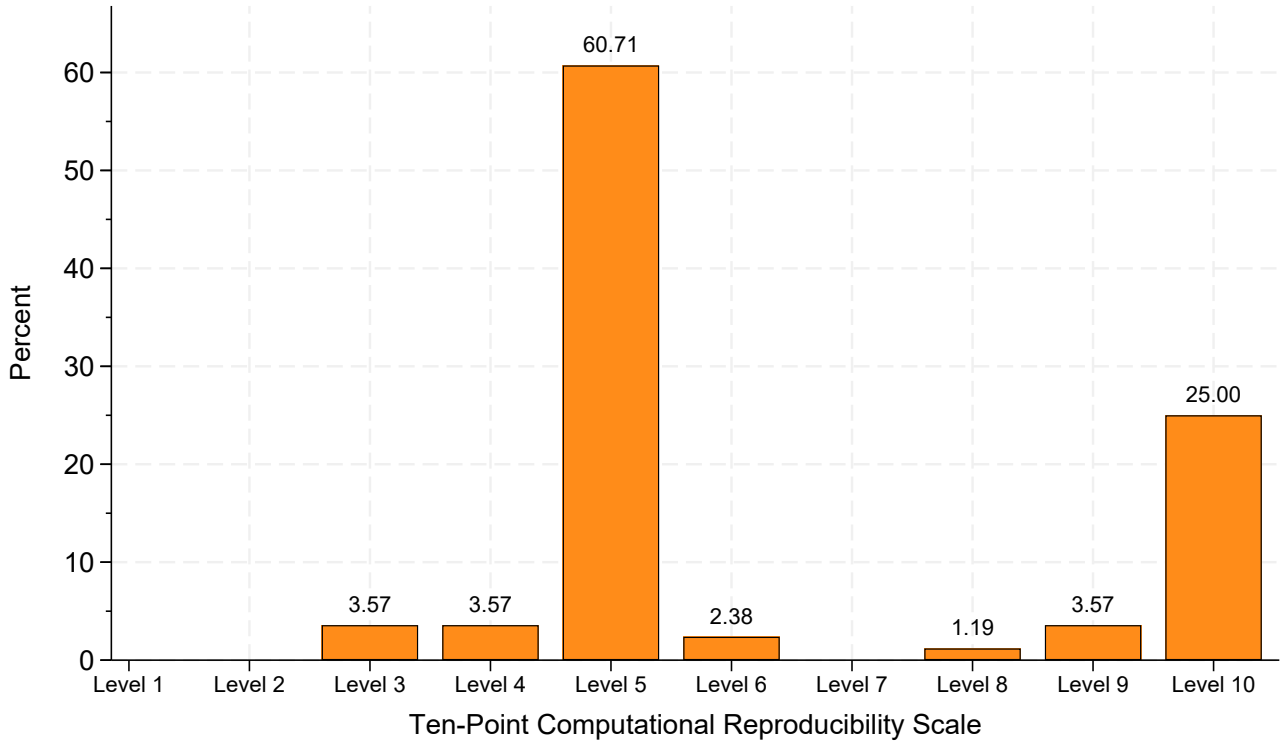
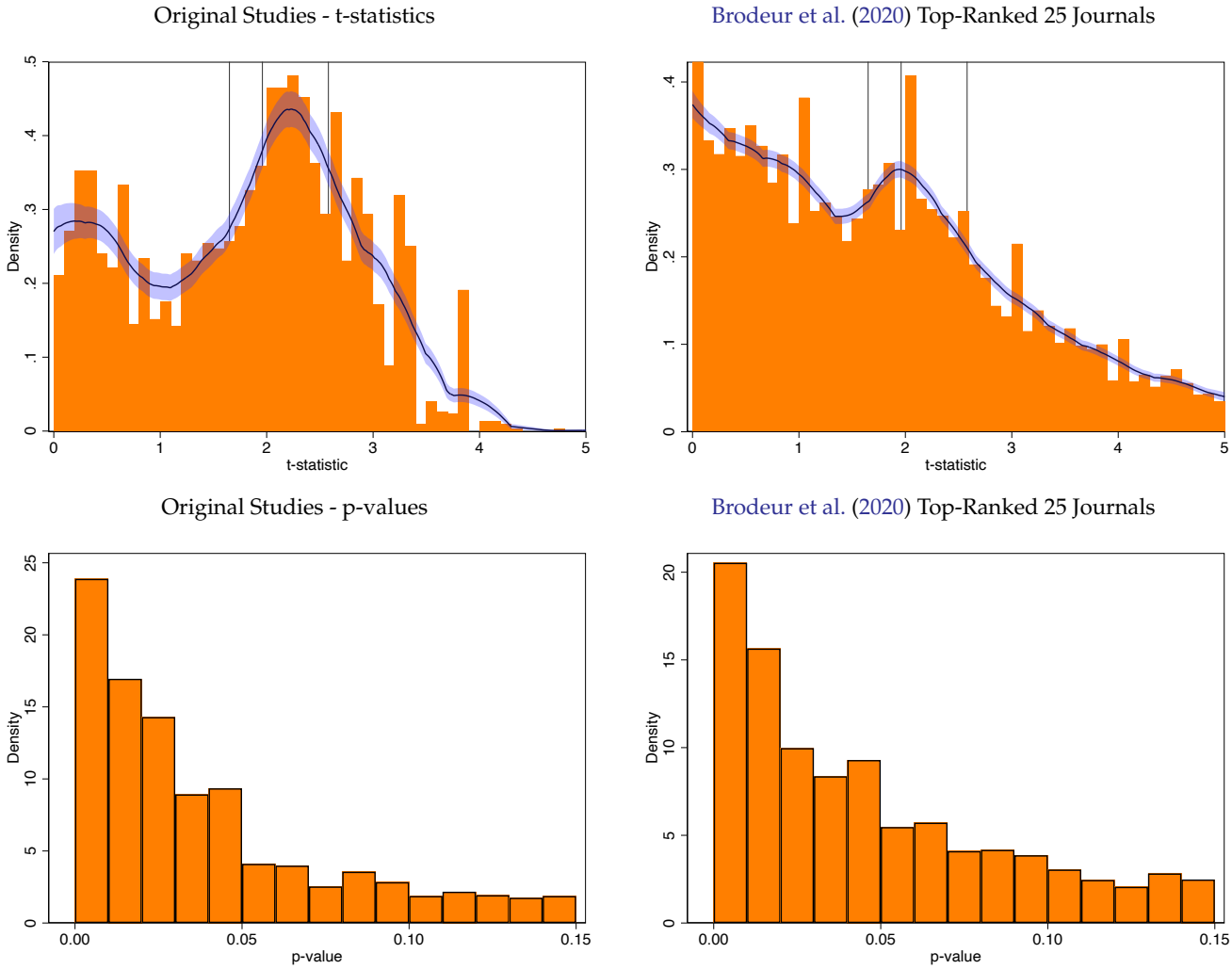
Figures

Figure 1: 10-Point Computationally Reproducibility Score



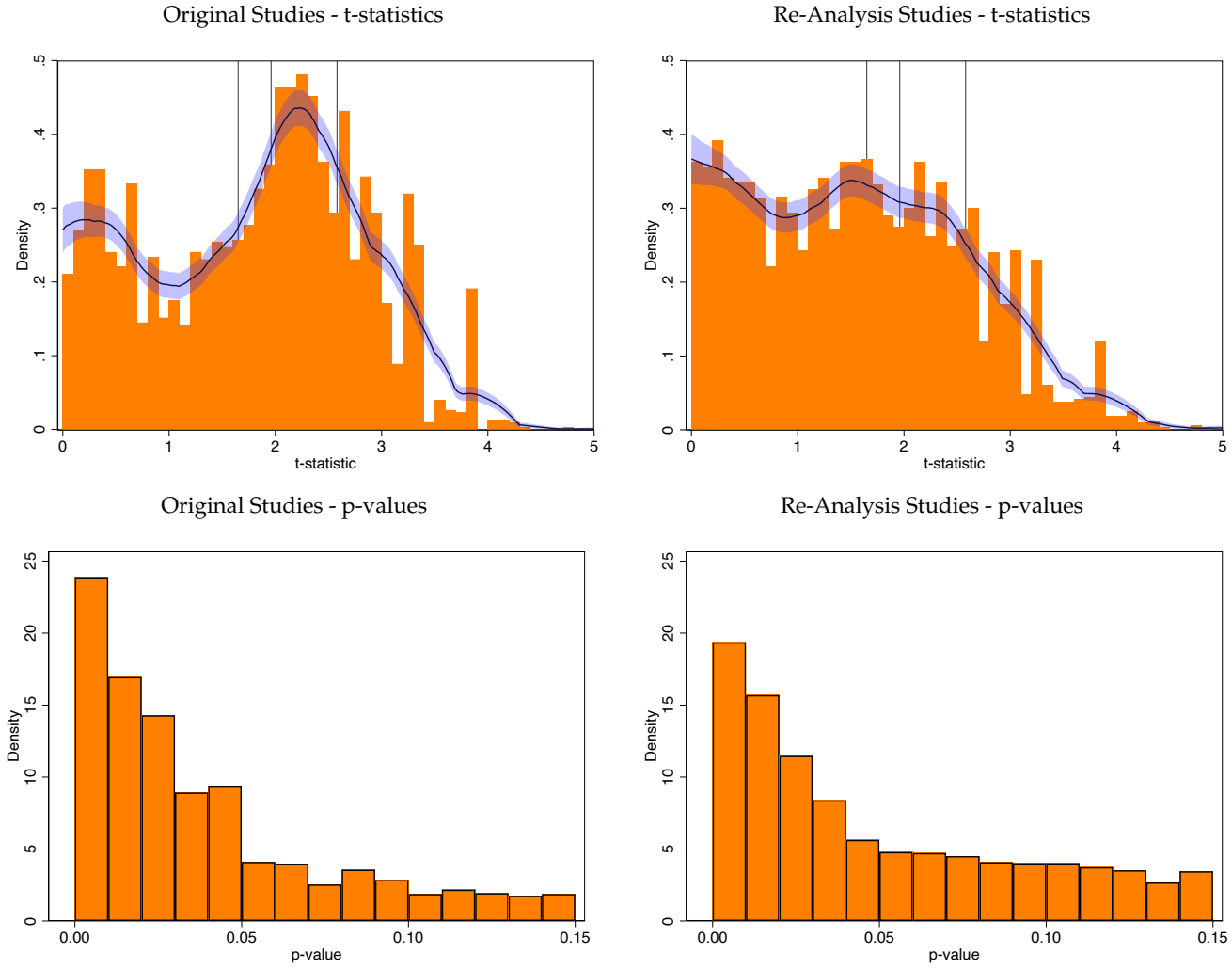
Notes: Each team assigned a reproducibility score on a scale of one to ten to the paper reproduced. See Online Appendix A.3 and Online Appendix Table 9 for a description of each score. Level 10 (L10) means that all necessary materials are available and produce consistent results with those presented 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.

Figure 2: Distributions of t-Statistics and p-Values for Original Studies and Brodeur et al. (2020)



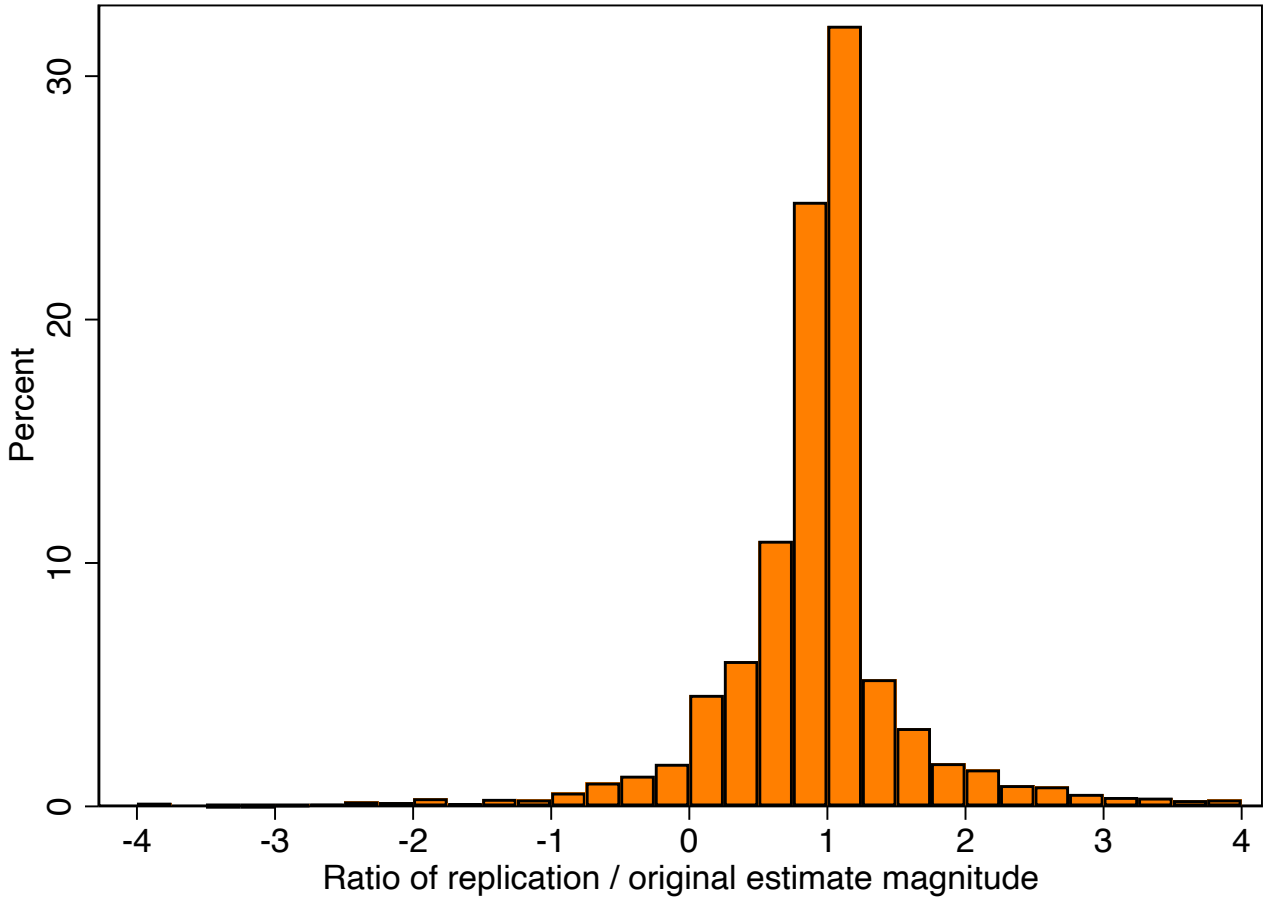
Notes: The top figures display a histogram of test statistics for $t \in [0, 5]$, with bins of width 0.1. The top left figure includes all original studies in our data set. As a comparison, the top right figure plots the corresponding histogram of z-statistics from the top-ranked 25 economics journals published in 2015 and 2018 (from Brodeur et al. (2020)). Vertical reference lines are displayed at conventional two-tailed significance levels. We superimpose an Epanechnikov kernel density curve. 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 Brodeur et al. (2020), respectively.

Figure 3: 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. 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.

Figure 4: Relative Effect Size



Notes: 48% of relative effect sizes are exactly equal to or greater than 1. This figure illustrates the ratio of robustness reproduction/replication 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.

Tables

Table 1: Summary Statistics by Journal

Discipline and Journal	# Articles Total (1)	# Articles RGs (2)	# Articles Editor (3)	# Tests (4)	Data Editor (5)
Economics	79	67	12	5,494	
American Economic Review	17	12	5	1,392	Yes
American Economic Review: Insights	2	0	2	149	Yes
American Economic J.: Applied Economics	9	6	3	260	Yes
American Economic J.: Economic Policy	11	11	0	811	Yes
American Economic J.: Macroeconomics	3	3	0	25	Yes
Economic Journal	20	18	2	1,262	Yes
Journal of Political Economy	8	8	0	1,283	No
Quarterly Journal of Economics	4	4	0	101	No
Review of Economic Studies	5	5	0	211	Yes
Political Science	31	16	15	1,089	
American Journal of Political Science	13	6	7	539	External
American Political Science Review	6	3	3	214	No
Journal of Politics	12	7	5	336	Yes
Total	110	83	27	6,583	

Notes: This table provides an overview of test statistics and articles reproduced and/or replicated by journal. Columns 1 and 4 indicate the number of article and test statistics per journal, respectively. Columns 3 and 4 report the number of articles per stream, where RGs is an acronym for Replication Games. Column 5 indicates if the journal has a data editor.

Table 2: Summary Statistics: Original Authors and Replicators

	Mean (1)	Standard Deviation (2)	Minimum (3)	Maximum (4)
Test Statistics per Report	59.84	72.67	0	421
Year	2022.13	0.33	2022	2023
Economic Articles	0.72	0.45	0	1
Proportion of Economics Papers in Top 5	0.43	0.50	0	1
GS Citations (As of Report Completed)	43.98	71.39	0	573
Original Authors				
Number Original Authors	2.63	1.23	1	6
Share Graduate Student	0.06	0.18	0	1
Avg. Experience (Years since PhD)	11.21	6.34	0	31.50
Avg. GS Citations	4269.05	8882.00	31	55633.5
Replicators				
Number Replicators	3.25	1.22	1	7
Share Published Top 5 Econ/Targeted Poli Sci	0.15	0.36	0	1
Share Pub. Targeted Journals	0.30	0.46	0	1
Share Pub. Top 5/Targeted Poli Sci (Past 5 Years)	0.14	0.34	0	1
Share Pub. Targeted Journals (Past 5 Years)	0.26	0.44	0	1
Share Team Graduate Student	0.49	0.34	0	1
Avg. Experience (Years since PhD)	3.12	3.10	0	13.50
Avg. GS Citations	478.49	1016.67	0	6095.33
Comfortable programming in Stata	0.74	0.44	0	1
Comfortable programming in R	0.64	0.48	0	1
Comfortable programming in MATLAB	0.14	0.34	0	1

Notes: Each observation is an article. We do not weight test statistics. The Top 5 journals in economics are the American Economic Review, Econometrica, Journal of Political Economy, Quarterly Journal of Economics, and Review of Economic Studies. The 3 leading political science journals in our sample are the American Journal of Political Science, American Political Science Review and Journal of Politics. Panels two and three focus on the original authors and replicators, respectively. Average experience is the mean of years since PhD. GS citations in the top panel refers to the number of Google Scholar citations for the original article as of the completion of the replication report. Average GS citations in the bottom panels refers to the number of Google Scholar citations at the time the report is completed.

Table 3: Summary Statistics by Types of Re-Analyses

	# Articles (1)	# Articles RGs (2)	# Articles Editor (3)	# Tests (4)
All Re-Analyses	103	81	22	6583
All Simultaneous Robustness Checks	51	41	10	809
Full Sample				
By Re-Analyses: Change in				
Control variables	58	45	13	1939
Sample	75	57	18	1774
Dependent Variable	23	18	5	285
Main Independent Variable	20	19	1	264
Estimation Method	33	28	5	605
Inference Method	23	19	4	542
Weighting Scheme	14	10	4	126
Use New Data	15	13	2	469
Economics				
By Re-Analyses: Change in				
Control variables	45	36	9	1612
Sample	55	47	8	1647
Dependent Variable	19	17	2	279
Main Independent Variable	15	15	0	195
Estimation Method	22	21	1	433
Inference Method	19	15	4	507
Weighting Scheme	9	8	1	80
Use New Data	13	11	2	461
Political Science				
By Re-Analyses: Change in				
Control variables	13	9	4	327
Sample	20	10	10	127
Dependent Variable	4	1	3	6
Main Independent Variable	5	4	1	69
Estimation Method	11	7	4	172
Inference Method	4	4	0	35
Weighting Scheme	5	2	3	46
Use New Data	2	2	0	8

Notes: This table shows the number of articles and test statistics for all re-analyses (top panel), by types of re-analyses (2nd panel), by types of re-analyses for economic articles (3rd panel) and by types of re-analyses for political science articles (bottom panel), respectively. The second and third columns show the number of reports created *via* replication games and editor stream, respectively.

Table 4: Shifts in Statistical Significance Regions

	Re-Analysis Significance Level					
Original Significance Level	Sign Change	Not Sig.	Sig. at 10%	Sig. at 5%	Sig. at 1%	Total
Not Significant	13.61	75.00	4.59	3.91	2.89	100.00
Significant at 10%	6.91	45.45	28.00	12.73	6.91	100.00
Significant at 5%	2.76	27.89	12.06	41.08	16.21	100.00
Significant at 1%	4.95	12.89	4.43	8.07	69.66	100.00
Total	7.32	37.72	7.80	14.06	33.10	100.00

Notes: This table illustrates shifts across significance and insignificance regions. Each row focuses on an initial level of statistical significance. Each column reports the share of re-analyses that ended up in each statistical significance region.

Table 5: Robustness Reproducibility and Replicability Rates

	(1) Full Sample	(2) Change Control	(3) Dep. Var.	(4) Change Estim.	(5) Infer. Method	(6) Ind. Var.	(7) Change Sample	(8) Change Weights	(9) New Data
Rep. if Orig. Sig. 5%									
Estimate	0.71	0.76	0.45	0.76	0.74	0.78	0.64	0.74	0.87
Confidence Interval	[0.70,0.73]	[0.73,0.79]	[0.35,0.55]	[0.72,0.81]	[0.67,0.82]	[0.72,0.85]	[0.61,0.67]	[0.64,0.85]	[0.84,0.91]
Rep. if Orig. Not Sig. 5%									
Estimate	0.88	0.92	0.80	0.85	0.88	0.77	0.86	0.97	0.83
Confidence Interval	[0.87,0.90]	[0.89,0.94]	[0.64,0.96]	[0.80,0.90]	[0.83,0.94]	[0.64,0.89]	[0.83,0.89]	[0.91,1.03]	[0.75,0.91]
Rep. if Orig. Sig. 10%									
Estimate	0.75	0.78	0.45	0.83	0.74	0.80	0.70	0.73	0.89
Confidence Interval	[0.74,0.77]	[0.75,0.81]	[0.36,0.55]	[0.79,0.86]	[0.67,0.82]	[0.74,0.86]	[0.67,0.73]	[0.63,0.83]	[0.86,0.92]
Rep. if Orig. Not Sig. 10%									
Estimate	0.85	0.88	0.93	0.82	0.84	0.54	0.82	0.92	0.75
Confidence Interval	[0.83,0.87]	[0.85,0.91]	[0.80,1.06]	[0.76,0.88]	[0.77,0.91]	[0.38,0.70]	[0.78,0.86]	[0.81,1.03]	[0.64,0.85]

Notes: Robustness reproducibility and replicability rates for four definitions by type of re-analyses. Columns present robustness reproducibility rates by type of re-analyses, which are not mutually exclusive. Columns 1-8 do not include re-analysis that use new data, while column 9 does. In (2), the re-analysis changed the control variables. In (3), the re-analysis changed the dependent variable. In (4), the re-analysis changed the estimation method. In (5), the re-analysis changed the inference method. In (6), the re-analysis changed the main independent variable. In (7), the re-analysis changed the sample. In (8), the re-analysis changed the weights applied, or applied weights for the first time. In (9), we present robustness replicability rates for re-analyses that introduced new data. 95% confidence intervals presented in square brackets.

Table 6: Many-Analysts' Replication Rate And Replicator Characteristics For Published Results Originally Statistically Significant at the 5% Level

	Category				
RQ	Neg. & Sig.	Neg. & Not Sig.	Pos. & Not Sig.	Pos. & Sig.	Total
1	42.78	43.33	13.89	0.00	100.00
2	36.75	24.79	30.13	8.33	100.00
3	0.00	33.33	63.89	2.78	100.00
4a	0.00	16.67	50.00	33.33	100.00
4b	16.67	0.00	50.00	33.33	100.00
4c	16.67	50.00	16.67	16.67	100.00
5a	0.00	16.67	52.78	30.56	100.00
5b	8.33	40.28	34.72	16.67	100.00
5c	22.22	52.78	8.33	16.67	100.00
6	0.00	30.56	52.78	16.67	100.00
7	8.33	13.89	61.11	16.67	100.00
8	0.00	23.61	76.39	0.00	100.00

Notes: Six many-analyst teams independently answered eight pre-registered research questions concerning the possible relationship between replication rate and selected author/replicator characteristics. This table restricts the analysis to originally published estimates that were statistically significant at the 5% level. The **columns** represent one of four categories that a many-analyst could classify their analysis; if a many-analyst found a relationship that was statistically significant (at the 5% level) and positive, it was included in the column 'Pos. & Sig.' The **rows** represent eight pre-registered research questions (two have three sub-questions). They are: 1- Does reproducibility/replicability rate depend on replicators' experience coding? 2- Does reproducibility/replicability rate depend on replicators' academic experience? 3- Does reproducibility/replicability rate depend on the authors' experience? 4a- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (i) Are reproducibility/replicability rate higher when authors' experience is high, and replicators' experience is low (in comparison to similar levels)? 4b- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (ii) Are reproducibility/replicability rate higher when authors' experience and replicators' experience is similar (in comparison to dissimilar levels)? 4c- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (iii) Are reproducibility/replicability rate higher when authors' experience is low, and replicators' experience is high (in comparison to similar levels)? 5a- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (i) Are reproducibility/replicability rate higher when authors' have high prestige, and replicators' experience have low prestige (in comparison to similar levels)? 5b- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (ii) Are reproducibility/replicability rate higher when authors' and replicators' prestige is similar (in comparison to dissimilar levels)? 5c- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (iii) Are reproducibility/replicability rate higher when authors' have low prestige, and replicators' experience have high prestige (in comparison to similar levels)? 6- Does reproducibility/replicability rate depend on the original authors providing raw data? 7- Does reproducibility/replicability rate depend on the original authors providing raw or intermediate data? 8- Does reproducibility/replicability rate depend on the original authors providing cleaning code? For example, the **top row can be interpreted** as no many-analysts find a positive and statistically significant relationship between replicators' experience coding and replication rate. 13.89% of many-analyst results find a positive but not statistically significant relationship. 42.78% find a negative and statistically significant relationship, and 43.33% of many-analyst results find a negative and not statistically significant relationship. Most many-analysts provide more than one estimate per research question, this table weights many-analysts equally.

A ONLINE APPENDIX A

A.1 Authors' Contribution

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Master University), Geir Godager (University of Oslo), Leonard Goff (University of Calgary), Da Gong (University of California, Riverside), Javier González (Department of Economics, Southern Methodist University), Jeremy Gretton (Public Health Agency of Canada), Cristina Griffa (University of Nottingham), Idaliya Grigoryeva (UC San Diego), Maja Grøtting (The Norwegian Institute of Public Health), Eric Guntermann (UC Berkeley), Jiaqi Guo (University of Birmingham), Alexi Gugushvili (University of Oslo), Hooman Habibnia (WU Vienna University of Economics and Business), Sonja Häffner (University of the Bundeswehr Munich), Jonathan D. Hall (University of Alabama), Olle Hammar (Linnaeus University and Institute for Futures Studies), Amund Hanson Kordt (University of Oslo), Barry Hashimoto (Independent), Jonathan S. Hartley (Stanford University), Carina I. Hausladen (ETH Zurich, work conducted while at California Institute of Technology), Tomáš Havránek (Institute of Economic Studies, Faculty of Social Sciences, Charles University), Harry He (University of California, San Diego), Matthew Hepplewhite (University of Oxford), Mario Herrera-Rodriguez (CREST-Ecole polytechnique, IP Paris), Felix Heuer (RWI – Leibniz Institute for Economic Research), Anthony Heyes (University of Birmingham), Anson T. Y. Ho (Toronto Metropolitan University), Jonathan Holmes (University of Ottawa), Armando Holzknecht (University of Innsbruck), Yu-Hsiang Dexter Hsu (National Taiwan University), Shiang-Hung Hu (California Institute of Technology), Yu-Shiuan Huang (UC Davis), Mathias Huebener (Federal Institute for Population Research (BiB) & IZA Bonn), Christoph Huber (WU Vienna University of Economics and Business), Kim P. Huynh (Bank of Canada), Zuzana Irsova (Institute of Economic Studies, Faculty of Social Sciences, Charles University, and Anglo-American University, Prague), Ozan Isler (The University of Queensland), Niklas Jakobsson (Karlstad University), Michael James Frith (University of Oslo), Raphaël Jananji (Université de Montréal), Tharaka A. Jayalath (University of Saskatchewan), Michael Jetter (University of Western Australia), Jenny John (University of Ottawa), Rachel Joy Forshaw (Heriot-Watt University), Felipe Juan (Howard University), Valon Kadriu (University of Kassel and INCHER), Sunny Karim (Carleton University), Edmund Kelly (University of Oxford), Duy Khanh Hoang Dang (King's College London), Tazia Khushboo (University of Calgary), Jin Kim (Northeastern Univeristy), Gustav Kjellsson (University of Gothenburg), Anders Kjelsrud (Oslo Metropolitan University), Jori Korpershoek (Erasmus University Rotterdam), Andreas Kotsadam (Ragnar Frisch Centre for Economic Research), Lewis Krashinsky (Princeton University), Suranjana Kundu (Indian Institute of Technology Delhi), Alexander Kustov (University of North Carolina at Charlotte), Nurlan Lalayev (Monash University), Audrée Langlois (Université Laval), Jill Laufer (UC Davis), Blake Lee-Whiting (University of Toronto), Andreas Leibing (DIW Berlin and Freie Universität Berlin), Gabriel Lenz (UC Berkeley), Joel Levin (UC San Diego), Peng Li (University of Bath), Tongzhe Li (University of Guelph), Yuchen Lin (University of Warwick), Goncalo Lima (European University Institute), Ariel Listo (University of Maryland), Dan Liu (Australian National University), Xuewen Lu (University of Calgary), Elvina Lukmanova (New Economic School), Alex Luscombe (University of Toronto), Lester R. Lusher (University of Pittsburgh), Ke Lyu (University of Nevada, Reno), Hai Ma (McGill University), Nicolas Mäder (Knauss School of Business, University of San Diego), Clifton Makate (Norwegian University of Life Sciences and Norwegian Geotechnical Institute), Alice Malmberg (UC Davis), Adit Maitra (The University of Melbourne), Marco Mandas (University of Cagliari), Jan Marcus (Freie Universität Berlin), Shushanik Margaryan (University of Potsdam), Lili Márk (Central European University), Diego Marino Fages (Durham University), Andres Martignano (University of Nottingham), Abi-

gail Marsh (Univerisy of Ottawa), Isabella Masetto (London School of Economics and Political Science), Anthony McCanny (University of Toronto), Emma McManus (Health Organisation, Policy and Economics, The University of Manchester), Ryan McWay (University of Minnesota), Lennard Metson (University of Oxford), Fabian Mierisch (Catholic University Eichstaett-Ingolstadt), Jonas Minet Kinge (University of Oslo), Sumit Mishra (Krea University), Myra Mohnen (University of Ottawa), Jakob Möller (WU Vienna University of Economics and Business), Rosalie Montambeault (Université Laval), Sébastien Montpetit (Toulouse School of Economics), Louis-Philippe Morin (University of Ottawa), Todd Morris (University of Queensland), Scott Moser (University of Nottingham, School of Politics and International Relations), Fabio Motoki (Norwich Business School at the University of East Anglia), Lucija Muehlenbachs (University of Calgary and Resources for the Future), Andreea Musulan (University of Toronto), Marco Musumeci (Erasmus University Rotterdam), Munirul Nabin (Deakin University), Karim Nchare (Vanderbilt University), Florian Neubauer (RWI - Leibniz Institute for Economic Research), Quan M. P. Nguyen (University of Sussex), Tuan Nguyen (Hasselt University), Viet Nguyen-Tien (London School of Economics), Ali Niazi (University of Calgary), Giorgi Nikolaishvili (University of Oregon), Ardyn Nordstrom (Carleton University), Patrick Nüß (Kiel University), Angela Odermatt (University of Oxford), Matt Olson (University of Pennsylvania Wharton), Henning Øien (Department of Health Management and Health Economics, University of Oslo), Tim Ölkers (University of Göttingen), Miquel Oliver i Vert (University of Nottingham), Emre Oral (University of Mannheim), Christian Oswald (University of the Bundeswehr Munich), Ali Ousman (McGill University), Ömer Özak (Department of Economics, Southern Methodist University, IZA and GLO), Shubham Pandey (Indian Institute of Technology Bombay), Alexandre Pavlov (Université de Montréal), Martino Pelli (Asian Development Bank, Université de Sherbrooke), Romeo Penheiro (University of Houston), RyuGyung Park (UC Davis), Eva Pérez Martel (Universitat Autonoma de Barcelona), Jörg Ankel-Peters (RWI - Leibniz Institute for Economic Research), Tereza Petrovičová (UCSD), Linh Phan (UC Davis), Alexa Prettyman (Towson University), Jakub Procházka (Masaryk University), Aqila Putri (University of Maryland), Julian Quandt (WU Vienna University of Economics and Business), Kangyu Qiu (University of Calgary), Loan Quynh Thi Nguyen (Queen Mary University of London), Andaleeb Rahman (Cornell University), Carson H. Rea (Emory University), Adam Reiremo (University of Oslo), Laëtitia Renée (Université de Montréal), Joseph Richardson (Lancaster University), Nicholas Rivers (University of Ottawa), Bruno Rodrigues (Ministry of Research and Higher Education, Luxembourg), William Roelofs (University of Toronto), Tobias Roemer (University of Oxford), Ole Rogeberg (Ragnar Frisch Centre for Economic Research), Julian Rose (RWI - Leibniz Institute for Economic Research), Andrew Roskos-Ewoldsen (UC Davis), Paul Rosmer (Ludwig Maximilian University of Munich), Barbara Sabada (Bank of Canada), Soodeh Saberian (University of Manitoba), Nicolas Salamanca (The University of Melbourne), Georg Sator (University of Nottingham), Daniel Scates (UC Davis), Elmar Schlüter (Justus Liebig University, Giessen), Cameron Sells (Indepenent Researcher), Sharmi Sen (Monash University), Ritika Sethi (Rice University), Anna Shcherbiak (WU Vienna University of Economics and Business), Moyosore Sogaolu (McMaster University), Matt Soosalu (Carleton University), Erik Ø. Sørensen (NHH Norwegian School of Economics), Manali Sovani (Tufts University), Noah Spencer (University of Toronto), Stefan Staubli (University of Calgary), Renske Stans (Erasmus University Rotterdam), Anya Stewart (UC Davis), Felix Stips (Luxembourg Institute of Socio-Economic Research), Kieran Stockley (University of Nottingham), Stephenson Strobel

(Cornell University), Ethan Struby (Carleton College, Boston College, and Minnesota Supercomputing Institute), John Tang (Utrecht University), Idil Tanrisever (University of California, Irvine), Thomas Tao Yang (Australian National University), Ipek Tastan (University of Calgary), Dejan Tatić (WU Vienna University of Economics and Business), Benjamin Tatlow (University of Nottingham), Féraud Tchuisseu Seuyong (Université de Montréal), Rémi Thériault (Université du Québec à Montréal), Vincent Thivierge (University of California, Berkeley), Wenjie Tian (University of Ottawa), Filip-Mihai Toma (California Institute of Technology), Maddalena Totarelli (University of Amsterdam), Van-Anh Tran (Monash University), Hung Truong (Simon Fraser University), Nikita Tsoy (INSAIT, Sofia University), Kerem Tuzcuoglu (Bank of Canada), Diego Ubfal (World Bank), Laura Villalobos (Salisbury University), Julian Walterskirchen (University of the Bundeswehr Munich), Joseph Tao-yi Wang (National Taiwan University), Vasudha Wattal (The University of Manchester), Matthew D. Webb (Carleton University), Bryan Weber (College of Staten Island - CUNY), Reinhard Weisser (University of the West of England), Wei-Chien Weng (National Taiwan University), Christian Westheide (University of Vienna and Leibniz Institute for Financial Research SAFE), Kimberly White (Ludwig Maximilian University of Munich), Jacob Winter (University of Toronto), Timo Wochner (Ludwig Maximilian University of Munich and ifo Institute), Matt Woerman (Colorado State University), Jared Wong (Yale University), Ritchie Woodard (University of East Anglia), Marcin Wroński (SGH Warsaw School of Economics), Gustav Chung Yang (National Taiwan University), Myra Yazbeck (University of Ottawa), Luther Yap (Princeton University), Kareman Yassin (Alexandria University and Carleton University), Hao Ye (University of Pennsylvania / Community for Rigor), Jin Young Yoon (Queen's University), Chris Yurris (McGill University), Tahreen Zahra (Carleton University), Mirela Zaneva (University of Oxford), Aline Zayat (University of Ottawa), Jonathan Zhang (McMaster University), Ziwei Zhao (University of Lausanne and Swiss Finance Institute), Yaolang Zhong (University of Warwick)

Computational reproducibility. Abel Brodeur (University of Ottawa and Institute for Replication), Joanne Haddad (ECARES, Université Libre de Bruxelles), Pu Sun (University of Ottawa)

Local organizer Replication Games. Marie Connolly (UQAM), Romain Gauriot (Deakin University), Leonard Goff (University of Calgary), Christoph Huber (WU Vienna University of Economics and Business), Andreas Kotsadam (Ragnar Frisch Centre for Economic Research), Diego Marino Fages (Durham University)

A.2 Guidelines for Choosing a Study

For the replication games, participants are assigned to a small team of about 3–5 researchers. Ideally, all researchers on a team are working in a similar field/subfield and have similarly preferred programming languages. Participants are then offered a short list of (about 5) studies in their field of interest about three weeks before the games. They are asked to choose a paper as a team. They are provided the following guidelines for choosing a study:

> Please read the Readme files to check for (i) (too) large data set/running time, (ii) software being used, (iii) completeness of the raw data. If none of these studies is interesting enough, please let us know ASAP so that we can suggest other studies with publicly available codes/data.
>
> The choice of which paper to replicate is very important. Avoid choosing a study using (i) methods you are not familiar with, (ii) use super computer or very long running time, (iii) only share final data set or (iv) data set in a language none of you can read.
>
> Last, avoid choosing a paper for which you have a conflict of interest (e.g., friend, coauthor).

A.3 Computational Reproducibility

Computational reproducibility is defined following the Guide for Accelerating Computational Reproducibility in the Social Sciences (https://bitss.github.io/ACRE/). See Hoces de la Guardia et al. (2024) for more details. Each level of computational reproducibility is defined by the availability of data and materials, and whether the available materials faithfully reproduce the display item of interest. The description of each level also includes possible improvements that can help advance the display item's reproducibility.

The classification of computational reproducibility is determined by the accessibility of data and materials, as well as the extent to which the provided materials accurately reproduce the numerical results. Each level's description encompasses potential enhancements over the previous level.

Note that the assessment is made at the journal article level using responses from the team survey. The assessment employs a 10-point scale, with 1 indicating that, given the existing conditions, replicators have no access to any reproduction package. On the other end of the scale at level 10, the replicators have full access to all essential materials, enabling faithful computational reproduction starting from the raw data.

The following is a direct reproduction from the Guide for Accelerating Computational Reproducibility in the Social Sciences.

> **Level 1 (L1)**: No data or code are available. Possible improvements include adding: raw data, analysis data, cleaning code, and analysis code.
>
> **Level 2 (L2)**: Code scripts are available (partial or complete), but no data are available. Possible improvements include adding: raw data and analysis data.
>
> **Level 3 (L3)**: Analytic data and code are partially available, but raw data and cleaning code are missing. Possible improvements include: completing analysis data and/or code, adding raw data, and adding analysis code.

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

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

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

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

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

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

Level 9 (L9): All the materials (raw data, analytic data, cleaning code, and analysis code) are available. The analysis code produces the same output as presented in the paper (CRA). However, the cleaning code fails to run or produces different results from those presented in the paper (not CRR). Possible improvements: debugging the cleaning code.

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

A.4 Formal Tests for P-Hacking and Publication Bias

In this subsection, we first formally document the extent of p-hacking and publication bias in our sample or original studies. We conduct tests designed to detect p-hacking and publication bias introduced by Brodeur et al. (2020) and Elliott et al. (2022). We then formally document how the re-analyses changed the distribution of test statistics.

A.4.1 Presence of p-Hacking and Publication Bias in Original Studies

We adopt diverse methodologies introduced by Brodeur et al. (2020) and Elliott et al. (2022) as our foundation. Our initial focus is on randomization tests, as designed by Brodeur et al. (2020) to affirm the

visually apparent discontinuities near conventional statistical thresholds. We assess whether the concentration of test statistics just above versus just below these thresholds significantly differs between the original studies and the re-analyses.

We operate under the assumption that the underlying distribution of p-values (for any research method) is continuous and infinitely differentiable. Any observed discontinuity in p-values is inferred to result from p-hacking or publication bias.

It's pertinent to note that publication bias is likely to operate predominantly in a single direction (towards significance), as an excess of successes is more indicative of bias than a scarcity. Hence, one-sided p-values are considered for our tests. The outcomes are detailed in Table 20 for the 5% threshold. In the first panel we use observations where $(0.01 < p < 0.09)$. The lower panels use smaller windows. In the first panel, 77.2% of the original analysis p-values within this window are significant. A test for whether this proportion is statistically greater than 0.50 yields a p-value of 0.000. Similarly, we obtain very small p-values for the smaller windows, confirming the presence of p-hacking or publication bias in the sample of original studies.

We further test for the presence of p-hacking and publication bias by employing the methodology and code by Elliott et al. (2022), and conducting six distinct tests to assess p-hacking and publication bias: Binomial, Fisher's, Discontinuity, CS1, CS2B, and LCM. The outcomes are detailed in Appendix Figure 47. This figure present p-curves and test statistics for the battery of p-hacking tests for the full sample in the first panel, for the economics subsample in the second, and the political science subsample in the third.

In the absence of p-hacking and publication bias, the p-curve should be non-increasing; a spike just to the left of the 0.05 threshold is indicative of p-hacking. This spike is present in the full sample, though larger in the political science subsample than the economics subsample.

Tests based on non-increasingness include the Binomial Test and Fisher's test. Only for the political science subsample is there sufficient evidence to reject the null that the density (PDF) of p-values is non-increasing. In the absence of p-hacking, the PDF is continuous. Again, only for the political science subsample is there sufficient evidence to reject the null that the density (PDF) of p-values is continuous.

Under general assumptions, p-curves are completely monotone (the CS1 test) and are upper bounded in PDF and its derivatives (CS2B test). Here the trend reverses, in that only the full sample and the economics subsample offer sufficient evidence to reject the null of monotonicity and violations of the upper bound and derivatives of the PDF.

Last, a consequence of hypothesizing the non-increasingness of the PDF is that the PDF is also concave. The LCM test (Least Concave Majorant) assesses concavity of the CDF of p-values. Again, only the full sample and the economics subsample offer sufficient evidence to reject the null of concavity.

Overall, we take this mixed evidence to indicate the presence of p-hacking in both the economics and political science subsamples, as well as the full sample.

A.4.2 Impacts of Re-Analyses

We now turn to formally documenting how re-analyses impact the extent of p-hacking and publication bias. We first rely on caliper tests as per the methodology outlined in Gerber and Malhotra (2008). Caliper tests scrutinize test statistics within a narrow range, slightly above and below a statistical signif-

icance threshold. The rationale behind this approach is rooted in the assumption that in the absence of manipulation, be it due to publication bias or p-hacking, we would anticipate a comparable frequency of test statistics falling just below a significance threshold and those falling just above it.

We estimate probit models where the dependent variable is a dummy variable that takes the value one if a test statistic is statistically significant at the 5%-level, and zero otherwise:

$$Pr(Significant_{pr} = 1) = \Phi(\alpha + \lambda Reanalysis_r) \tag{1}$$

where $Significant_{iajt}$ is a dummy variable for whether p-value p in report r is statistically significant at the 10%, 5% or 1%-level. We rely on probit models throughout and present the average marginal effects and associated standard errors clustered at the report-level. The variable of interest is $Reanalysis$, which represents a dummy variable that takes a value of one if the p-value is associated with a re-analysis, and zero if it is associated with the original publication.

The estimates are reported in Appendix Table 21 for the 5% significance threshold. See Appendix Tables 22 and 23 for the other statistical significance thresholds. In column 1, we restrict the sample to [0.05±0.04]. The other columns repeat the specification in column 1 but with narrower bandwidths.

We find that test statistics in replication reports are about 10-20 percentage points less likely to be statistically significant than an estimate in an original study. We find the opposite result for the 10% level. The point estimates are positive, highlighting that many original point estimates move from the 5% and 1% significance regions to the 10% region. The point estimates for the 1% level are all small and statistically insignificant.

We then rely on an application of Andrews and Kasy (2019). The results are presented in Appendix Table 24. The columns μ, τ, and df represent the model's estimated parameters (using an underlying t-distribution and symmetric sign probabilities). The fourth column $[0, 1.645]$ presents the relative publication probability for a t-statistic in the $[0, 1.645]$ interval compared to one in the reference interval of $(2.576, \infty)$.

We find that a not statistically significant test statistic is 17% as likely as a very statistically significant test statistic to be observed (published). Similarly, for the $(1.645, 1.96]$ interval, the original analyses offer only a 38% relative publication probability. These findings suggest that original articles in our sample suffer from severe publication bias.[67]

As a comparison, we estimate that the same relative 'publication' probability for our re-analyses. This comparison serves only as a benchmark since re-analyses are not submitted for publication and thus do not suffer from publication bias. Nonetheless, we see this comparison as insightful. We find that the relative 'publication' probability for a re-analysis jumps to 27% from 17%. This trend continues for the $(1.645, 1.96]$ interval, where we observe a 66% relative publication probability in a re-analysis versus 38%. For the relative publication probability of test statistics significant at the 5% level, the original analyses offer an almost equal probability of 96%, whereas the re-analysis is now slightly lower than the original at 89%.

[67] The second and third panels offer a similar analysis for the economics and political science subsamples, respectively. The economics subsample behaves similarly to that of the full sample. The political science subsample behaves similarly, with the exception of the not statistically significant interval where the original analysis is more likely to have not statistically significant result published.

A.5 Types of Re-Analyses

We group re-analyses into eight groups: (i) alternative control variables, (ii) change the sample, (iii) change (coding of) the dependent variable, (iv) change (coding of) the main independent variable, (v) change estimation method, (vi) change inference method, (vii) change weighting scheme and (viii) replication using new data. We provide examples for each group in what follows.

Alternative control variables: Removing, adding or changing control variables. In our sample, there are 1,939 new re-analyses involving alternative controls.

Change the sample: Decreasing or increasing the sample size. In our sample, there are 1,774 new re-analyses involving changing the sample size. Replicators may change the sample by adding/removing years, geographical units or individuals. For instance, a team could check if the results are robust to adding/removing a state to/from the analytical sample.

Change (coding of) the dependent variable: The replicators may change the coding of the dependent variable. In our sample, there are 285 new re-analyses involving changing the dependent variable. Examples include using an alternative standardization of the outcome variable and using a composite index of several indicators as the dependent variable.

Change (coding of) the main independent variable: The replicators may change the coding of the main independent variable. In our sample, there are 264 new re-analyses involving changing the main independent variable. An example is using a continuous variable instead of a dummy variable for treatment.

Change estimation method: This category involves any changes to the estimation method. In our sample, there are 605 new re-analyses involving changing the estimation method. Examples include using non-linear models and changing the variables used for matching.

Change inference method: This category involves changing the inference method. In our sample, there are 542 new re-analyses involving changing the inference method. Examples include bootstrapping the standard errors and clustering at a different level.

Change weighting scheme: This category involves changing the weighting scheme. In our sample, there are 126 new re-analyses involving changing the weighting scheme. Examples include removing a weighting scheme used by the authors.

Replication using new data: Replication using new data involve both collecting new data or using data from another data source. In our sample, there are 469 new re-analyses involving using new data. Replicators have used new data for the dependent, independent or control variables.

A.6 Many-Analysts: Methodology

A.6.1 Team Construction

We asked a subset of coauthors on this paper (replicators) if they would like to help analyse our Meta Database. We informed them that we would "have different teams independently working together

at answering the same research questions (e.g., what is the reproducibility/replicability rate for each specific type of robustness checks/recoding)." The subset of coauthors who received an invitation to volunteer were: (1) contacted between September 21st and October 8th *and* (2) had completed, or were near completion of, their replication report. We sent invitations (a simple sign-up form) in an email which also asked the replicators to respond to individual and team leader surveys which formed parts of our previous analysis. As a crude lower bound on the number of individuals who were invited between September 21st and October 8th, we had 87 individual surveys completed.[68] When we closed the period for volunteering on October 8th, we had 10 individuals sign-up as "meta-analysts."

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

A.6.2 Meta Database

After pre-registering our procedures (https://osf.io/8wsqx/), we provided all of our analyst teams with the link to a folder which contains four documents: (1) Meta Database as a *.dta document; (2) Clarifying Questions and Comments document with a *.txt extension; (3) Reporting Guidelines excel file showing how we liked teams to report their results; and (4) an Analysts Document for Variables and Variable Labels as a *.docx. The Meta Database is the dataset which they can conduct their analysis on, although they may also use new data. The *.txt file is a running set of questions/comments the analysts can provide to I4R to improve the database.

While we had constructed the majority of the Meta Database when sharing it to all teams (October 2023), we still had replication reports and surveys being entered. That is, the dataset initially provided to all teams was not yet completely built. As such, analysts obtained an updated and final version of the dataset in February 2024.

We provided a link to the variable names and their labels associated with the Meta Database. Of note, teams had access to the affiliation of the replicators who desired to remain anonymous. This information is not provided in the Publicly Available Meta Database. This dataset omits all information which could be used to identify the replicators.

In what follows, we provide a brief description of what some teams have done.

A.6.3 Team One

Team one is composed of only one researcher, Nikolai Cook. This subsection contains a detailed account of his efforts.

[68] We also had 36 team leader surveys completed in that time. With an average of 3 people per team, another crude estimate would be about 108 individuals.

Before moving to the 12 research questions posed to the many-analysts, I considered the effects of weighting throughout this analysis. In a dataset that only examines the original research questions, one row (observation) represents a single point estimate. That is, without weights, each estimate would be equally weighted in a regression. Because of the nature of this dataset (where each row is a reproduction effort) there are multiple reproduction estimates for each original estimate. Further, not all original author estimates are attempted by the replicators - it is likely that the replicators chose estimates that were more central to the paper, easier to reproduce, or even simply more interesting. There is also the consideration that the result of a first reproduction can affect where replicators place their efforts. It is not unclear a priori that if, when replicating, I were to find a similar result as the original authors I would be more or less inclined to continue testing its sensitivity. The same holds true in the converse, it is unclear whether I would be more or less inclined to deeply examine an estimate if I find that it is wildly different following small specification changes. For these reasons, I have applied article weights to my entire analysis. In this manner, all reproductions have the same effect at the article level. That is a reproduction which examines one estimate 10 times has the same weight as a reproduction that examines 10 estimates once.

Reasoning along the same lines, that the rows in this dataset are not independent and likely have strong correlations in the error terms between estimates at the paper level, lead me to apply clustering of my standard errors at the same level.

The remainder of this section details each research question, the logic behind the specification, and the result of my analysis. All regressions are estimated using ordinary least squares; as the dependent variables are all indicators please note these are all linear probability models with all the attendant caveats.

1. Does reproducibility/replicability rate depend on replicators' experience coding?

   In the absence of directly observing a survey response along these lines, I have chosen to model experience coding in the same manner as assuming work experience minus some constant is equal to the respondent's age from labor economics. I use here years since PhD as my primary independent variable. In my first specification, this means that the coefficient on the independent variable may be interpreted as, a one year increase in the replicator's average years since PhD represents a 2.5% decrease in the probability that an originally statistically significant result at the 5% level is reproduced. With a p-value of 0.020, this result is statistically significant well within conventional thresholds.

   In a second specification of the same variables (row 5 in the automated output), I note that there are non-negligible observations associated with zero average years since PhD (the lowest value is zero, representing all student groups) or zero citations. A group without a PhD and without citations may have experience coding, but I hold greater confidence that the subsample of replicators with at least some time with a PhD and a non-zero amount of citations have coding experience commensurate with how long they have held those doctorates. The second specification then does not change the dependent or independent variables, but changes the estimation subsample. The resulting coefficient is similar (-0.027) and of similar, albeit slightly less strong, statistical significance ($p = 0.035$).

In the second row, the only difference from the first is substituting the dependent variable. The coefficient can now be interpreted as the effect of a one year increase in the replicators' average years since PhD on the probability the result was replicated conditional on the fact the original result was statistically significant at the 10% level. Perhaps surprisingly, I find a similar effect to the first dependent variable; a one year average increase in PhD experience leads to a 2% reduction in replicating the original result. Together with the earlier finding, experience with a PhD (my proxy for coding experience absent survey information) seems equally as effective at reproducing a very statistically significant and a marginally statistically significant original result.

In the third and fourth rows, the dependent variables now indicate successful replication of originally statistically insignificant results at the 5% and 10% levels, respectively. In both cases the coefficient on average years since PhD is reduced in magnitude by a factor of around 5, and becomes statistically insignificant (p=0.609 and $p = 0.763$, respectively).

One key variable asked by the Institute for Replication of the analysts is that, given the coefficient the analysts observe is it "a meaningful effect." Leaving interpretation to the analyst, here I would argue no - the unweighted mean of the dependent variable is approximately 77% and the standard deviation (again unweighted) of the average years a replicator team with PhD has is 2.7, a one standard deviation shift represents only around a 5% decrease in probability of reproducibility. Throughout my analysis, I use a perhaps naive but consistent rule. If the coefficient of the primary independent variable represents a shift in 10% of the mean of the dependent variable, I conclude that the effect is meaningful.

2. Does reproducibility/replicability rate depend on replicators' academic experience?

   The second research question asks about the reproducibility rate being affected by the replicators' academic experience. I interpret academic experience as time spent as a professor. In doing so I generate my primary independent variable of "research team has a professor" an indicator that takes a value one only if at least one of the replicators is an assistant, associate, or full professor (with no distinction between them). I also consider academic experience to be near zero without publication, and so exclude replicator-results if the average citations of the replicator team is zero. This definition and sample restriction effectively asks, given a team has published something in the past, is there an effect of having someone active in academia? Approximately 75% of replication results are assigned a value of one for the primary independent variable.

   I find a large and statistically significant effect of having someone experienced in academia on the probability of a result being replicated, regardless if the original result was statistically significant at the 5% or 10% level. Respectively, having at least one professor reduces the probability of replication by 20.5% and 14.6% for the two thresholds ($p = 0.001$ and $p = 0.002$). I commensurately indicate that yes, this is a meaningful effect for the later many-analysts' subjective magnitude variable.

   In a second specification of these two regressions, I now include controls for whether any of the replicators have published in a top 5 economics or one of the 3 political science journal, and separately for whether they have done so in the past 5 years. Even with these controls for different

publication-experience, the coefficient for professor inclusion remains similar in magnitude and statistical significance.

Once again, the magnitude and statistical significance of the coefficient on the primary independent variable are small and statistically insignificant when the dependent variable considers originally not statistically significant results.

3. Does reproducibility/replicability rate depend on the authors' experience?

   When defined as the average of the original authors' years since PhD, or the same but conditional on whether one of the original authors has tenure, there seems to be no effect of the original authors' experience on the later replication rate. The lowest p-value for the 8 specifications (4 dependent variables, 2 specifications each) is 0.522.

4a. Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (i) Are reproducibility/replicability rate higher when authors' experience is high, and replicators' experience is low (in comparison to similar levels)?

   These research questions invite the reader at first to compare the replicator and the authors at the same levels of experience. For example, comparing tenureship at the same bar. However, when examining the underlying data, there are clear differences between who is publishing and who is willing to prepare replications. On average, replicators are younger, less likely to have tenure, and less likely to be a professor. It is with these different characteristics in mind that my bar for authors' experience being high would be different than replicators' experience being high. In an effort to maximize the reader's anecdotal intuition, I have chosen to compare original authors with tenure to replicator teams that have a professor, representing 85% and 75% of the replication results, respectively. An alternative may have been to use standardized years of experience, however I believe the value of these goalposts or obviously identifying features is easier to contextualize. Further, by creating a variable which indicates the intersection, I have identified 24% of reproduction results.

   Compared to all other teams, when the original authors are tenured and the replicators at least have one professor on their team, the reproducibility rate is 17.2% higher ($p = 0.010$) in the case of originally 5% statistically significant results and 15.3% higher ($p = 0.001$) in the case of originally 10% significant results. This indicator identifies the "best" of the authors being replicated by the "best" of the replicators, leading to a large and statistically significant increase in the reproducibility rate.

   As with the previous research questions, when the dependent variable concerns the replication of statistically insignificant results, none of the coefficients of interest are large or statistically significant themselves.

   If instead of defining academic experience as high or low based on relative accomplishment of certain milestones, I define an indicator that takes a value one if the original authors have more citations, on average, than the replicators, I find similar results. While no doubt a crude measure, this indicator takes the value zero a surprising 22% of the time - that is there is a non-negligible

amount of replications being done by replicators quite experienced in academia, and arguably even moreso than their original author counterparts.

4b. Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (ii) Are reproducibility/replicability rate higher when authors' experience and replicators' experience is similar (in comparison to dissimilar levels)?

In answering this research question, I compare the intersection of replications that are done by teams with a professor and on original results produced by an authorship which has at least someone with tenure. This represents approximately 60% of the reproduction results. In the alternative definitions, I use as an indicator the difference between the authors' citations and the replicators' citations being less than 2000 (identifying 57% of the sample). Regardless of which of these two definitions are used and which of the four dependent variables are examined, there is not enough evidence to reject the null of no relationship between author and replicator experience and the reproducibility rate.

4c. Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (iii) Are reproducibility/replicability rate higher when authors' experience is low, and replicators' experience is high (in comparison to similar levels)?

In answering this research question, I either use an indicator which takes a value one if the original author team does not have tenure and the replication team does have a professor (15% of results) or identify when the average of the replicator team's citations is higher than the original authors (22% of results).

The results mirror those of 4a. Now, whenever authors experience is low and replicator experience is high, the replication rate is 20.6% lower ($p = 0.005$) and 16.0% lower ($p = 0.002$) depending on original estimate significance level. The second definition offers similar results, with 18.2% ($p = 0.005$) and 13.9% ($p = 0.077$) lower replication rates, respectively.

5a. Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (i) Are reproducibility/replicability rate higher when authors' have high prestige, and replicators' experience have low prestige (in comparison to similar levels)?

In order to examine prestige, I follow previous work in using RePEc's top institutions lists. Using the same list for both original authors and replicators affiliations, I find that 41% of reproduction results are prestigious original authors and 9% prestigious replicators. Generating an indicator for the interaction where original authors have prestige and replicators do not creates a value of one 36% of the time. Only when it comes to originally statistically significant results are there large and statistically significant coefficients of 14.3% and 13.3% increases in reproducibility ($p = 0.056$ and $p = 0.013$, respectively).

In a second specification, I compare the prestige of original author affiliations with replicator PhD prestige (allowing for the possibility that the replicator's affiliation and PhD program may be differently prestigious in my measure). An indicator which takes a value of one when original authors are prestigious and the PhD training of the replicator is not prestigious represents 30% of the results. As before, there is a large and statistically significant coefficient representing a 17.7% and

13.9% increase in the replication rate ($p = 0.007$ and $p = 0.006$, respectively for the two original statistical significance thresholds).

Once again, when originally not statistically significant original results are examined, there is insufficient evidence to reject the null hypothesis of a relationship between this interaction and the replication rate.

5b. Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (ii) Are reproducibility/replicability rate higher when authors' and replicators' prestige is similar (in comparison to dissimilar levels)?

In order to compare authors and replicators with similar levels of prestige, I generate an indicator which takes a value one if both the authors and replicators are affiliated with non-prestigious institutions or if both authors and replicators are affiliated with prestigious institutions. My second specification will instead use the replicators' PhD (rather than affiliation) prestige. This indicator takes a value one 59% and 56% of the time.

There is a large and statistically significant decrease in the replication rate when there are similar levels of prestige: 14.0% and 13.1% ($p = 0.062$ and $p = 0.015$) lower in the case of the first definition and 17.3% and 12.1% lower ($p = 0.011$ and $p = 0.027$) in the case of the second definition.

Once again, when originally not statistically significant original results are examined, there is insufficient evidence to reject the null hypothesis of a relationship between this interaction and the replication rate.

5c. Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (iii) Are reproducibility/replicability rate higher when authors' have low prestige, and replicators' experience have high prestige (in comparison to similar levels)?

Authors have low prestige and replicators have high prestige, according to the definitions above, around 5% of the time. The indicator rises to 14% of the time if replicator PhD prestige is used instead. Regardless of definition, I do not find any evidence of a relationship between these interactions and reproducibility rate for originally statistically significant effects. Nearly uniquely in this analysis however, I do now reject the null when not-statistically significant original estimates are not more likely to be reproduced (though only in the first specification which uses affiliations and not in the second which uses replicator PhD prestige).

6 Does reproducibility/replicability rate depend on the original authors providing raw data?

Original authors provide raw data 45% of the time. I find insufficient evidence to reject the null hypothesis of no effect of the original authors providing raw code and the replication rate. A second specification, which refines the sample into only those that also provide full cleaning code, finds similar.

7 Does reproducibility/replicability rate depend on the original authors providing raw or intermediate data?

If instead, I create an indicator that takes a value one if original authors provide raw data or provide intermediate data (value one 80% of the time) and restrict the sample to reproductions where

final data is not provided, I find a large, meaningful, and statistically significant reduction in the probability of replication to the tune of 23.4% and 16.2% ($p = 0.014$ and $p = 0.016$) for originally statistically significant results at the 5 and 10% levels, respectively.

8 Does reproducibility/replicability rate depend on the original authors providing cleaning code?

If I create an indicator which takes a value one if cleaning code is provided and only raw data is provided (that is intermediate or final data is not), I find replicators are less likely to reproduce originally not statistically significant results at a rate of 24.6% and 27.9% respectively ($p = 0.005$ and $p = 0.083$).

A.6.4 Team Two

Team two is composed of Abel Brodeur and Derek Mikola.

We met to discuss how we'd approach the research questions on December 15th, 2023 and again on December 17th, 2023 and December 20th, 2023. At these time, we made some general decisions which guided our analysis:

- We will have two estimations for each research questions: one unweighted, and one weighted by the inverse number of tests in each report.
- Each question (or subquestion) would have a separate model, focusing on one specific parameter estimate.
- Ordinary least squares would be used to estimate models which were linear in parameters.
- Standard errors would be clustered at the report level (alternatively thought of as the team-level or original paper-by-team level).
- Models would be parsimonious in controls. In general, our controls included a combination of: number of authors, number of replicators, a binary variable for if the paper was published in a political science journal, dummy variables for journals, percentage of a team which were professors(both replicators and original authors), dummy variables for the broad categories of robustness checks.

The lion's share of the code was written just before December 25th, 2023. Adjustments to those scripts were made with successive data dumps in January and February, 2024.

In general, our results fail to reject a null hypothesis of no effect in each of the sub questions: we find six (pointwise) statistically significant coefficient estimates out of a possible 96 different models. Coefficient magnitudes are quite small relative to the mean of the dependent variables (on the estimated sample): 36 out of 96 coefficients have magnitudes greater than 5% of the mean of their model's dependent variable. Coefficient estimates generally deflate towards zero when weighting by the inverse proportion of the number of tests in each report.

Models in questions 1 and 2 look at different measures of replicators' "experience", while question 3 looks at experience of original authors. It is difficult to see any strong relationship between experience and the reproducibility/replicability rates for dependent variables 1 and 2 (where original authors'

results were statistically significant at the 5% and 10% levels, respectively). The signs of experience coefficients typically vary between small positive and small negative numbers. However, models with dependent variables 3 and 4 show experience coefficients that are positive in ten of twelve models (albeit small) and statistically significant at the 10% level in only two of twelve models. While the trend across questions is that increasing experience helps reproduction rates of *null* results, failing to reject the (pointwise) null hypothesis in 10 of 12 models doesn't provide us confidence for strong conclusions.

Questions 4(a) through 4(c) look at the interaction of experience levels between teams of replicators and original authors. None of the models have (pointwise) statistically significant coefficient estimates for the interaction of experience levels. Again, it is difficult to see a pattern in coefficient signs and magnitudes across the three models.

Question 5(a) through 5(c) look at the interaction of "prestige levels" between teams of replicators and original authors. Similar to questions 4(a) and 4(b), it is difficult to see any patterns across these three questions.

Another possible comparison across models would be between 4(a) and 5(a) (or 4(b)/5(b) and 4(c)/5(c)). That is, using two different measures of similarity in either experience/prestige/impact. For replicators with less experience/prestrige than original authors and for replicators and original authors with similar levels of experience and prestige, there does not appear to be a trending relationship between coefficient signs and magnitudes. However, when replicators are more experienced/prestigious than original authors, we see original authors' results which were (not) statistically significant are trending towards (more) less likely to reproduce.

For the remaining questions (6, 7 and 8) their respective responses below summarize the results.

1. *Does reproducibility/replicability rate depend on replicators' experience coding?*

    - Main Independent Variable: total years of programming for the team of replicators. This was given to Meta Analysts in the Meta Database.
    - Findings: Seven of eight models did not find the coefficient estimate for total years of programming for the replicators statistically to be statistically significant at the 10% level. A team's total years of programming seems to be positively related to the reproducibility rate of null results (at the 10% level) in our unweighted model (coefficient equalling about 0.39% with a p-value of about 0.046). However, the effect size halves when adding weights (coefficient equalling about 0.26% with a p-value of about 0.20). All of our eight different models found one additional year of programming for a team to be smaller than 5% of the mean of their respective dependent variables. Our largest effect size was about 0.43% of the mean of their respective dependent variable, suggesting only non-marginal changes in the years of programming may matter for reproducibility of results and replicators' experience programming.

2. *Does reproducibility/replicability rate depend on replicators' academic experience?*

    - Main Independent Variable: total years since graduating from a PhD program for the team of replicators. This was derived from the two variables: (1) average years since PhD for the team of replicators and (2) the number of replicators.

- Findings: Seven of eight models did not find the coefficient estimate for total years since graduating from a PhD program for the replicators statistically to be statistically significant at the 10% level. A team's total years since graduating from a PhD program seems to be positively related to the reproduciblity rate of null results (at the 10% level) in our unweighted model (coefficient equalling about 0.34% with a p-value of about 0.054). However, the effect size halves when adding weights (coefficient equalling about 0.17% with a p-value of about 0.31). All of our eight different models found one additional year since graduating from a PhD program for a team to be smaller than 5% of the mean of their respective dependent variables. Our largest effect size was about 0.38% of the mean of their respective dependent variable, suggesting only non-marginal changes in the years since graduating from a PhD program may matter for reproducibility of results and replicators' experience programming.

3. *Does reproducibility/replicability rate depend on the authors' experience?*

    - Main Independent Variable: total years since graduating from a PhD program for the team of original authors. This was derived from the two variables: (1) average years since PhD for the team of original authors and (2) the number of original authors.
    - Findings: Eight of eight models did not find the coefficient estimate for total years since graduating from a PhD program for the original authors statistically to be statistically significant at the 10% level. All of our eight models found one additional year since graduating from a PhD program for a team of original authors to be smaller than 5% of the mean of their respective dependent variables. Our largest coefficient magnitude was about 0.25% of the mean of their respective dependent variable, suggesting only non-marginal changes in the years since graduating from a PhD program may matter for reproducibility of results and original authors' experience programming.

4 *Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience?*

    - Variable Construction: We continued using the total years since graduating from a PhD program from the previous two questions. We rank-ordered the distribution of *both* replicators and original authors total years since graduating. Each observation in the distribution was either a team of replicators *or* a team of original authors. We then found the median value of "experience." Teams above the median were considered higher experience, while those below the median were considered lower experience.

    4a *Are reproducibility/replicability rate higher when authors' experience is high, and replicators' experience is low (in comparison to similar levels)?*

        - Main Independent Variable: A binary variable, equal to one if the original authors were above the median *and* the replicators were below the median. Zero otherwise.
        - Findings: None of our eight models had coefficient estimates which were statistically significant at the 10% level. All coefficients were positive, with magnitudes ranging between 0.58% and 8.14% of the dependent variable. For reproducibility when the original

authors' results were statistically significant (dependent variables 1 and 2) all four coefficients (weighted and unweighted, for each dependent variable) were smaller than 5% of the dependent variable. In contrast, where original authors' results were not statistically significant (dependent variables 3 and 4) all four coefficients (weighted and unweighted) were larger than 5% of the original coefficients.

4b Are reproducibility/replicability rate higher when authors' experience and replicators' experience is similar (in comparison to dissimilar levels)?

- Main Independent Variable: A binary variable, equal to one if the original authors and the replicators had similar experience levels (both high *or* both low). Zero otherwise.
- Findings: None of our eight models had coefficient estimates which were statistically significant at the 10% level. For reproducibility when the original authors' results were statistically significant (dependent variables 1 and 2), three of four coefficients (weighted and unweighted, for each dependent variable) were positive, with two of the positive coefficients being larger than 5% of the dependent variable. In contrast, where original authors' results were not statistically significant (dependent variables 3 and 4) three of four coefficients (weighted and unweighted) were negative, with two of the negative coefficients being larger than 5% of the original coefficients.

4c Are reproducibility/replicability rate higher when authors' experience is low, and replicators' experience is high (in comparison to similar levels)?

- Main Independent Variable: A binary variable, equal to one if the original authors were below the median *and* the replicators were above the median. Zero otherwise.
- Findings: None of our eight models had coefficient estimates which were statistically significant at the 10% level. All four coefficients were negative for models whose dependent variable was reproducibility of statistically significant results (dependent variables 1 and 2). In contrast, all four coefficients were positive for models whose dependent variable was reproducibility of statistically insignificant results. While six of eight coefficients were larger than 5% of the mean of the dependent variable, all weighted models deflated the coefficient estimates by about 5 to 10 percentage points towards zero.

5. *Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige?*

- Variable Construction: total citations for a team (replicators *or* original authors) as a proxy for prestige. We rank-ordered the distribution of *both* replicators and original authors total citations. Each observation in the distribution was either a team of replicators *or* a team of original authors. We then found the median value of "prestige." Teams above the median were considered higher prestige, while those below the median were considered lower prestige.

5a *Are reproducibility/replicability rate higher when authors' have high prestige, and replicators' have low prestige (in comparison to similar levels)?*

- Main Independent Variable: A binary variable, equal to one if the original authors were above the median *and* the replicators were below the median. Zero otherwise
- Findings: None of our eight models had coefficient estimates which were statistically significant at the 10% level. Coefficients ranged between -0.076 and 0.036. while the percent of the coefficient size to the mean of their model's dependent variable ranged between 0.16% and 9.6%.

5b *Are reproducibility/replicability rate higher when authors' and replicators' prestige is similar (in comparison to dissimilar levels)?*

- Main Independent Variable: A binary variable, equal to one if the original authors and the replicators had similar prestige (both high *or* both low). Zero otherwise.
- Findings: None of our eight models had coefficient estimates which were statistically significant at the 10% level. Coefficients ranged between -0.013 and 0.10. while the percent of the coefficient size to the mean of their model's dependent variable ranged between 0.72% and 13.24%.

5c *Are reproducibility/replicability rate higher when authors' have low prestige, and replicators' have high prestige (in comparison to similar levels)?*

- Main Independent Variable: A binary variable, equal to one if the original authors were below the median *and* the replicators were above the median. Zero otherwise.
- Findings: Three of our eight models had coefficient estimates which were statistically significant at the 10% level, two of which were also statistically significant at the 5% level. The three statistically significant results come from dependent variables 1 and 2 (original authors' results significant at 5% and 10% respectively). We fail to reject the null of no effect when using weights when modelling dependent variable 2. All statistically significant estimates were negative, ranging between -0.14 (-17.16% of the dependent variable's mean) and -0.20 (-27.04 of the dependent variable's mean). This suggest, that when less "prestigious" original authors' work is analysed by more "prestigious" replicators, their original results are less likely to be reproduced (statistically significant at the 5% and 10% levels). When we look at dependent variables 3 and 4, the coefficients are negative for unweighted models and positive for weighted models.

6 *Does reproducibility/replicability rate depend on the original authors providing raw data?*

- Main Independent Variable: A binary variable equalling one if the original authors provided raw data in their replication folder. Zero otherwise.
- Findings: In all eight of our models, our coefficient estimate for whether providing raw data affects the likelihood of reproduction/replication is statistically insignificant at the 10% level. That said, coefficients' magnitudes are greater than 5% of the dependent variable's mean in three of our four models (dependent variable 1, both weighted and unweighted; dependent variable 2, unweighted) where original authors' results were statistically significant. Also where original authors' results were statistically significant (dependent variables 1 and 2), all coefficient estimates associated with original authors providing raw data were positive.

Where original authors' results were not statistically significant (dependent variables 3 and 4), none of the coefficients' magnitudes were greater than 5% of the dependent variable's mean.

7 *Does reproducibility/replicability rate depend on the original authors providing raw or intermediate data?*

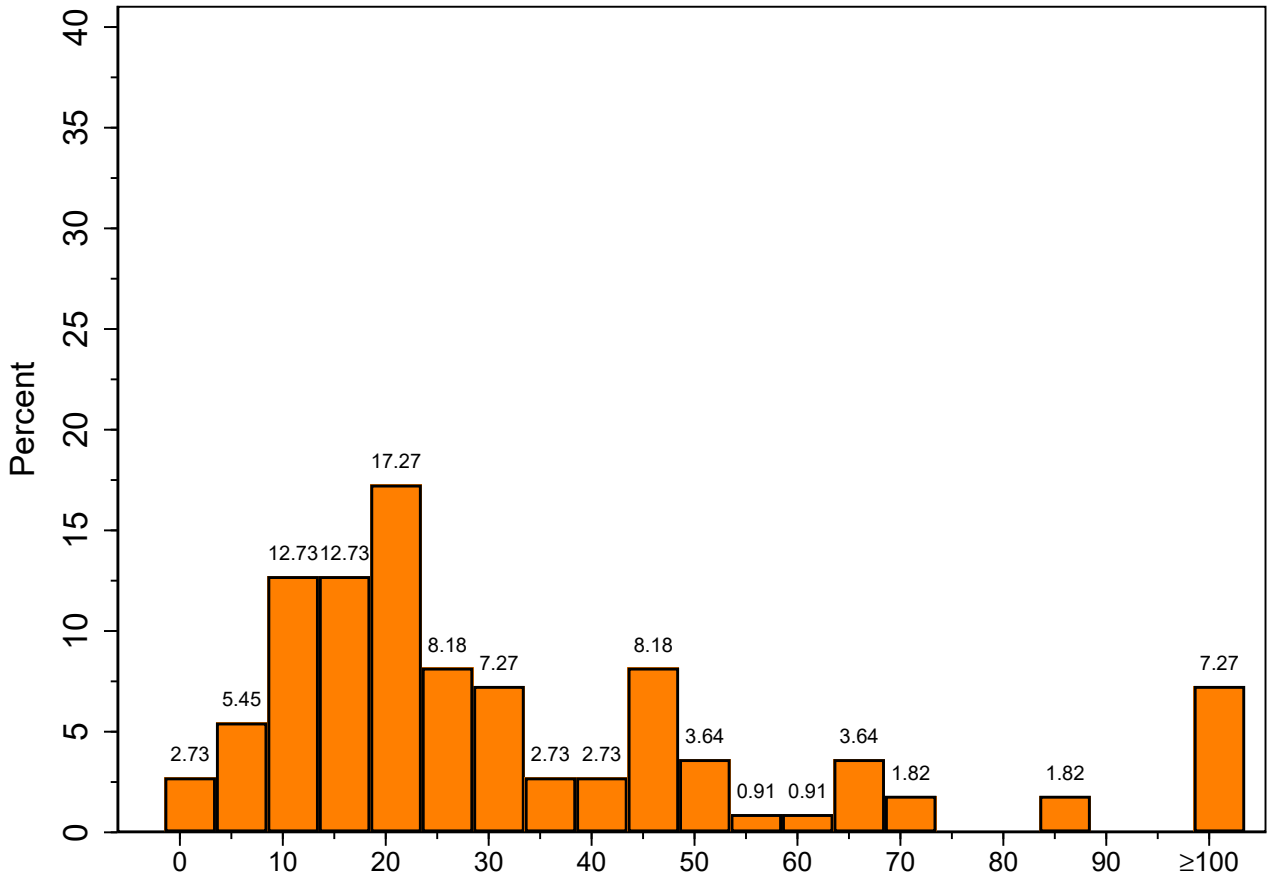
- Main Independent Variable: A binary variable equalling one if the original authors provided raw data *or* (inclusive) intermediate data in their replication folder. Zero otherwise.
- Findings: We find that providing raw or intermediate data makes replicators more likely to reproduce/replicate the results which were significant at the 10% level in the original paper (without using weights). This results disappears when we incorporate weights in the model: coefficients decrease in magnitude by about three-fourths while standard errors increase by about 20%. All eight models (four dependent variables x {weighted, unweighted}) have positive coefficients. Weighting our results seems to deflate (*inflate*) coefficient magnitudes when the dependent variable represents original papers' results which were (*not*) significant at 5 or 10% levels.

8 *Does reproducibility/replicability rate depend on the original authors providing cleaning code?*

- Main Independent Variable: A binary variable equalling one if the original authors provided cleaning code (scripts) in their replication folder. Zero otherwise.
- Findings: Across all four dependent variables and both weighted and unweighted specifications, we failed to find statistically significant estimates at the 10% level. That is, providing cleaning code seems not to affect the reproducibility/replicability rate of original authors. Coefficients magnitudes were positive and greater than 5% of the dependent variable's mean only for dependent variable one (original estimate and replicators' estimate are both significant at the 5% level) for both weighted and unweighted models.

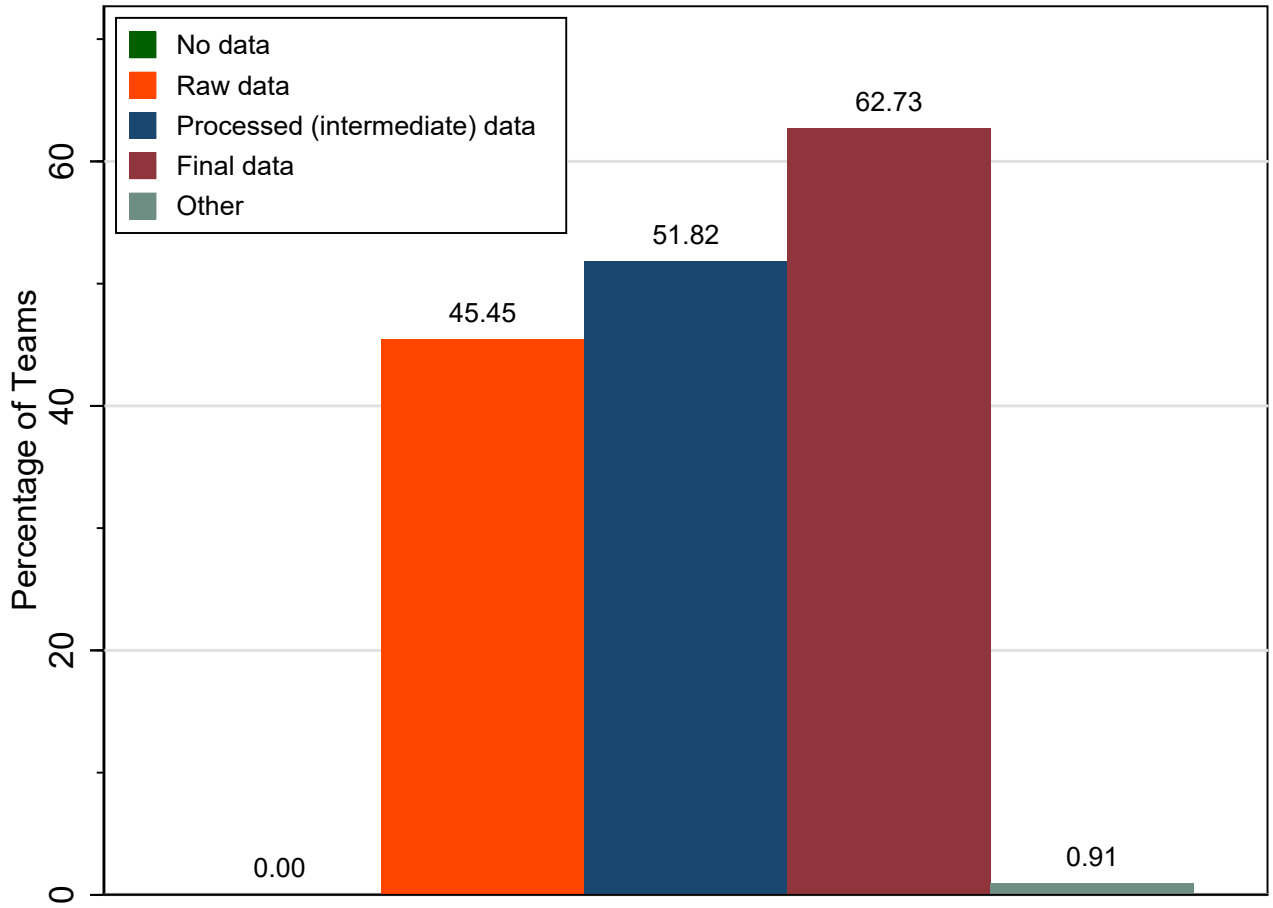
Appendix Figures

Figure 5: Histogram of Number of Active Work Days



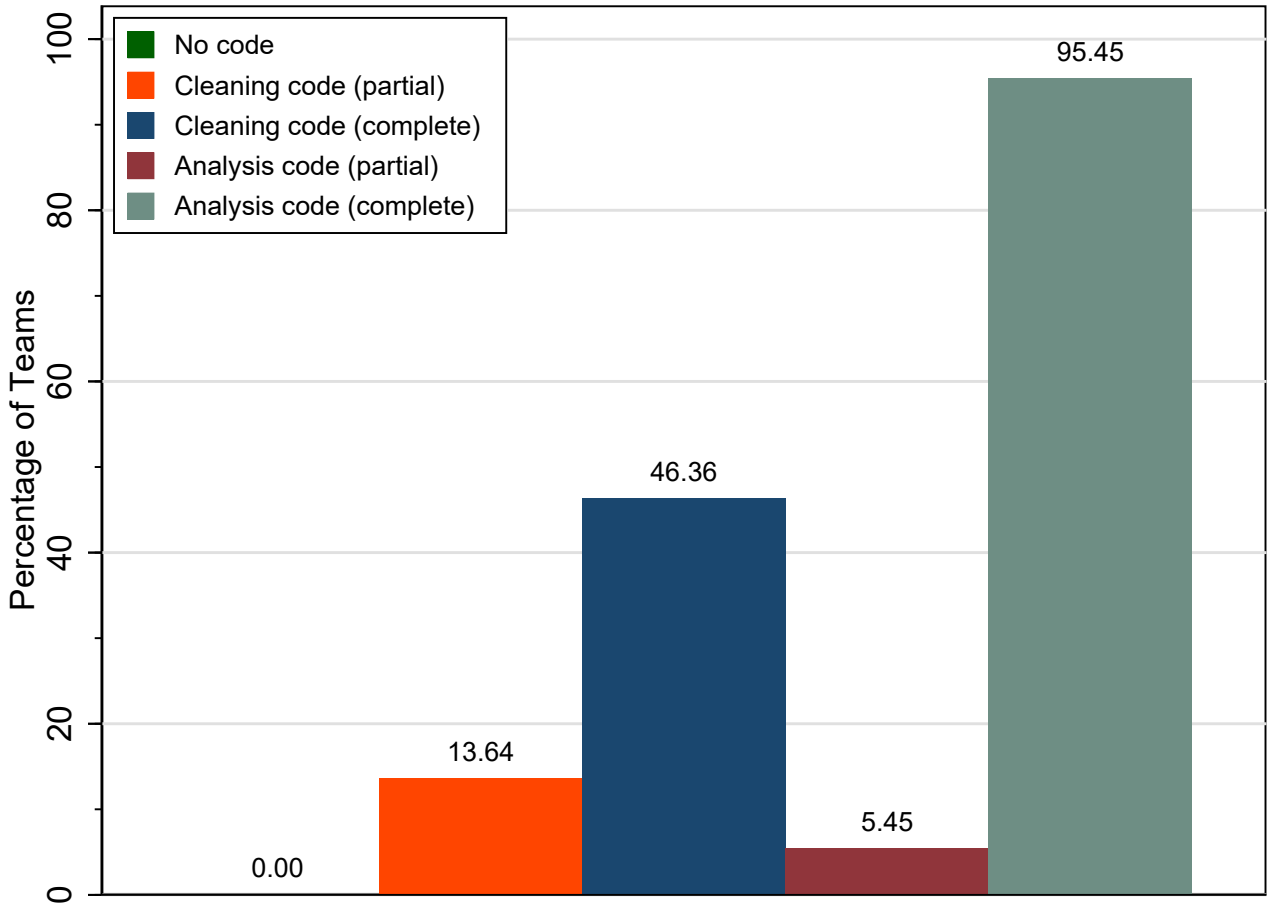
Notes: Data collected *via* survey of our replicators after completing their reports. This figure illustrates the number of active days each team worked on their report.

Figure 6: Which of the following forms of data were provided in the original authors' replication package? (Select all which apply)



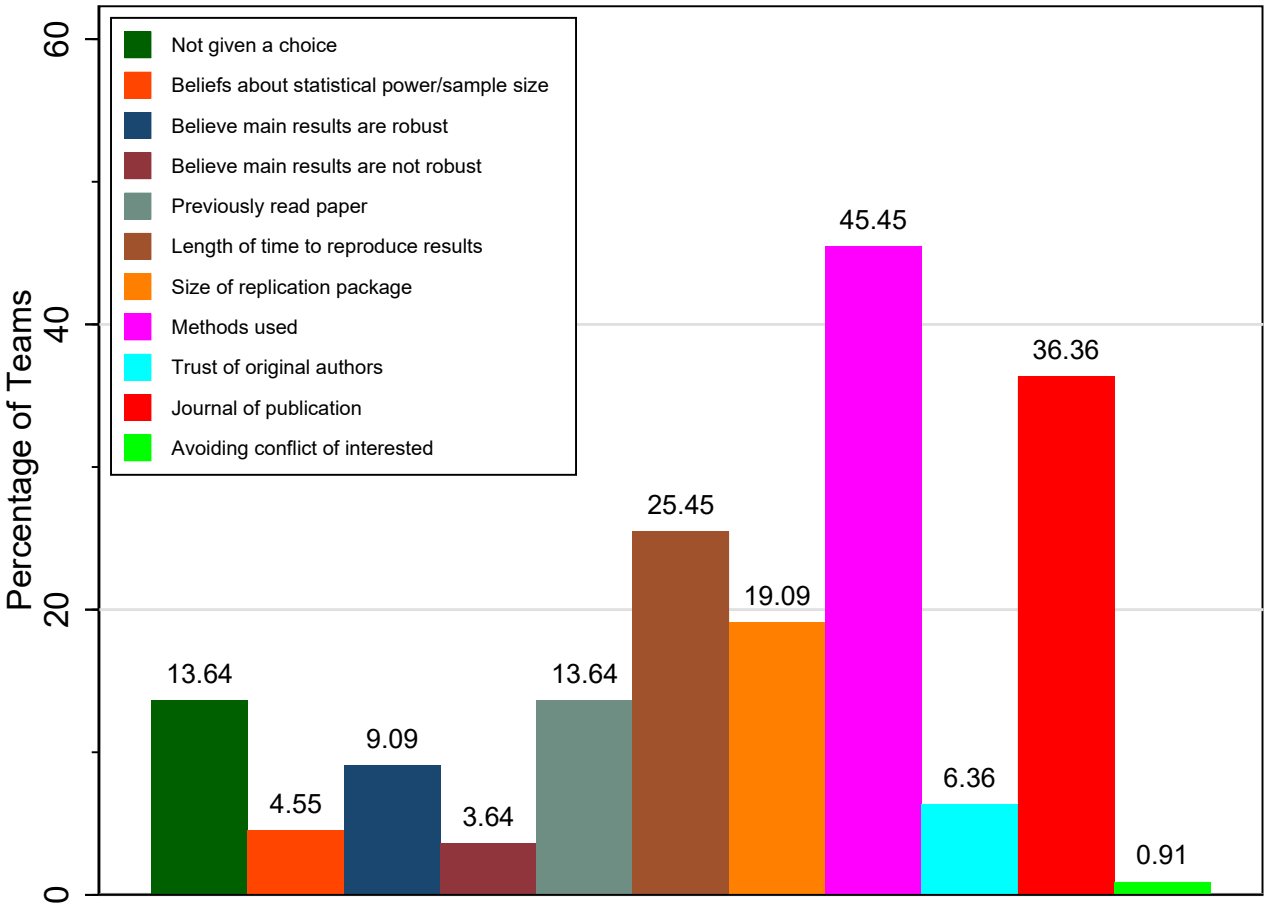
Notes: Data collected *via* survey of our replicators after completing their reports. This figure illustrates the responses to the question: "Which of the following forms of data were provided in the original authors' replication package? (Select all which apply)"

Figure 7: Which of the following forms of code were provided in the original authors' replication package? (Select all which apply)



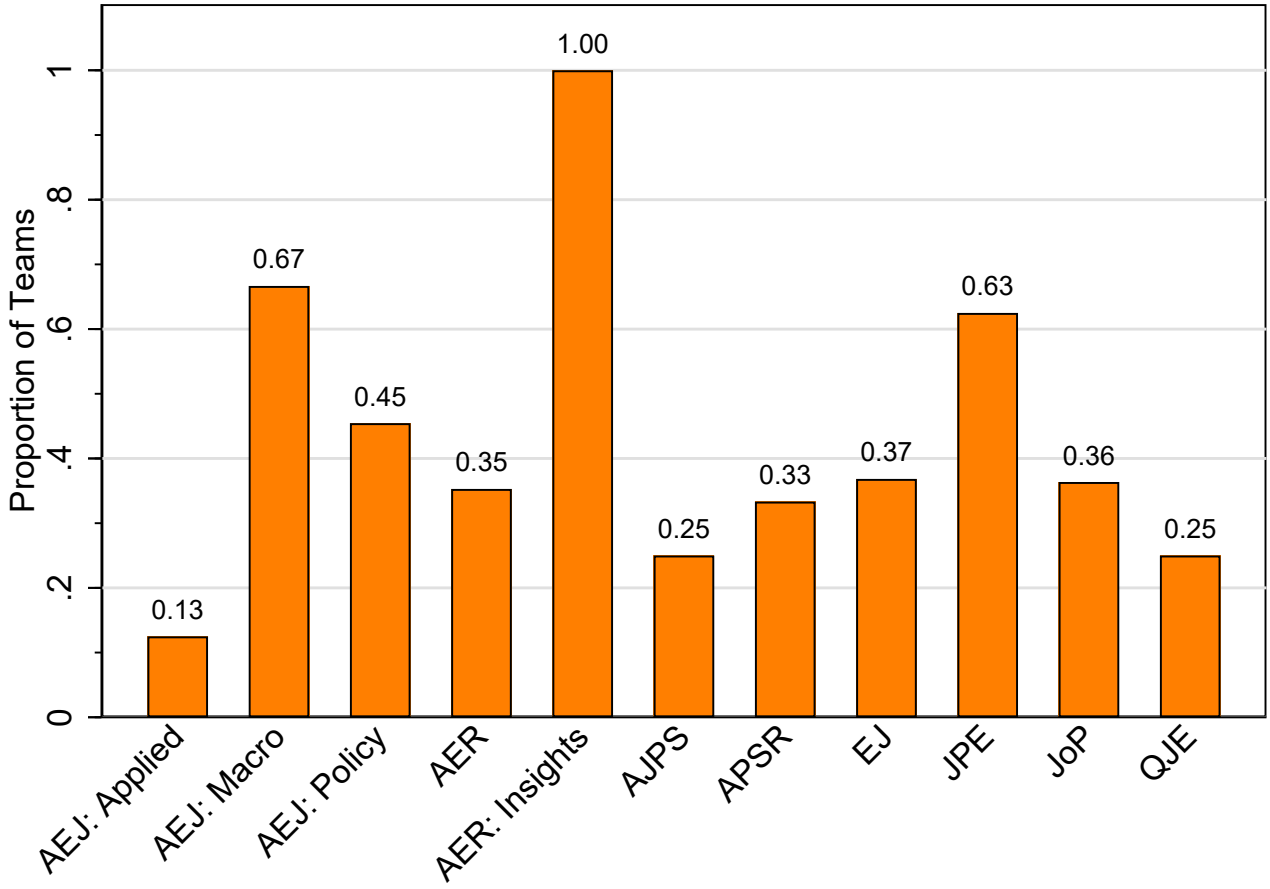
Notes: Data collected *via* survey of our replicators after completing their reports. This figure illustrates the responses to the question: "Which of the following forms of code were provided in the original authors' replication package? (Select all which apply)"

Figure 8: For what reasons did you select your specific paper to reproduce and/or replicate from the list of papers provided? (Select all which apply)



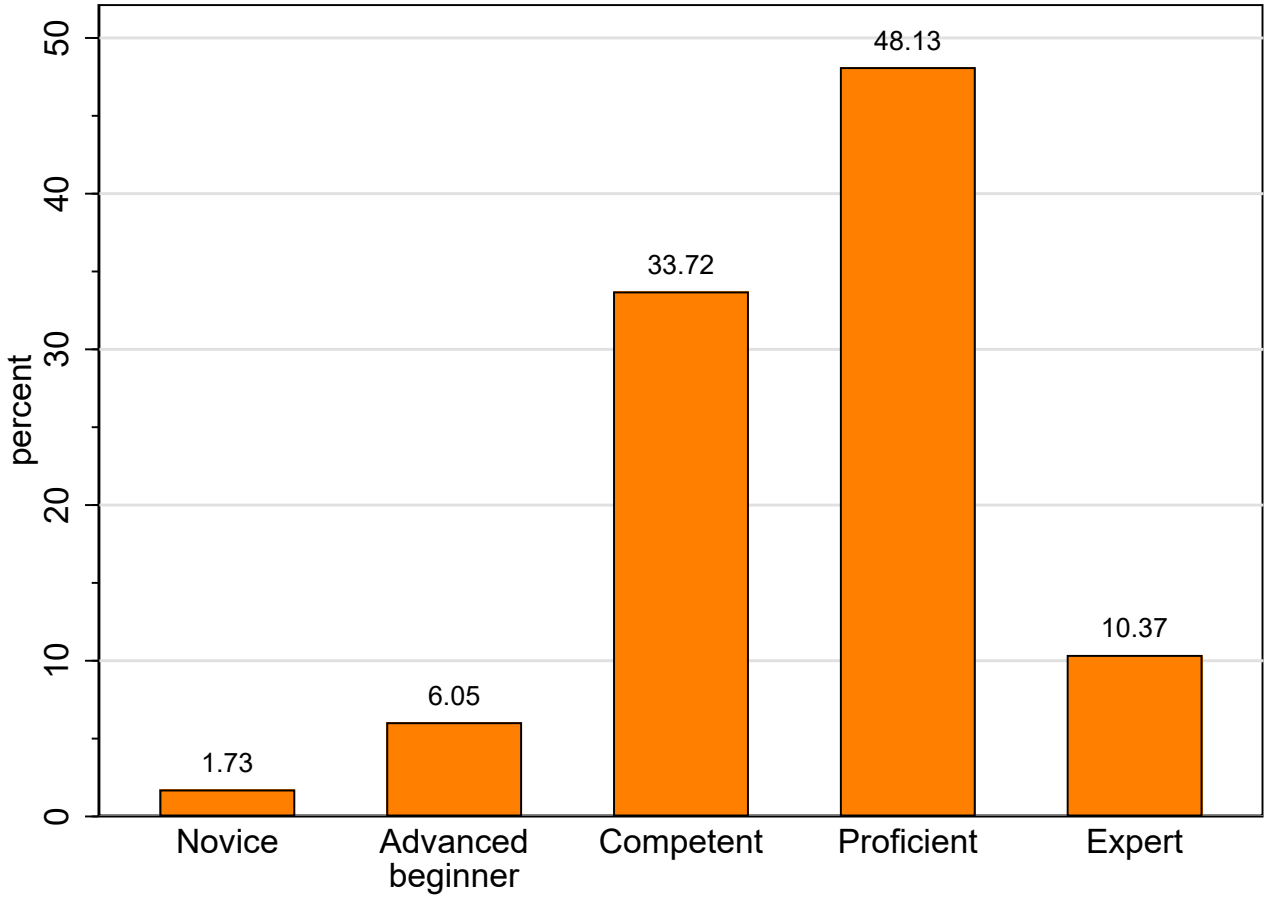
Notes: Data collected *via* survey of our replicators after completing their reports. This figure illustrates the responses to the question: "For what reasons did you select your specific paper to reproduce and/or replicate from the list of papers provided?"

Figure 9: Share of Teams Who Chose their Study Because of the Journal



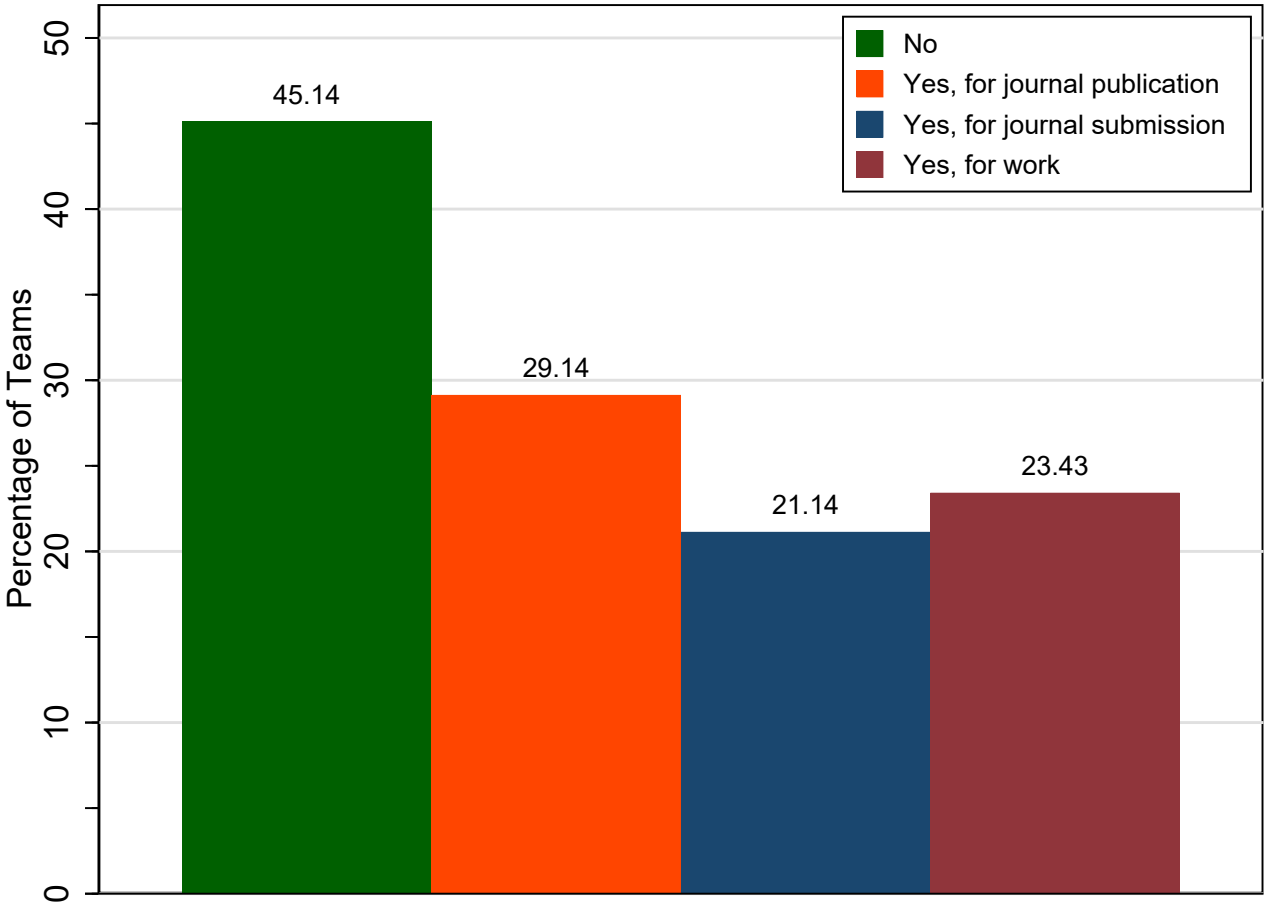
Notes: Data collected *via* survey of our replicators after completing their reports. This figure illustrates the share of teams that selected their study because of the journal for each journal separately. The data comes from the team survey's question: "For what reasons did you select your specific paper to reproduce and/or replicate from the list of papers provided?". For instance, all teams who chose to reproduce/replicate a study published in the *American Economic Review: Insights* chose it because of the journal (and potentially other reasons, found in the previous figure).

Figure 10: What is your level of expertise as a programmer?



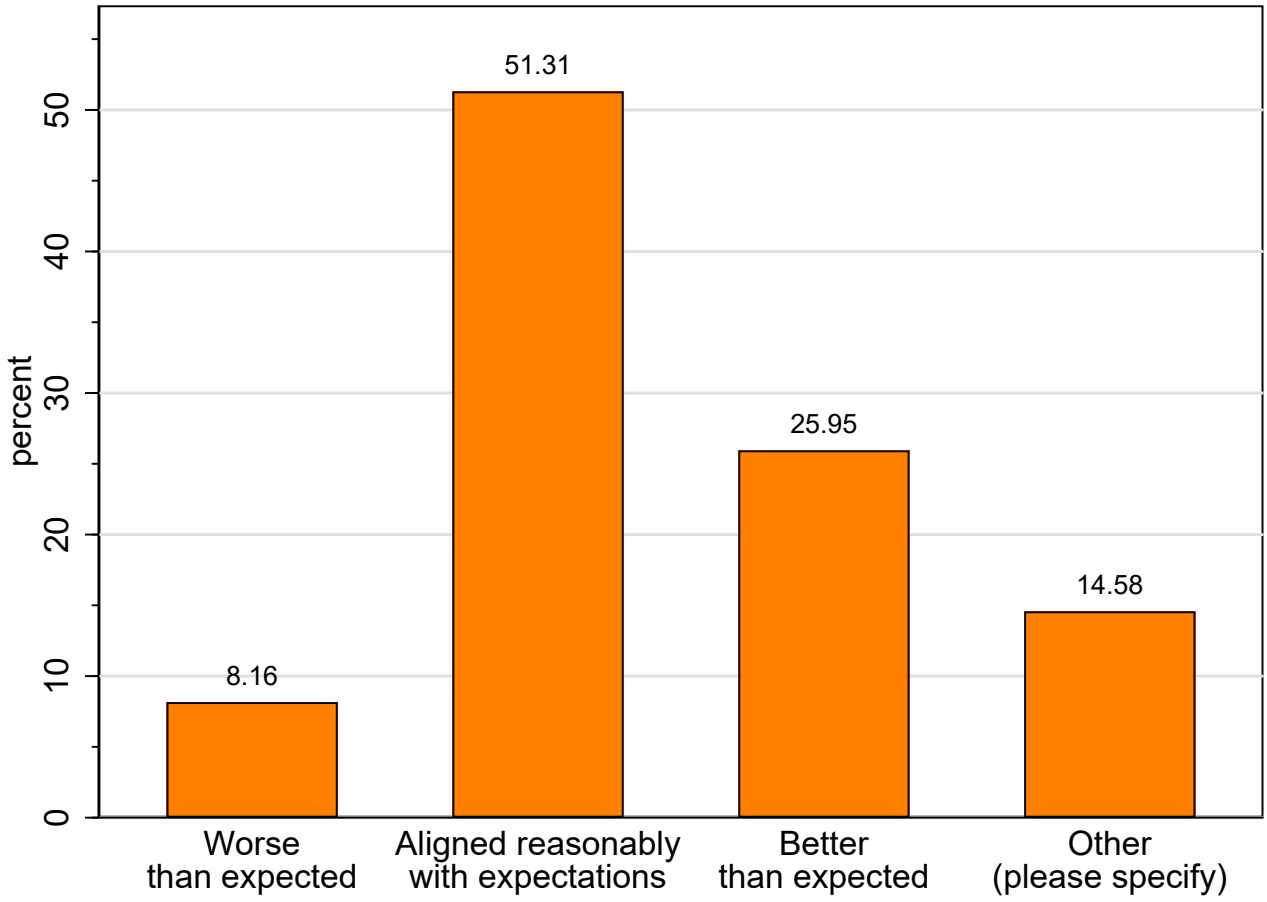
Notes: Data collected *via* survey of our replicators after completing their reports. This figure illustrates the responses to the question: "What is your level of expertise as a programmer?"

Figure 11: Have you ever had to produce a replication package? (Select all which apply)



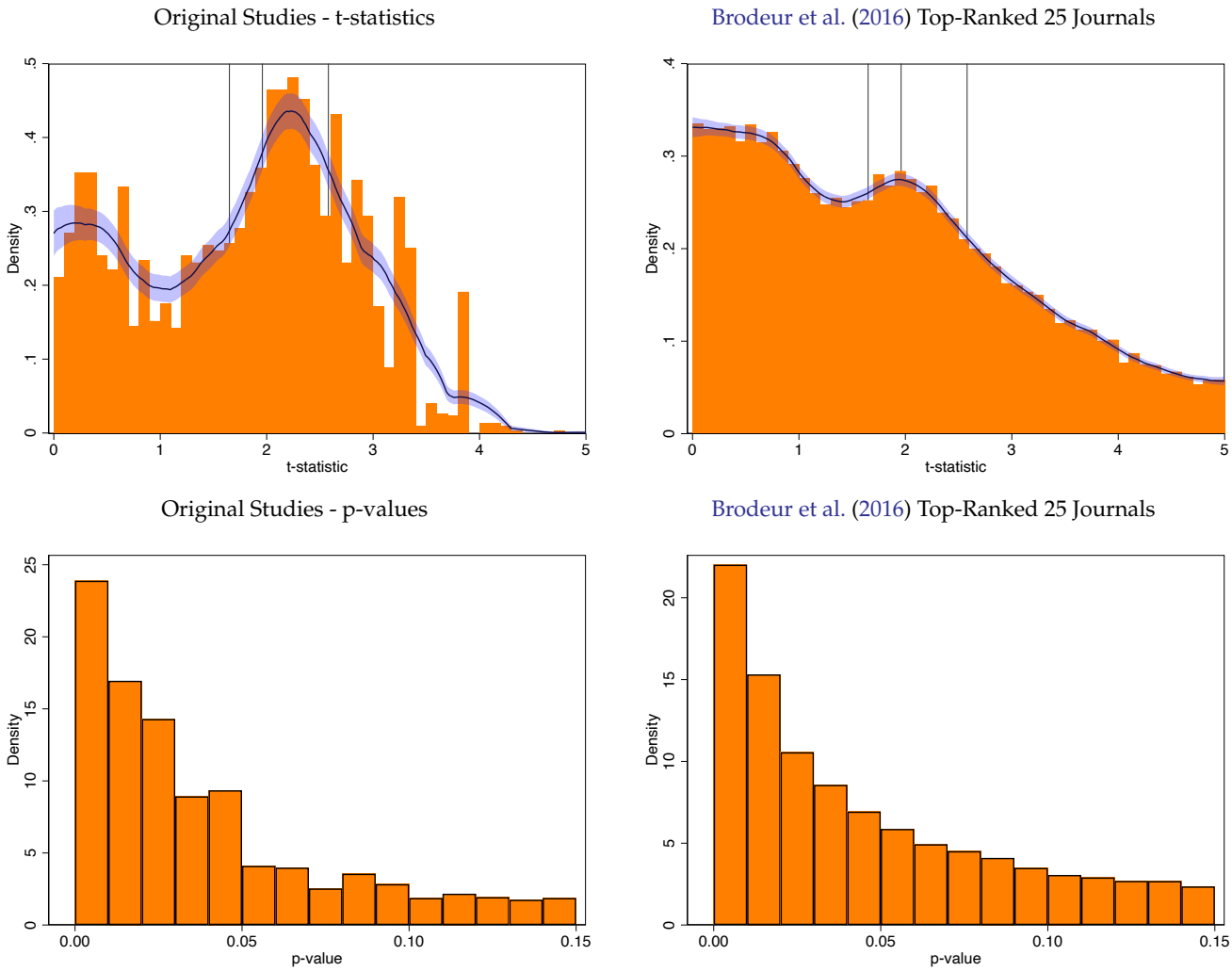
Notes: Data collected *via* survey of our replicators after completing their reports. This figure illustrates the responses to the question: "Have you ever had to produce a replication package? (Select all which apply)"

Figure 12: Which of the following best describes how the replication package aligned with your expectations?



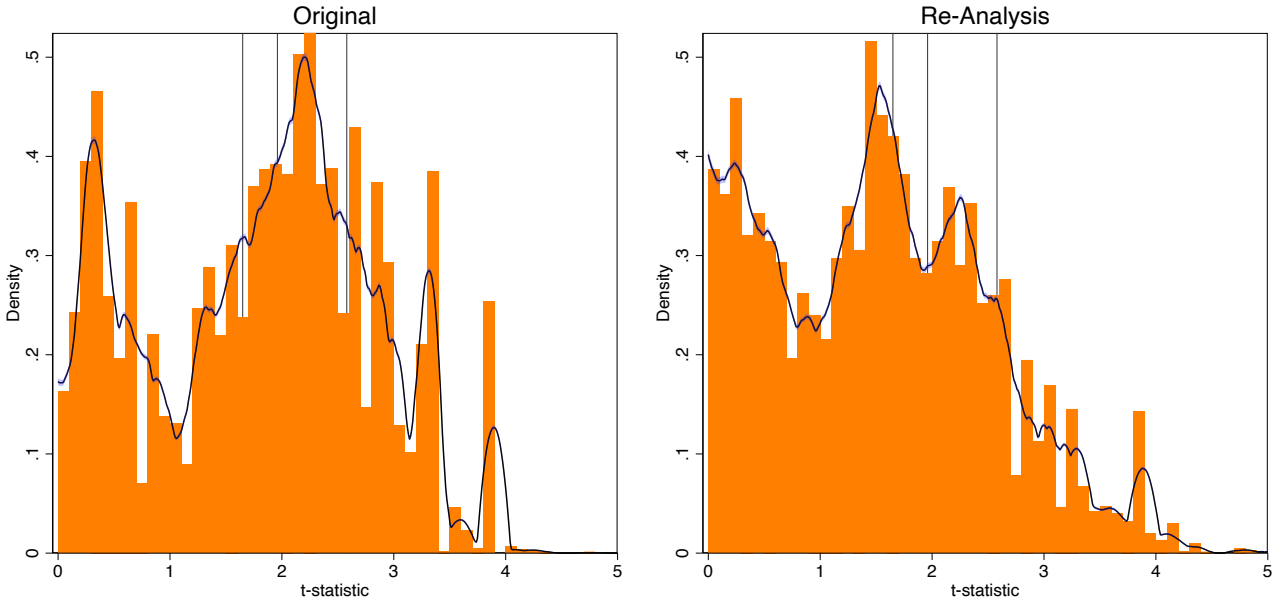
Notes: Data collected *via* survey of our replicators after completing their reports. This figure illustrates the responses to the question: "For what reasons did you select your specific paper to reproduce and/or replicate from the list of papers provided?"

Figure 13: Distributions of t-Statistics and p-Values for Original Studies and Brodeur et al. (2016)



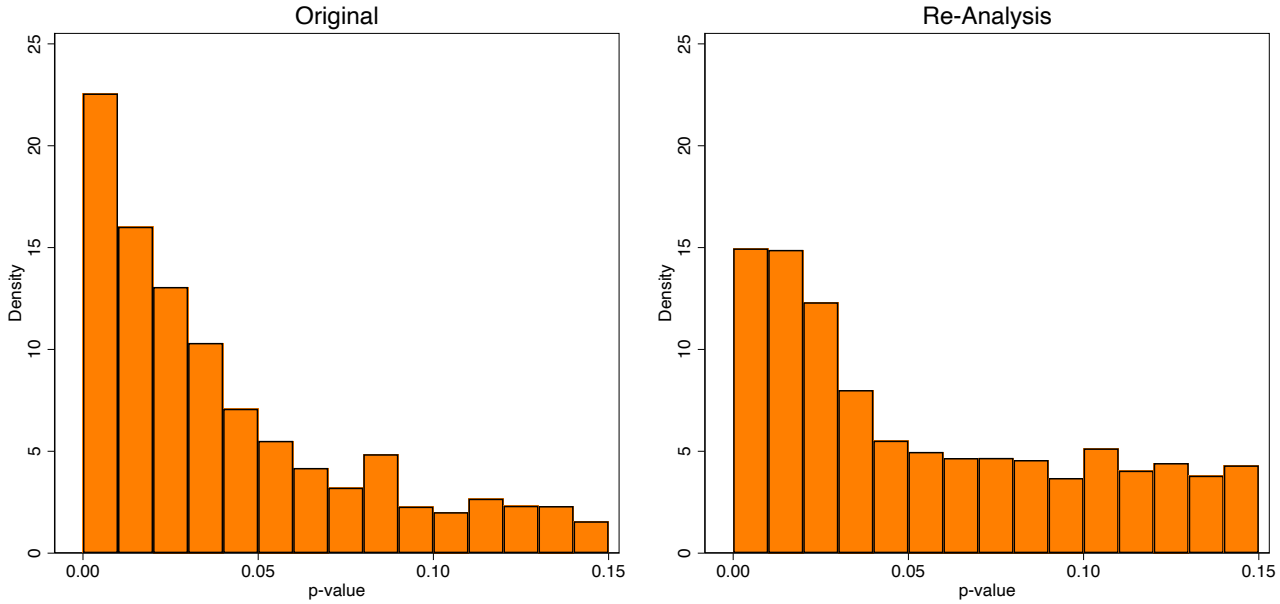
Notes: The top figures display a histogram of test statistics for $t \in [0, 5]$, with bins of width 0.1. The top left figure includes all original studies in our data set. As a comparison, the top right figure plots the corresponding histogram of z-statistics from the top-ranked 25 economics journals published in 2015 and 2018 (from Brodeur et al. (2016)). Vertical reference lines are displayed at conventional two-tailed significance levels. We superimpose an Epanechnikov kernel density curve. 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 Brodeur et al. (2016), respectively.

Figure 14: Weighted Distributions of t-Statistics for Original Studies and Re-Analyses



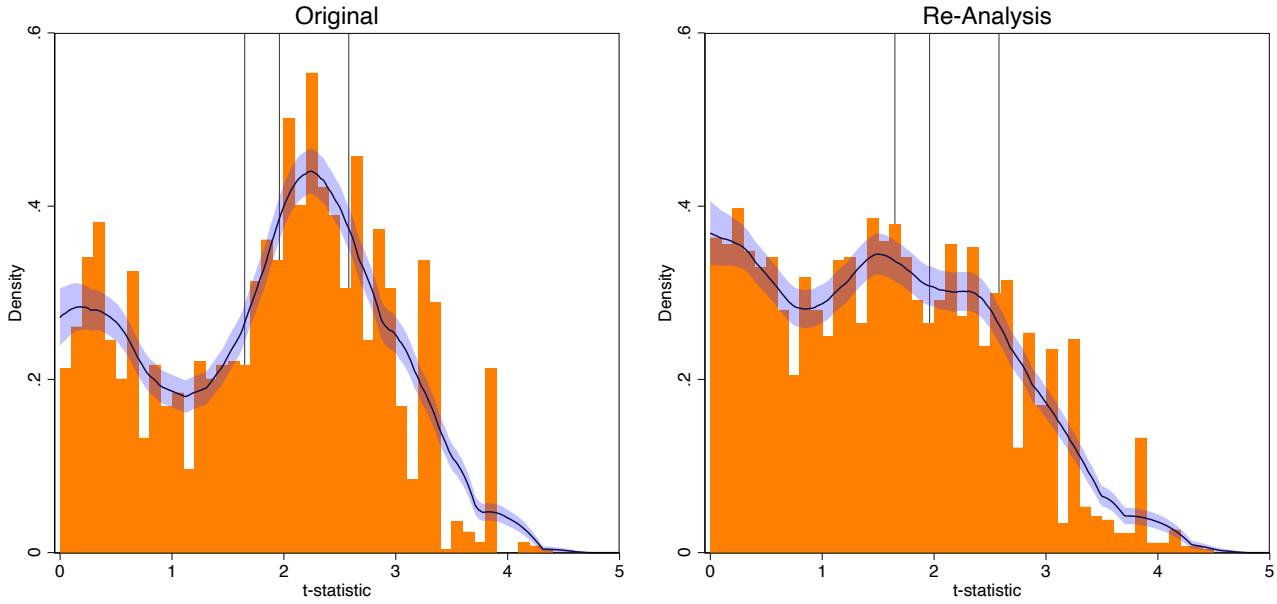
Notes: The figures display histograms of test statistics for $t \in [0, 5]$, with bins of width 0.1, among original studies and re-analyses, respectively. Vertical reference lines are displayed at conventional two-tailed significance levels. We superimpose an Epanechnikov kernel density curve. We use the inverse of the number of tests presented in the same article to weight observations.

Figure 15: Weighted Distributions of p-values for Original Studies and Re-Analyses



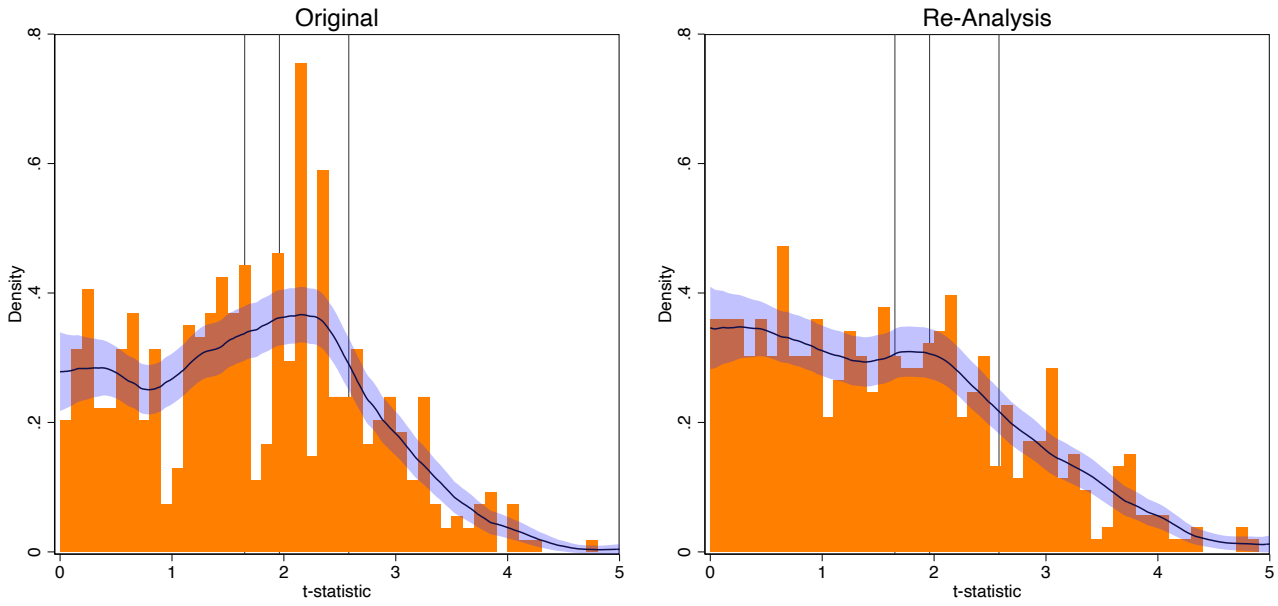
Notes: The figures display histograms of test statistics for p-values $\in [0.0025, 0.1500]$, with bins of width 0.0025, among original studies and re-analyses, respectively. We use the inverse of the number of tests presented in the same article to weight observations.

Figure 16: Economics: Distributions of t-Statistics for Original Studies and Re-Analyses



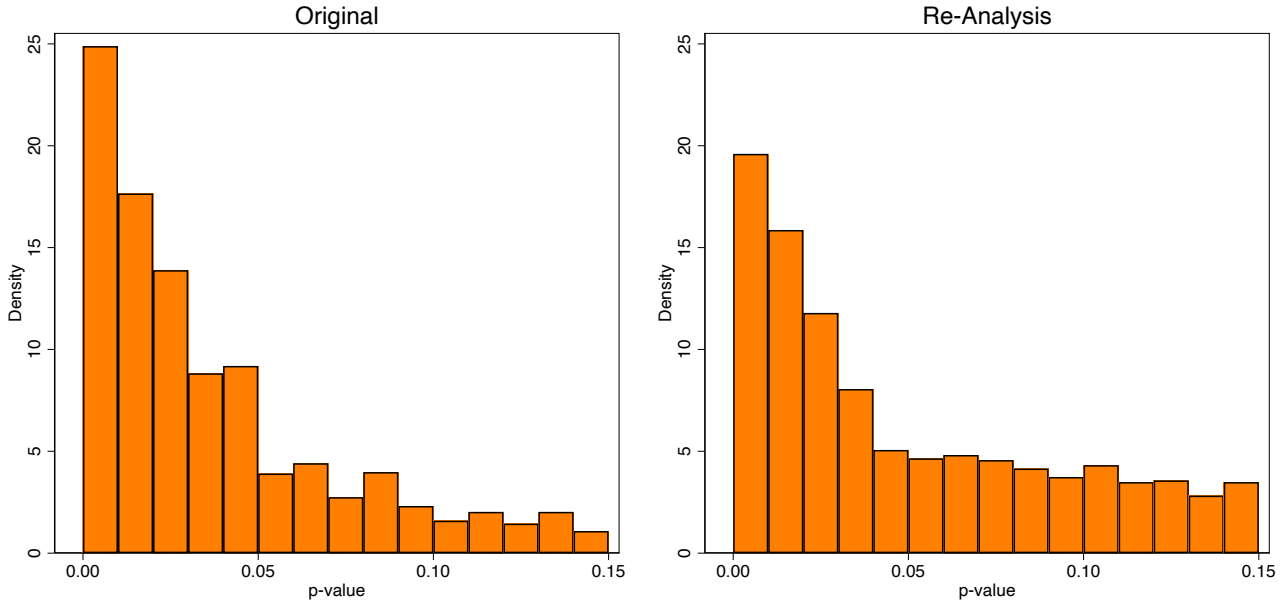
Notes: We restrict the sample to articles published in economic journals. The figures display histograms of test statistics for $t \in [0, 5]$, with bins of width 0.1, among original studies and re-analyses, respectively. Vertical reference lines are displayed at conventional two-tailed significance levels. We superimpose an Epanechnikov kernel density curve.

Figure 17: Political Science: Distributions of t-Statistics for Original Studies and Re-Analyses



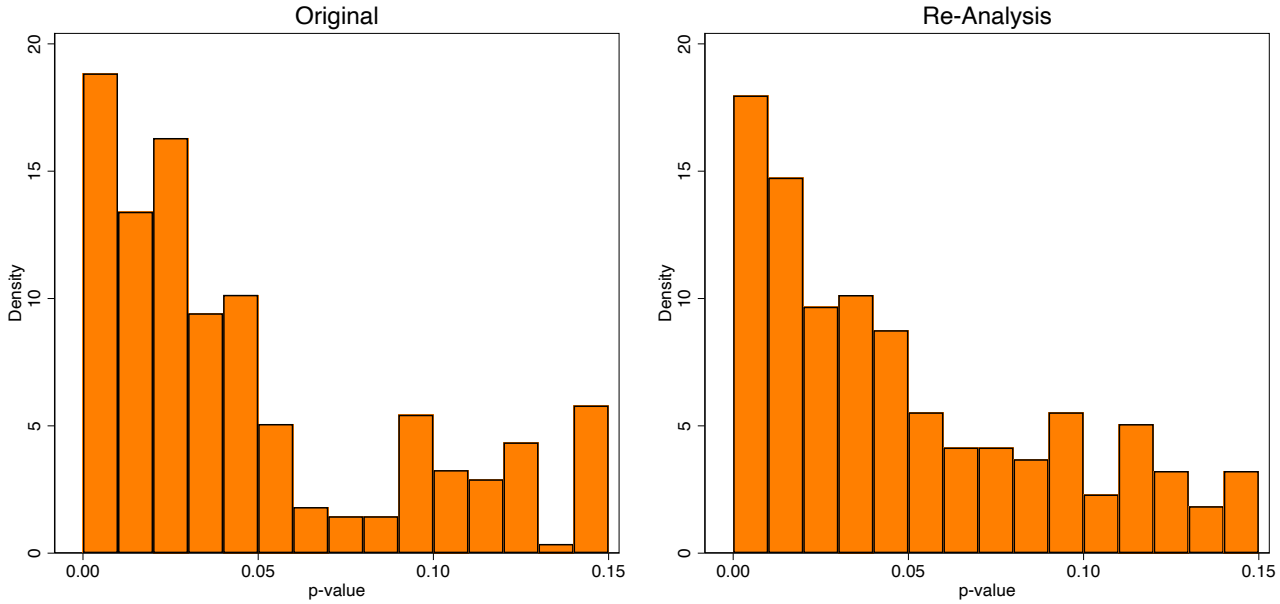
Notes: We restrict the sample to articles published in political science journals. The figures display histograms of test statistics for $t \in [0, 5]$, with bins of width 0.1, among original studies and re-analyses, respectively. Vertical reference lines are displayed at conventional two-tailed significance levels. We superimpose an Epanechnikov kernel density curve.

Figure 18: Economics: Distributions of p-values for Original Studies and Re-Analyses



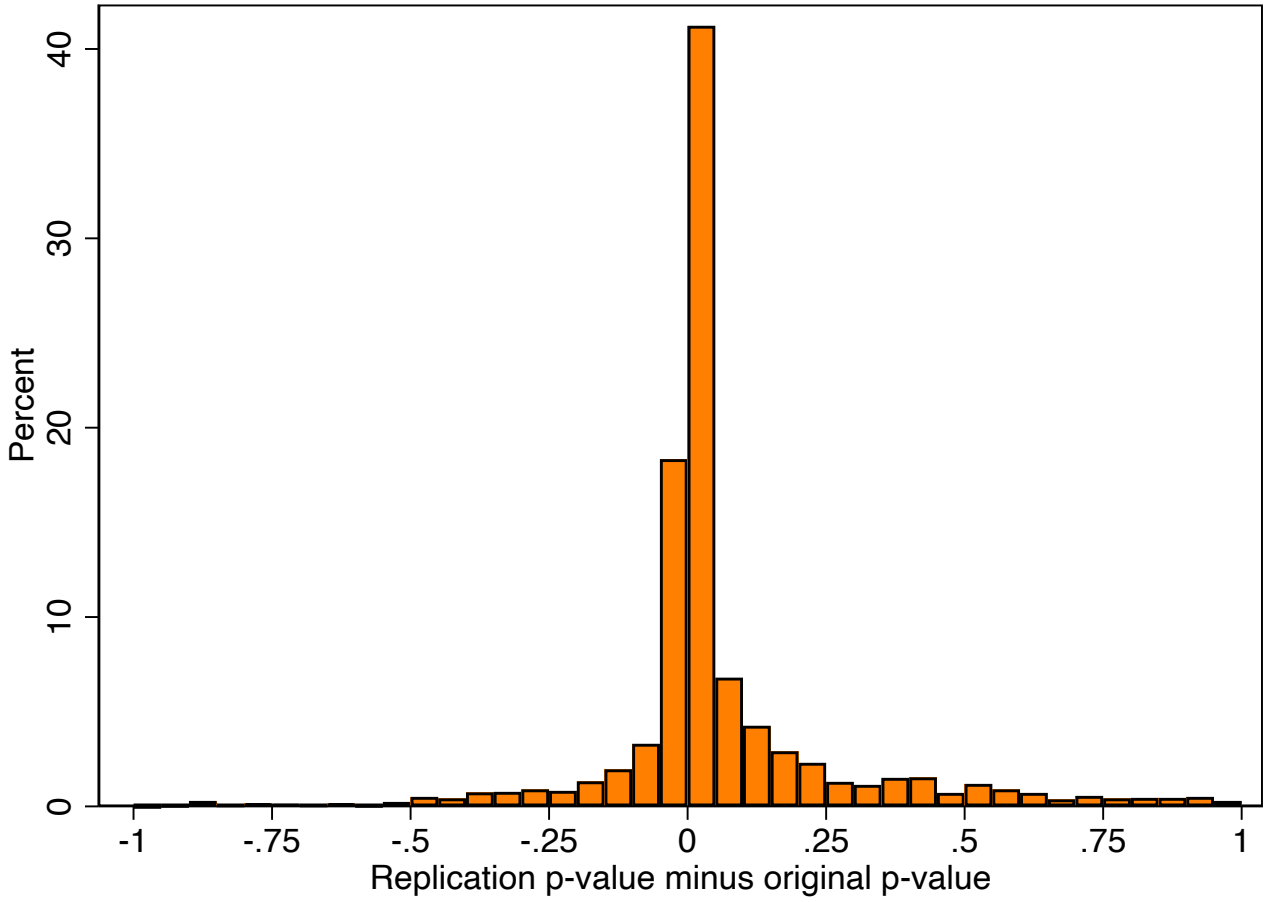
Notes: We restrict the sample to articles published in economic journals. The figures display histograms of test statistics for p-values $\in [0.0025, 0.1500]$, with bins of width 0.0025, among original studies and re-analyses, respectively. We use the inverse of the number of tests presented in the same article to weight observations.

Figure 19: Political Science: Distributions of p-values for Original Studies and Re-Analyses



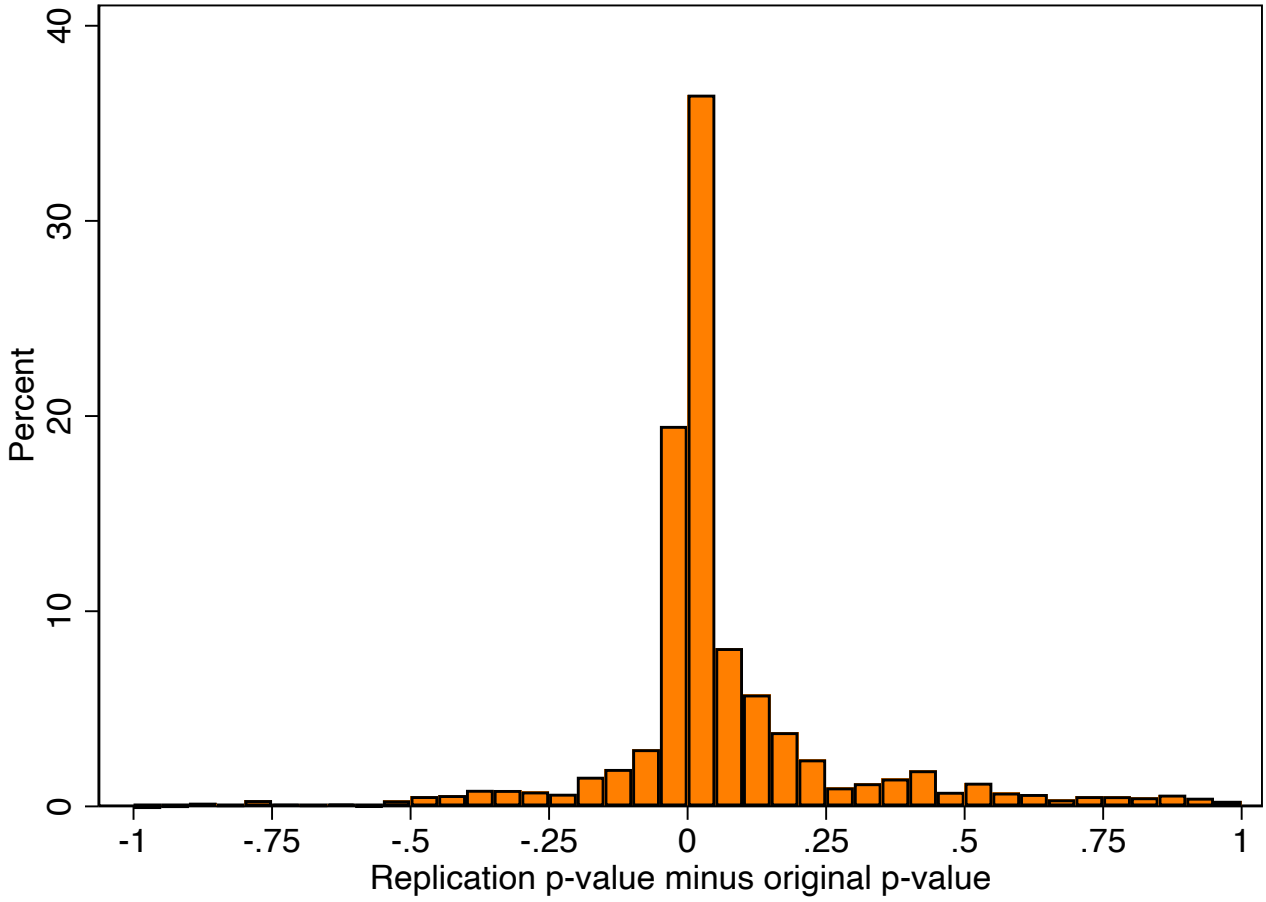
Notes: We restrict the sample to articles published in political science journals. The figures display histograms of test statistics for p-values $\in [0.0025, 0.1500]$, with bins of width 0.0025, among original studies and re-analyses, respectively. We use the inverse of the number of tests presented in the same article to weight observations.

Figure 20: Distribution of $p_{\text{replication}} - p_{\text{original}}$



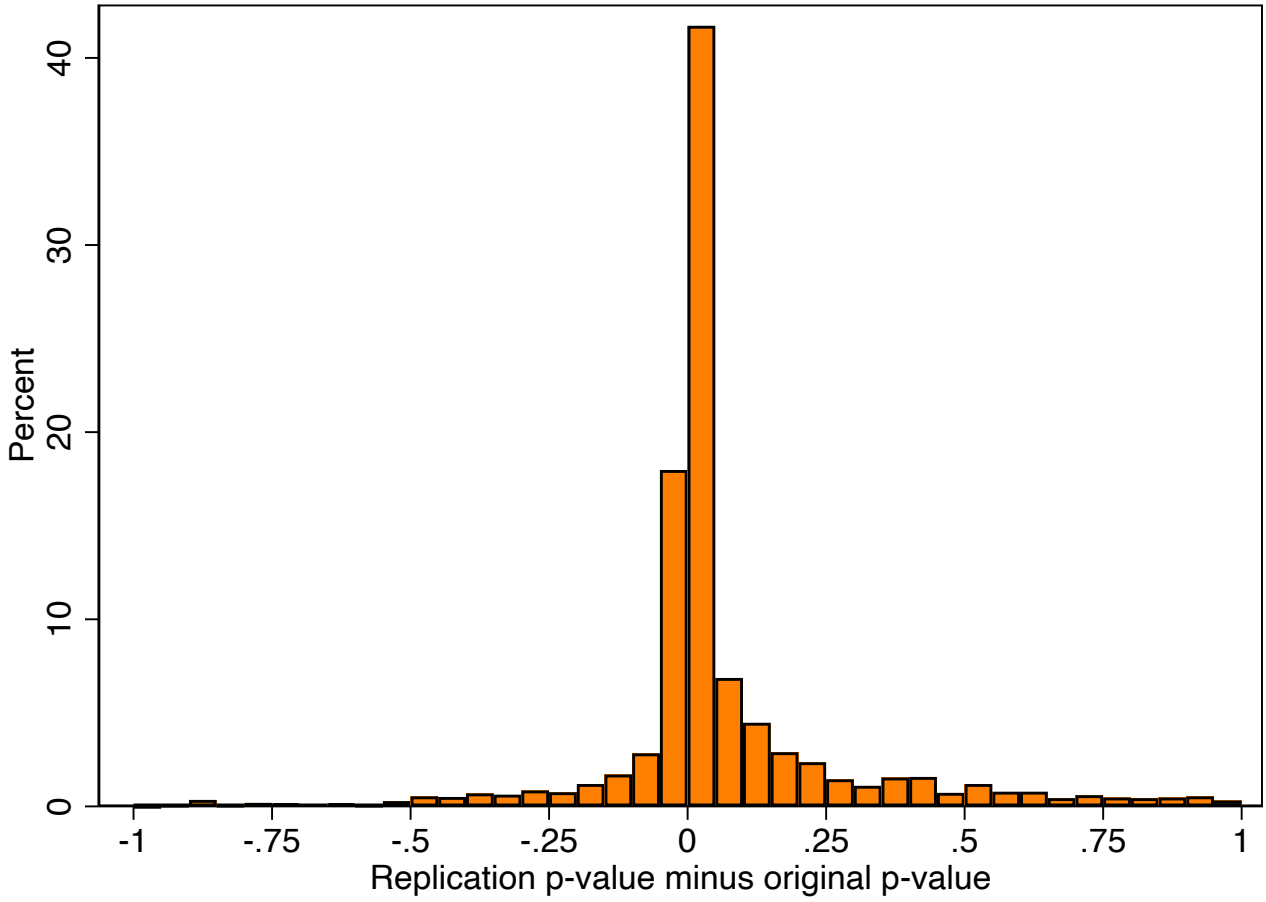
Notes: This figure presents the distribution of ($p_{\text{replication}} - p_{\text{original}}$).

Figure 21: Distribution of $p_{\text{replication}} - p_{\text{original}}$ (weighted)



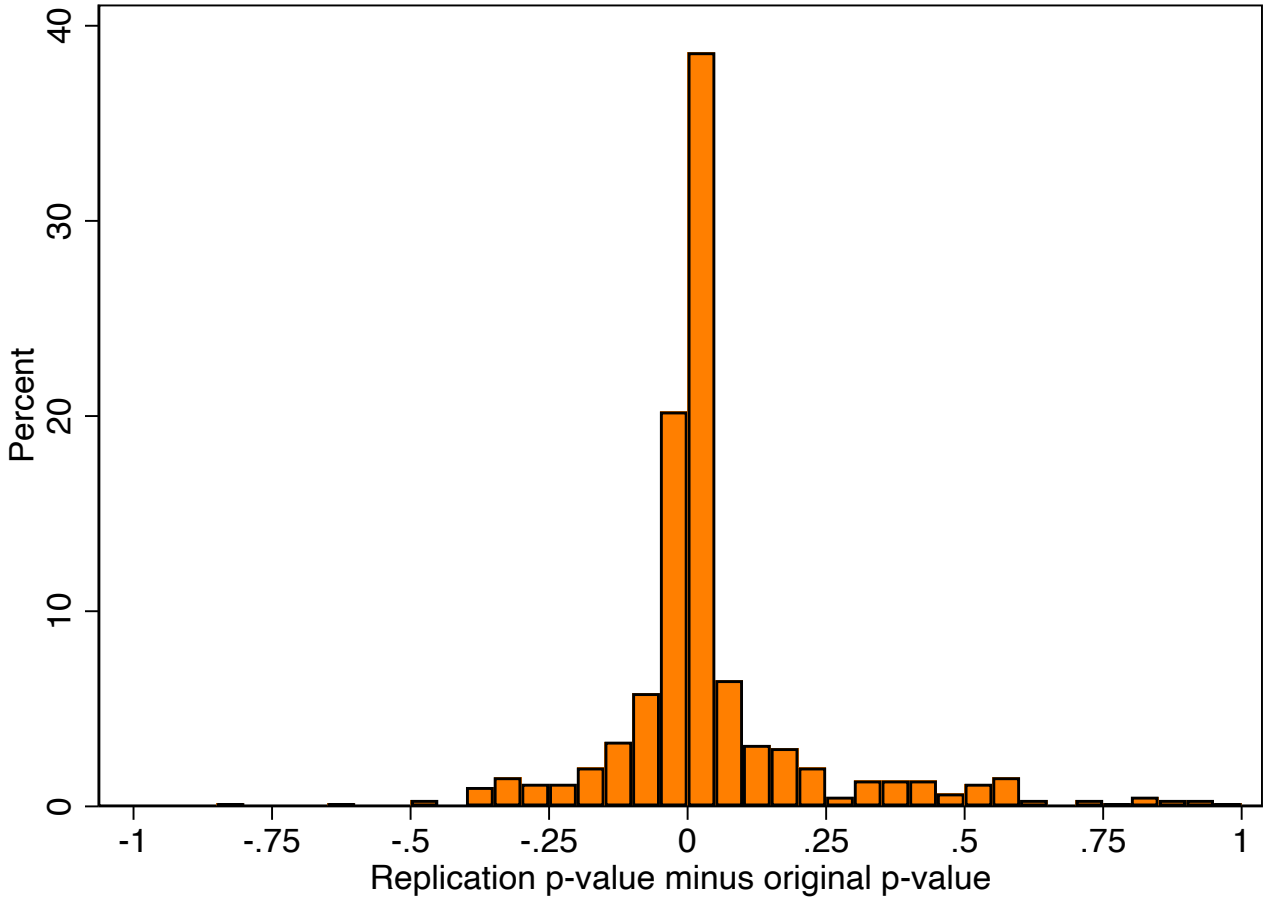
Notes: This figure presents the distribution of ($p_{\text{replication}} - p_{\text{original}}$) while applying article weights.

Figure 22: Distribution of $p_{\text{replication}} - p_{\text{original}}$ in Economics



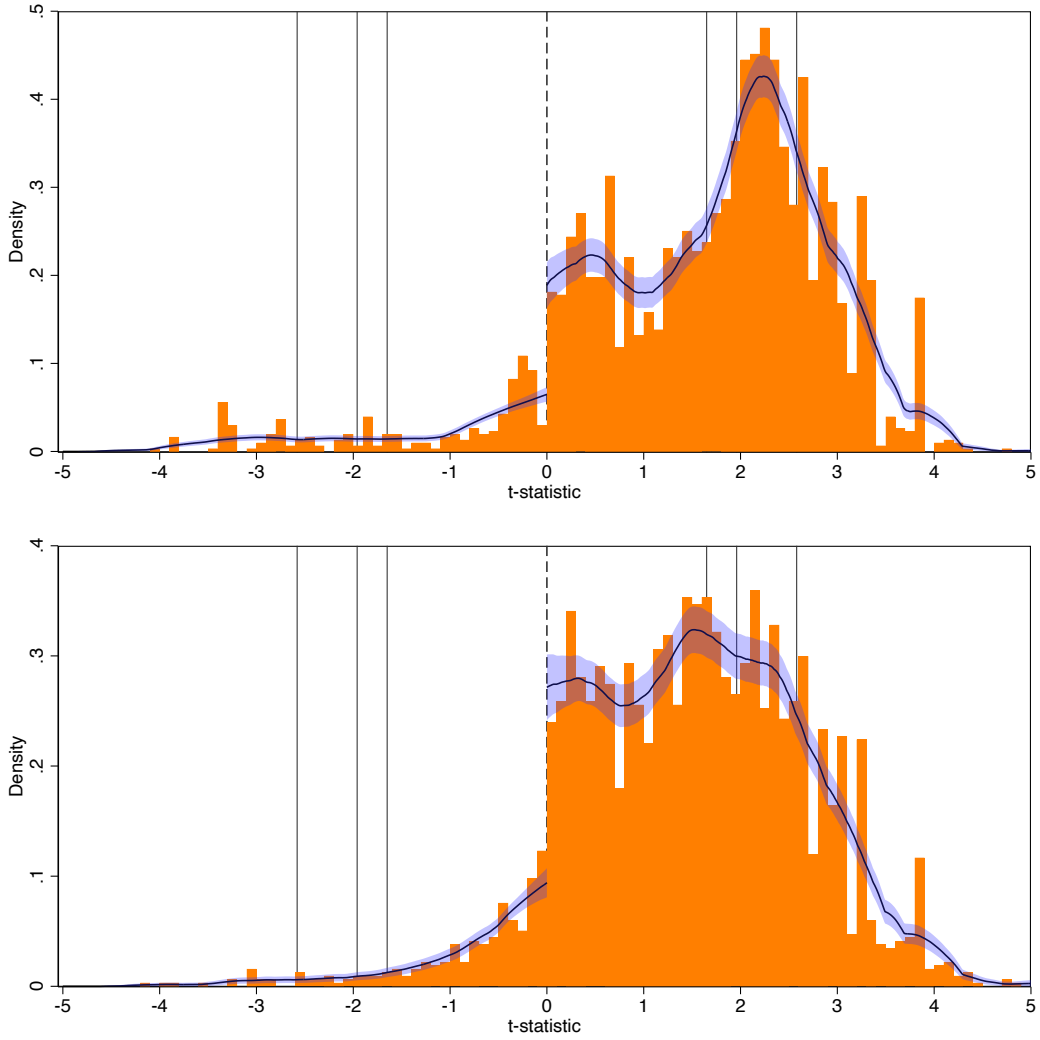
Notes: This figure presents the distribution of ($p_{\text{replication}} - p_{\text{original}}$) for economics.

Figure 23: Distribution of $p_{\text{replication}} - p_{\text{original}}$ in Political Science



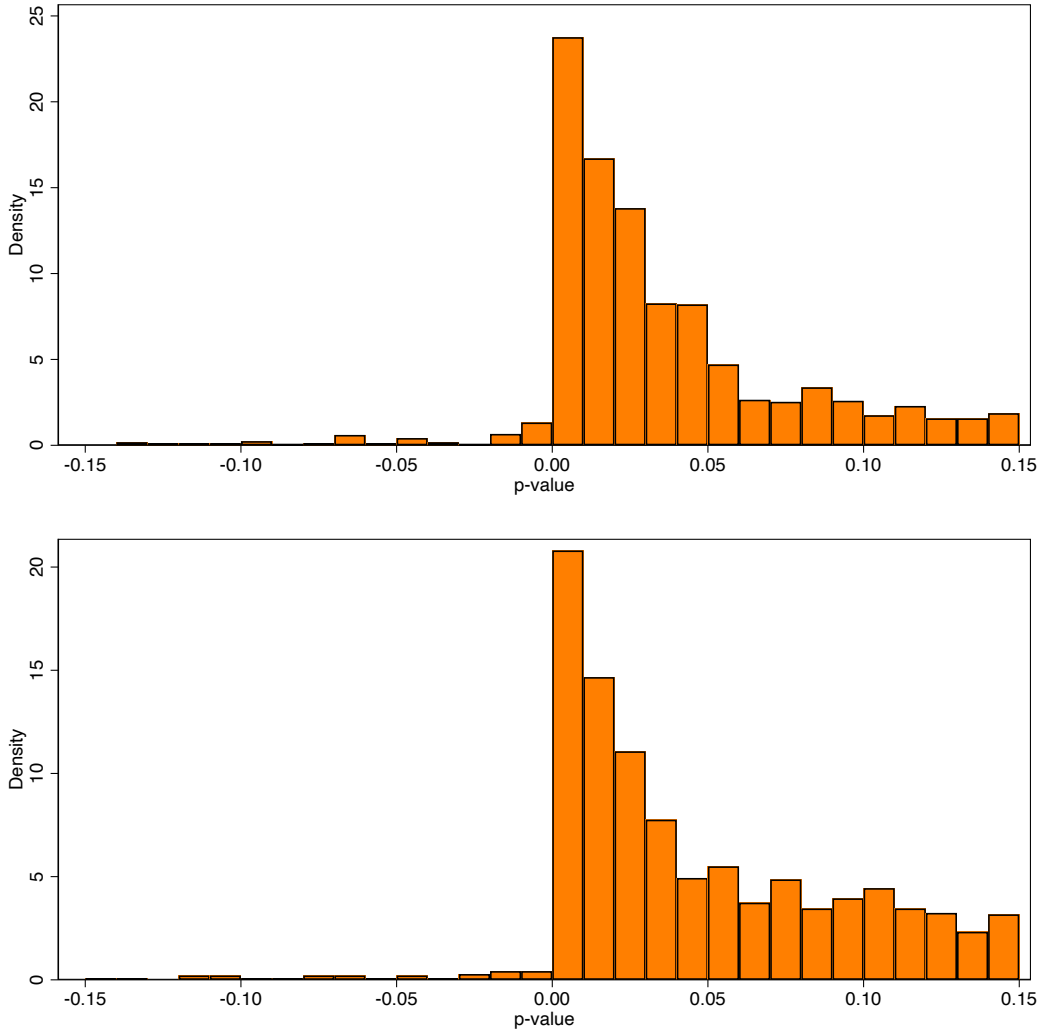
Notes: This figure presents the distribution of $(p_{\text{replication}} - p_{\text{original}})$ for political science.

Figure 24: t-curves, where negative represents a sign change from original to replicator



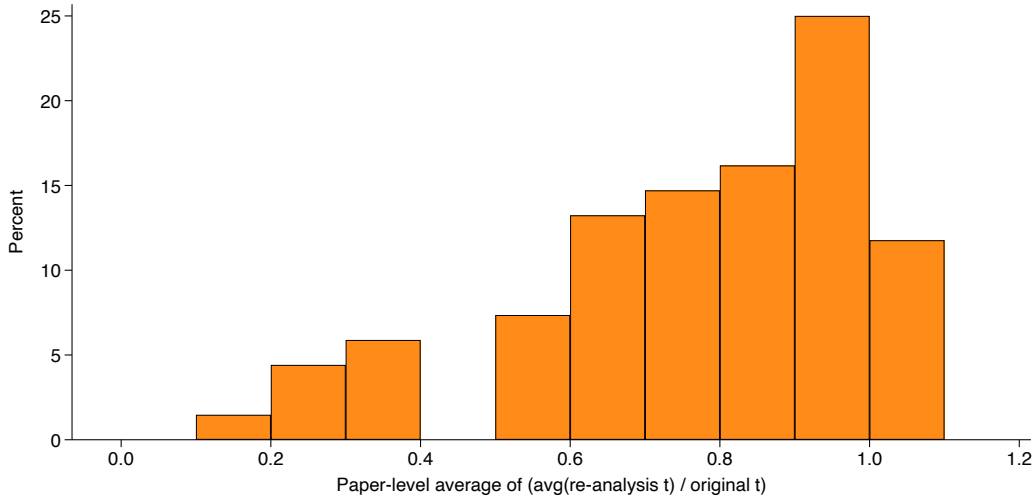
Notes: Both panels display a histogram of test statistics for $t \in [0, 5]$, with bins of width 0.1. If the replicator's estimated effect was of the opposite sign than the originally published estimate, we set the sign of the t-statistic to be negative. The top panel displays the t-statistics associated with the originally published estimates. The bottom panel displays the t-statistics associated with the replicators' estimates. We have added a dashed reference line at $t = 0$, demarcating the areas where the replicators' and original estimates agree in sign. For both sides of the zero line, vertical reference lines are displayed at conventional two-tailed significance levels. We superimpose an Epanechnikov kernel density curve, separately estimated for the positive and negative masses.

Figure 25: p-curves, where negative represents a sign change from original to replicator



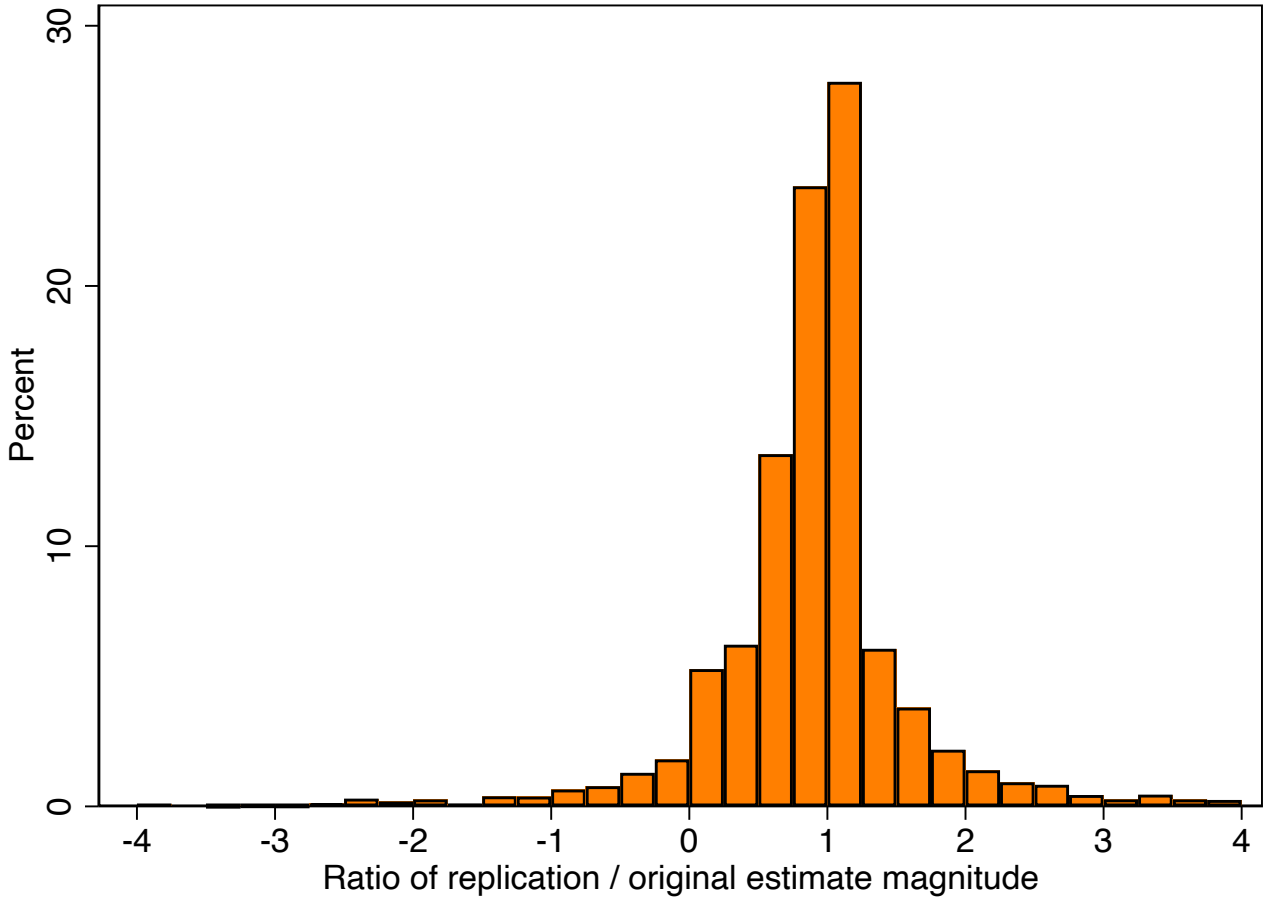
Notes: Both panels display a histogram of test statistics for $p \in [0.00, 0.15]$, with bins of width 0.01. If the replicator's estimated effect was of the opposite sign than the originally published estimate, we set the sign of the p-value to be negative. The top panel displays the p-values associated with the originally published estimates. The bottom panel displays the p-values associated with the replicators' estimates. We have added a dashed reference line at $p = 0$, demarcating the areas where the replicators' and original estimates agree (right) and disagree (left) in sign.

Figure 26: Relative t-statistics



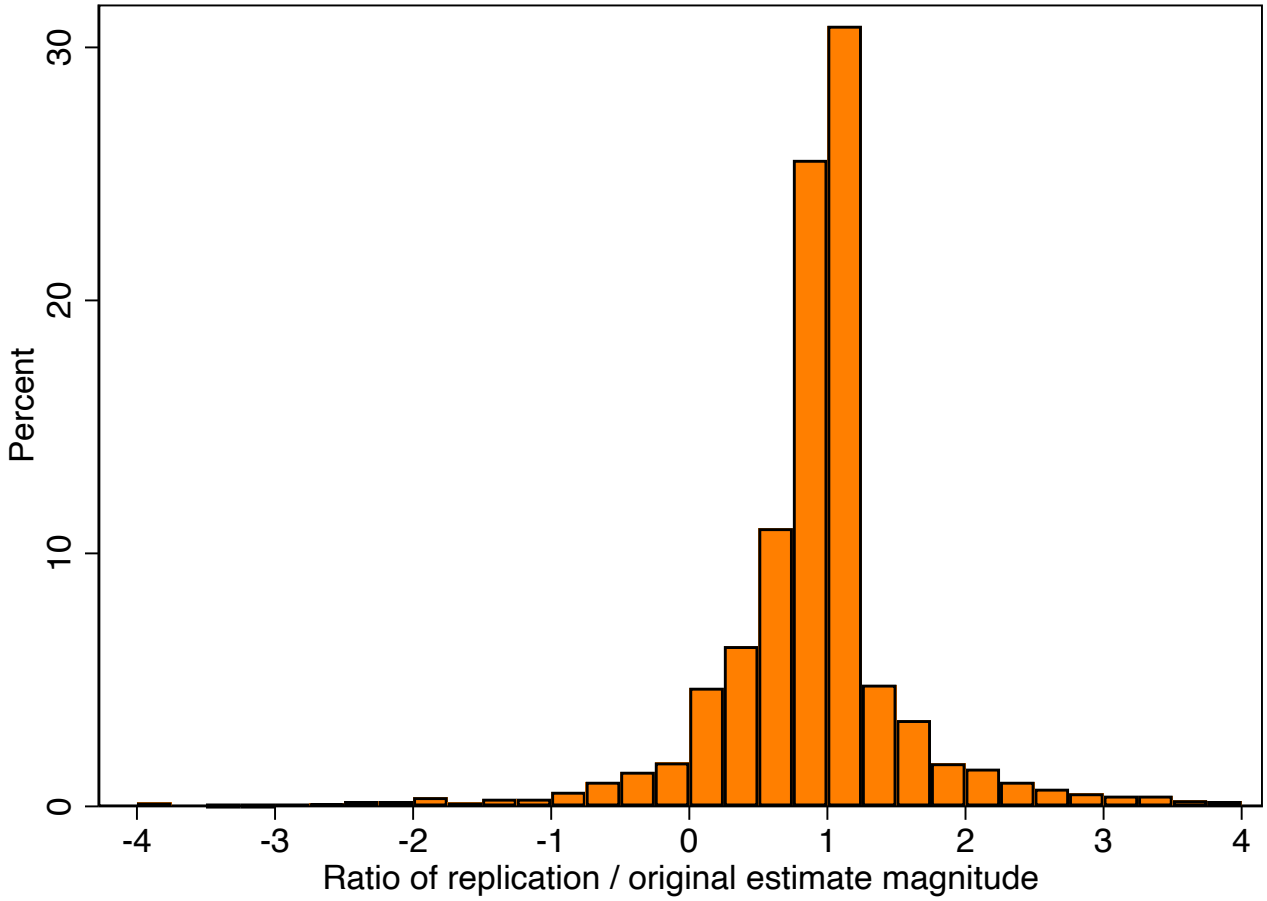
Notes: The data contains multiple reanalysis t-statistics for every original result. We first take the average of the re-analysis t-statistics by original result (if the reanalysis and original coefficients were of opposite sign, we assign the original to be positive and the reanalysis to be negative, otherwise everything is in absolute terms). We then take this average and divide by the original result . These values are then averaged at the paper level to get a paper's relative t-statistic when replicated. The average ratio of reanalysis to original statistical significance is 77% and median is 83%.

Figure 27: Relative Effect Size: Weighted



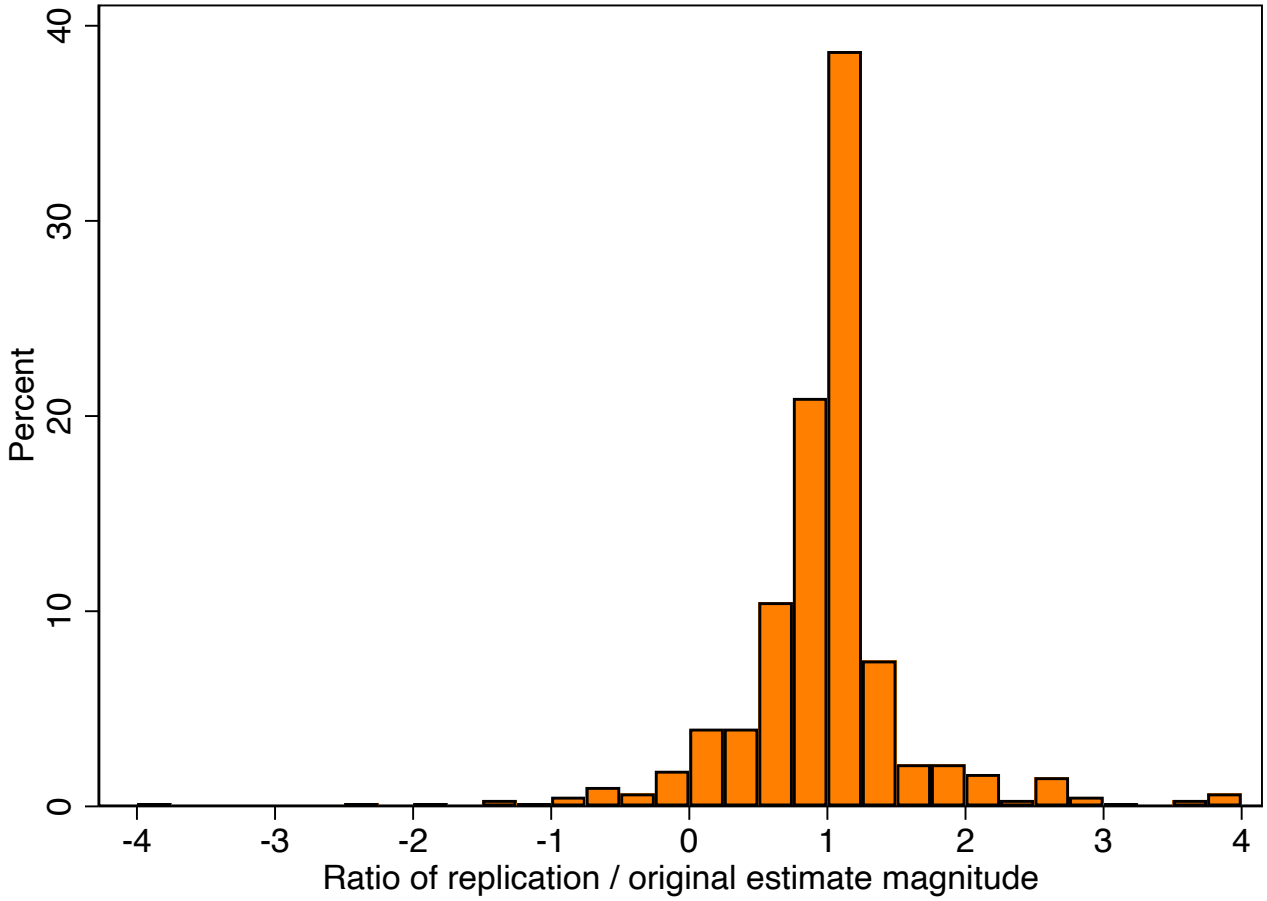
Notes: This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. A positive value indicates that the re-analysis estimate is in the same direction as in the original study. A 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. We use the inverse of the number of test statistics in each replication report to weight observations.

Figure 28: Economics: Relative Effect Size



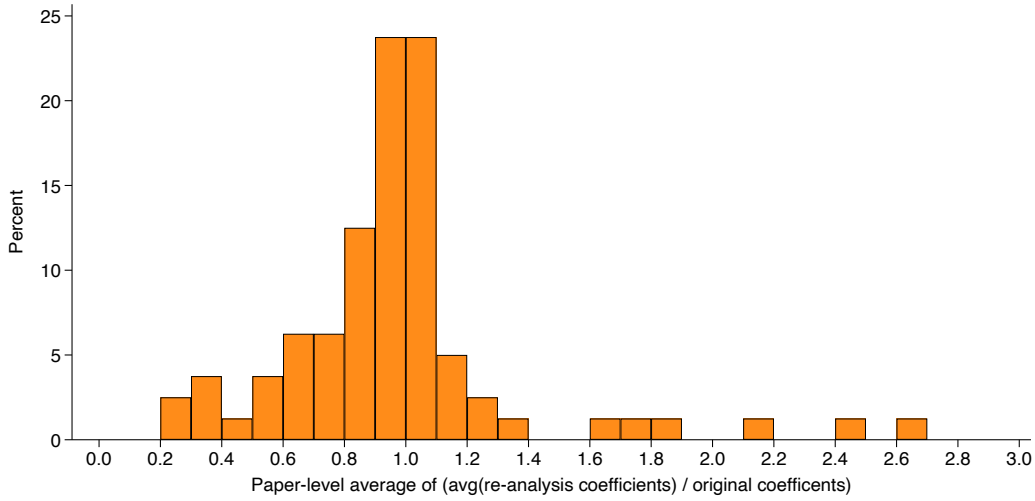
Notes: The sample is restricted to original articles published in economic journals. This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. A positive value indicates that 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.

Figure 29: Political Science: Relative Effect Size



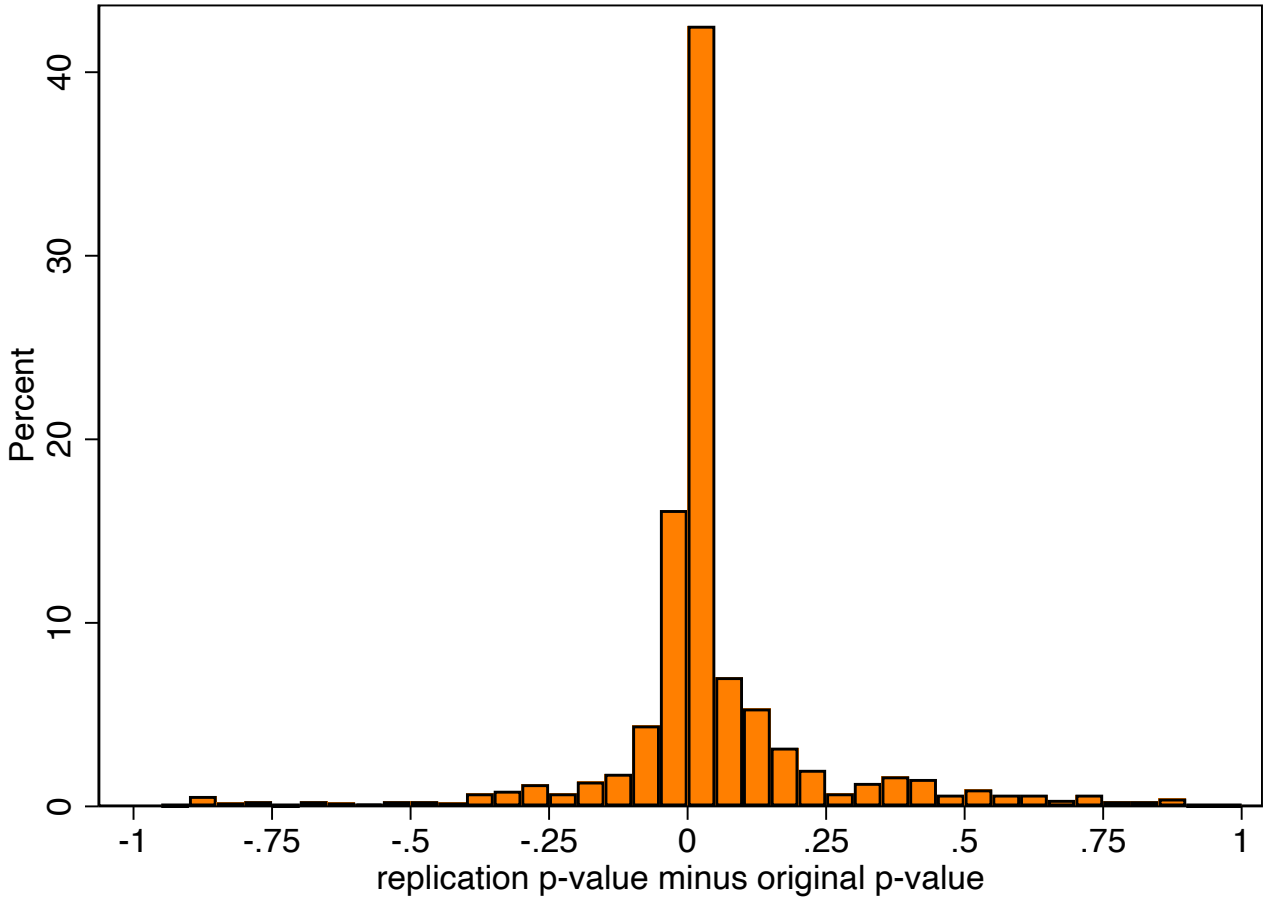
Notes: The sample is restricted to original articles published in political science journals. This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. A positive value indicates 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.

Figure 30: Relative effect size at the paper level



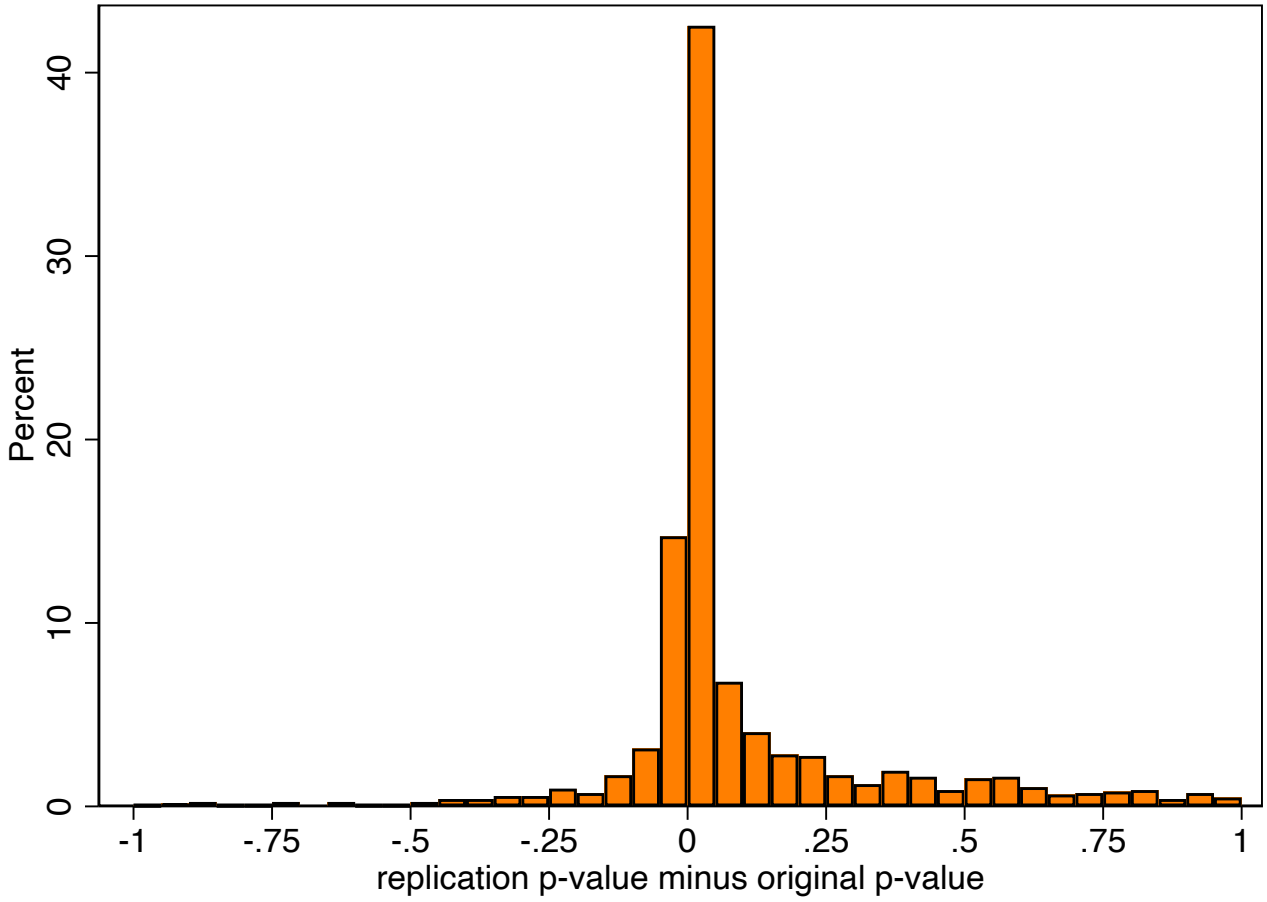
Notes: The data contains multiple reanalysis effect estimates for every original result. We first take the average of the re-analysis effect estimates by original result (if the reanalysis and original coefficients were of opposite sign, we assign the original to be positive and the reanalysis to be negative, otherwise everything is in absolute terms). We then take this average and divide by the original result. These values are then averaged at the paper level to get a paper's average relative effect size when replicated. The average ratio of reanalysis to original statistical significance is 97.7%.

Figure 31: Alternative Control Variables: Distribution of P-Values



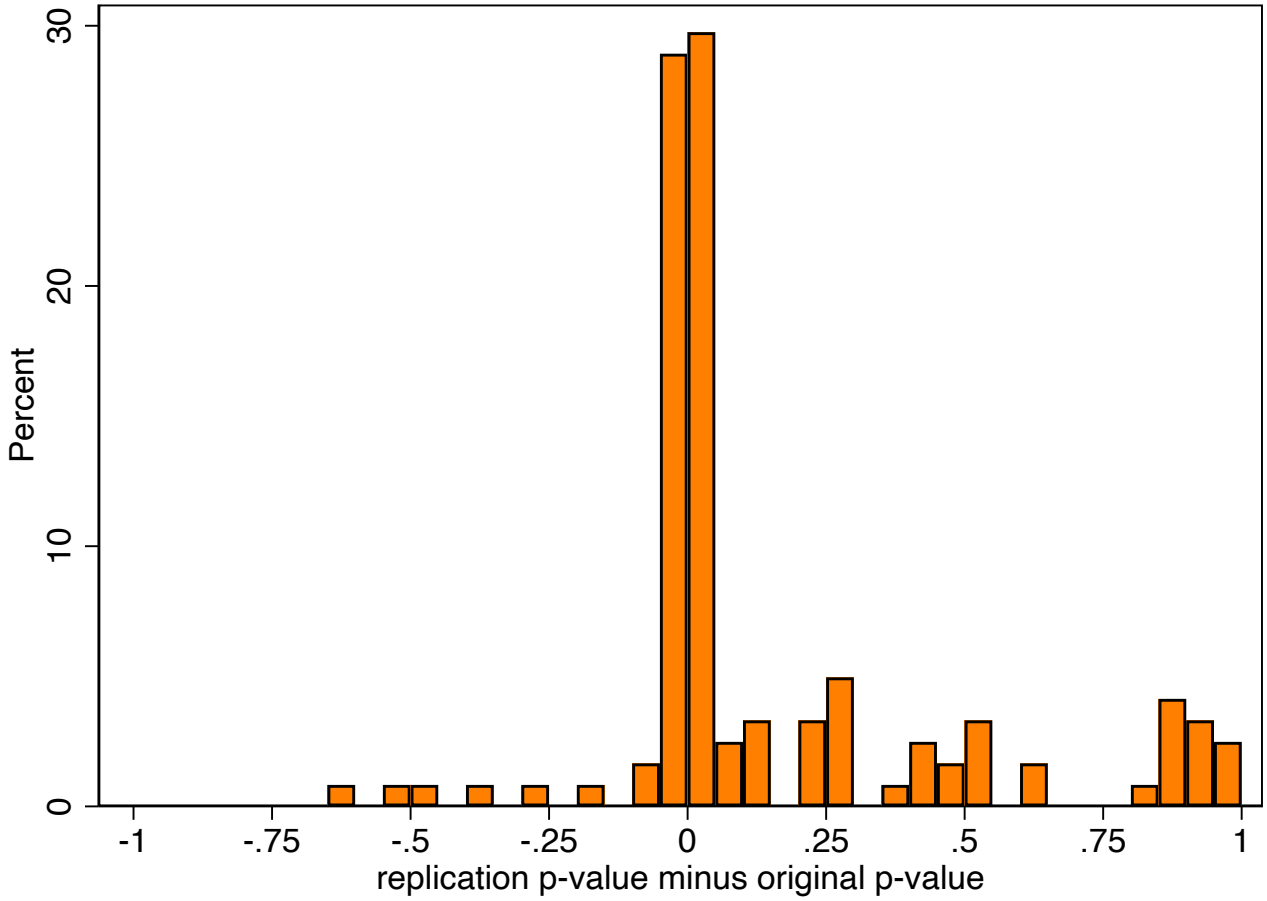
Notes: The sample is restricted to re-analyses for which an alternative control variables is made by the replicators. This figure illustrates the difference in p-values of the robustness reproduction/replication and original estimates.

Figure 32: Changing Sample: Distribution of P-Values



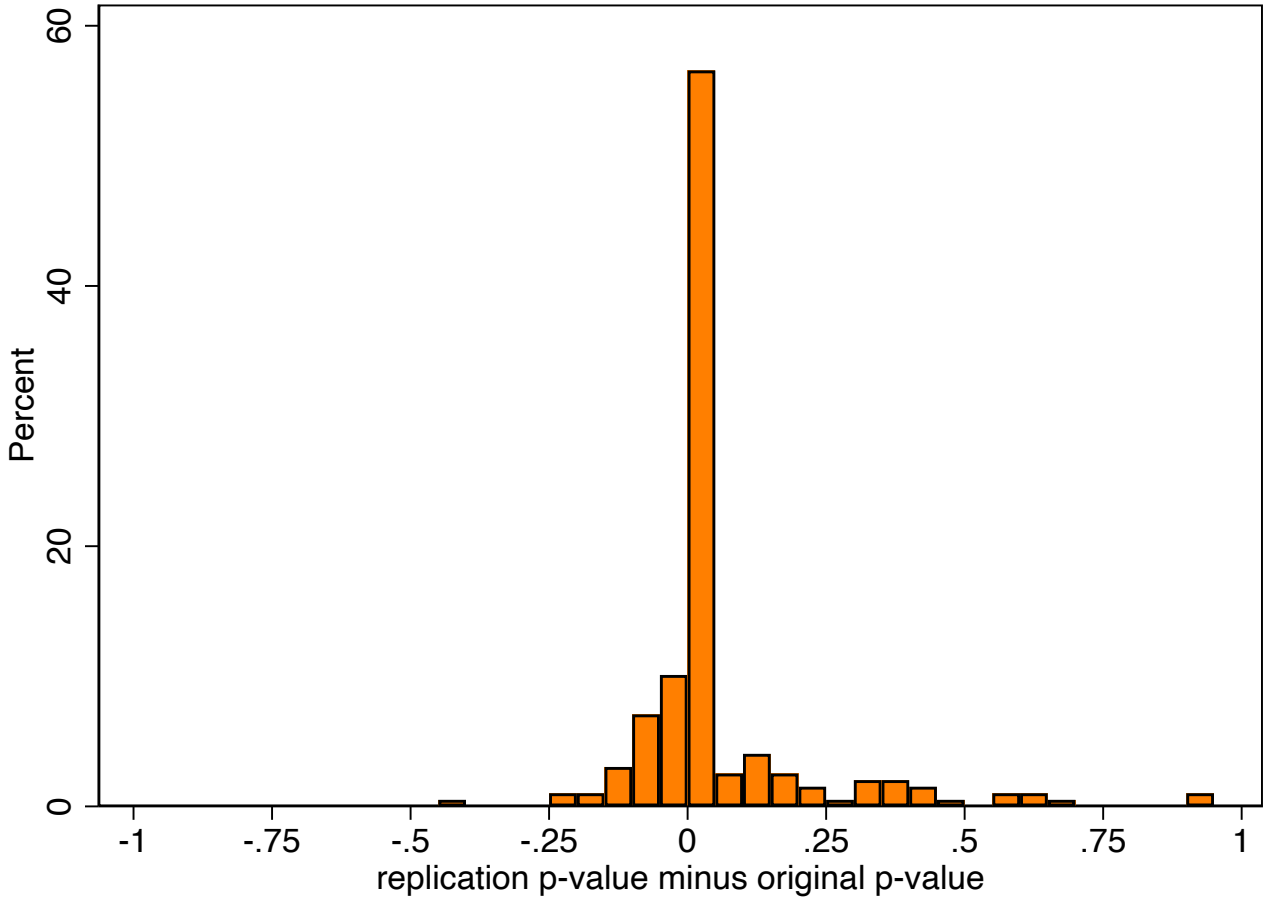
Notes: The sample is restricted to re-analyses for which the replicators changed the sample. This figure illustrates the difference in p-values of the robustness reproduction/replication and original estimates.

Figure 33: Changing Dependent Variable: Distribution of P-Values



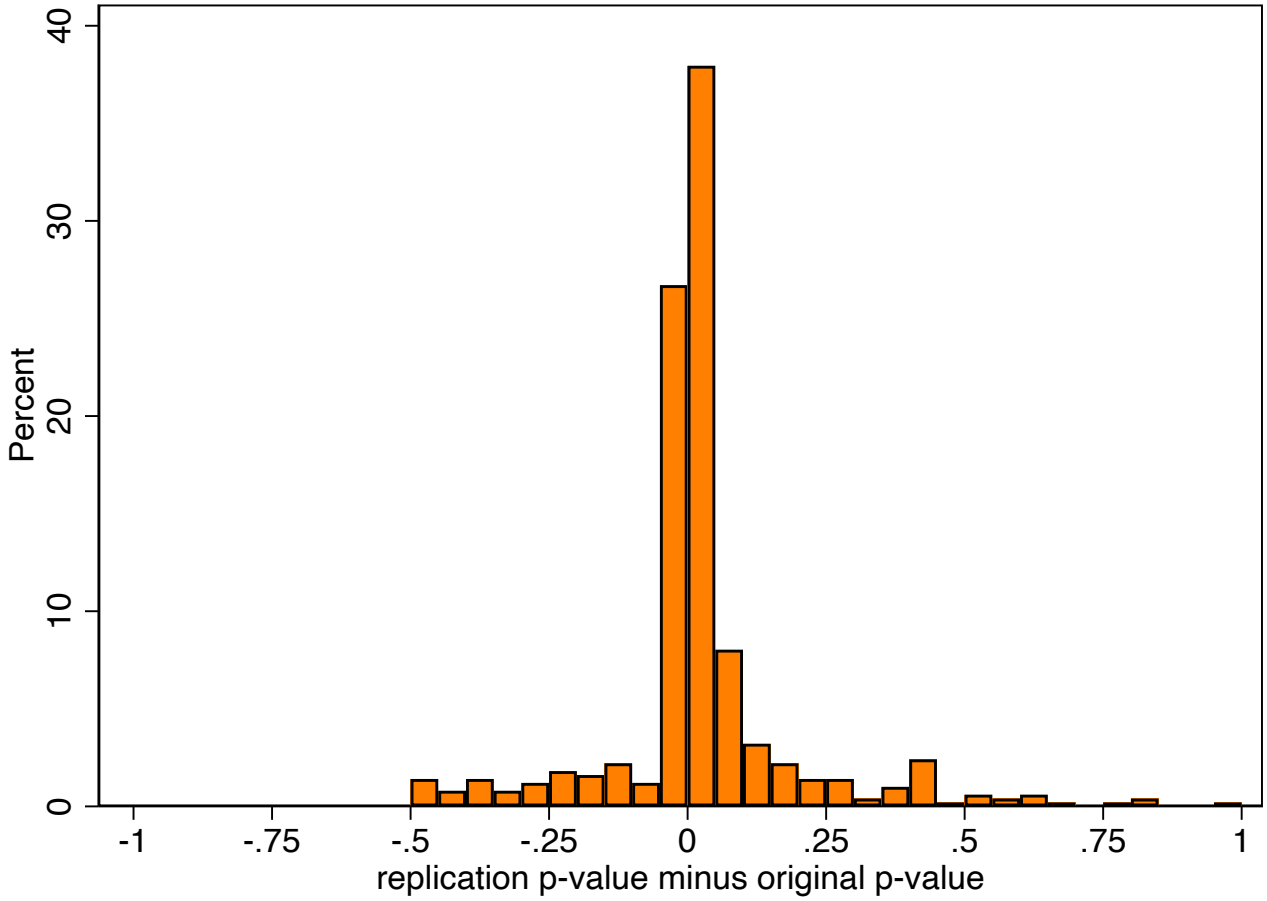
Notes: The sample is restricted to re-analyses for which the replicators changed the dependent variable. This figure illustrates the difference in p-values of the robustness reproduction/replication and original estimates.

Figure 34: Changing Main Independent Variable: Distribution of P-Values



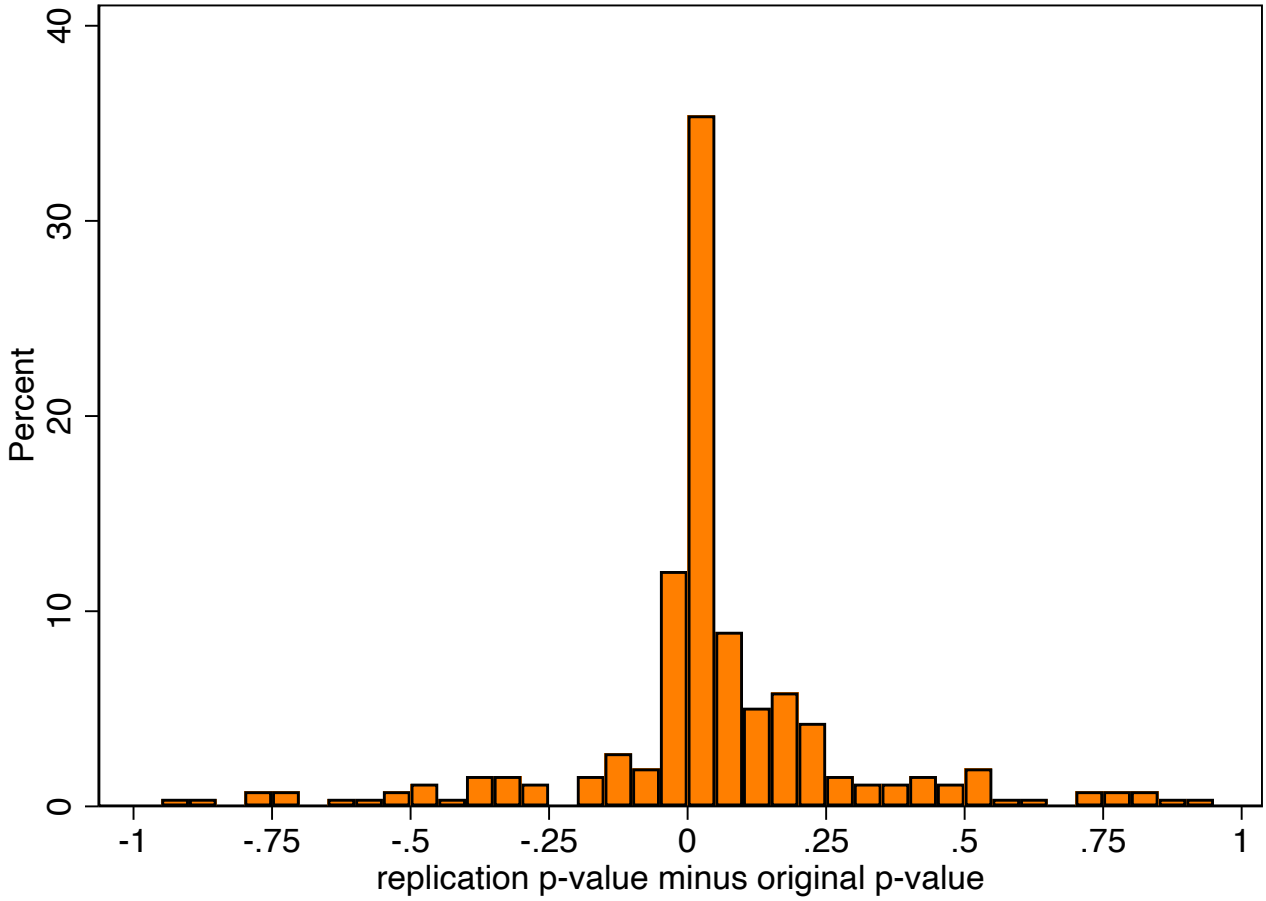
Notes: The sample is restricted to re-analyses for which the replicators changed the (coding of) the main independent variable. This figure illustrates the difference in p-values of the robustness reproduction/replication and original estimates.

Figure 35: Changing Estimation Method: Distribution of P-Values



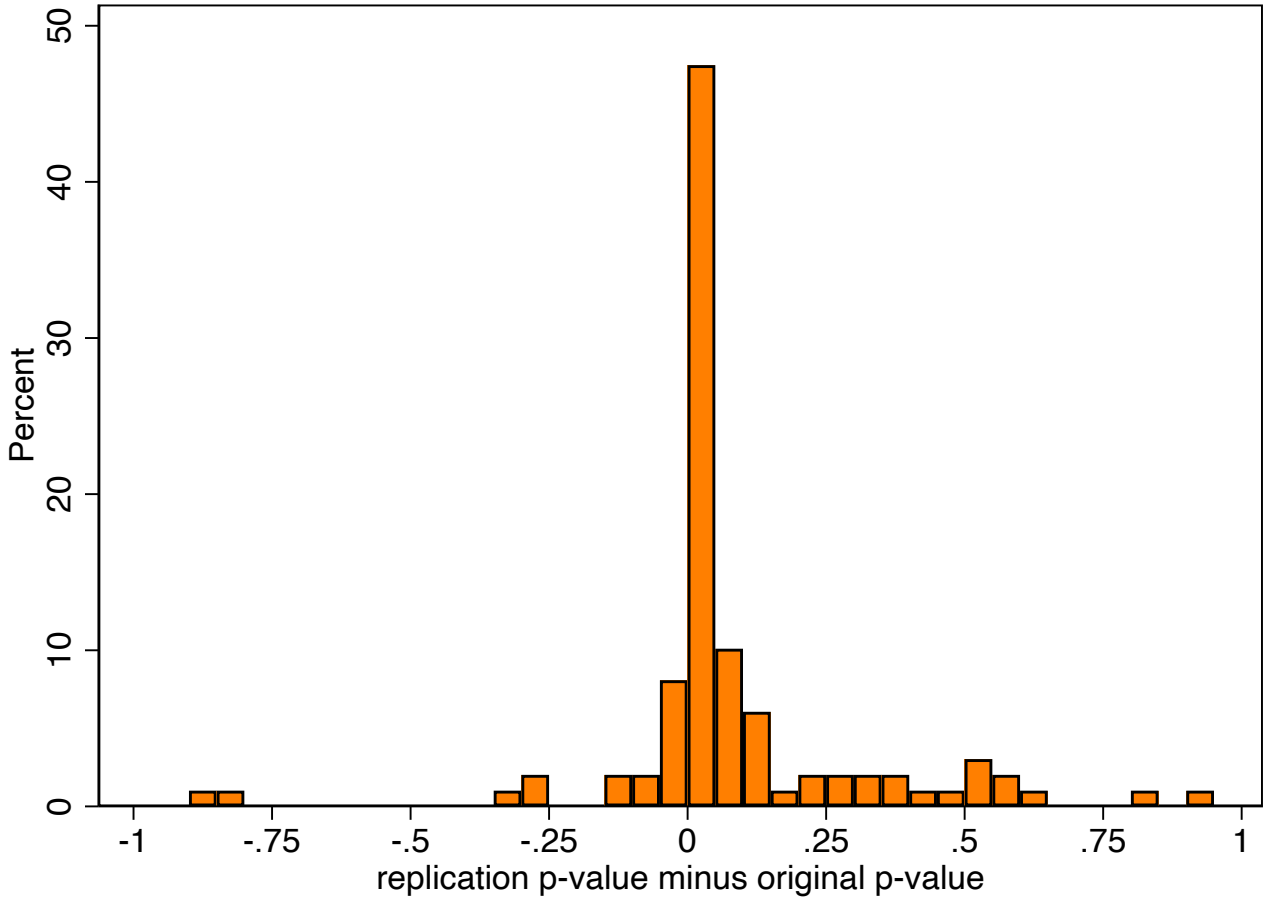
Notes: The sample is restricted to re-analyses for which the replicators changed the estimation method. This figure illustrates the difference in p-values of the robustness reproduction/replication and original estimates.

Figure 36: Changing Inference Method: Distribution of P-Values



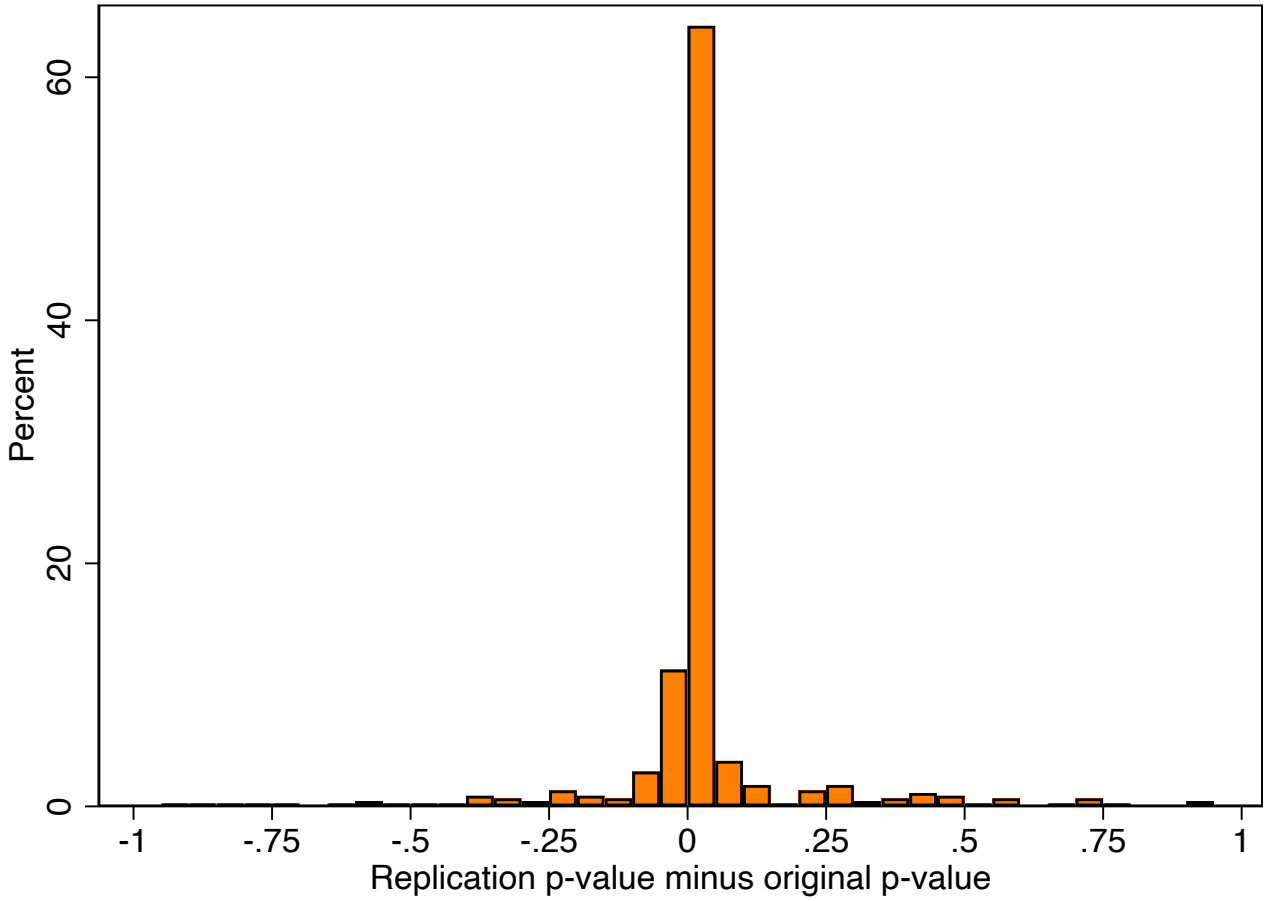
Notes: The sample is restricted to re-analyses for which the replicators changed the inference method. This figure illustrates the difference in p-values of the robustness reproduction/replication and original estimates.

Figure 37: Changing Weighting Scheme: Distribution of P-Values



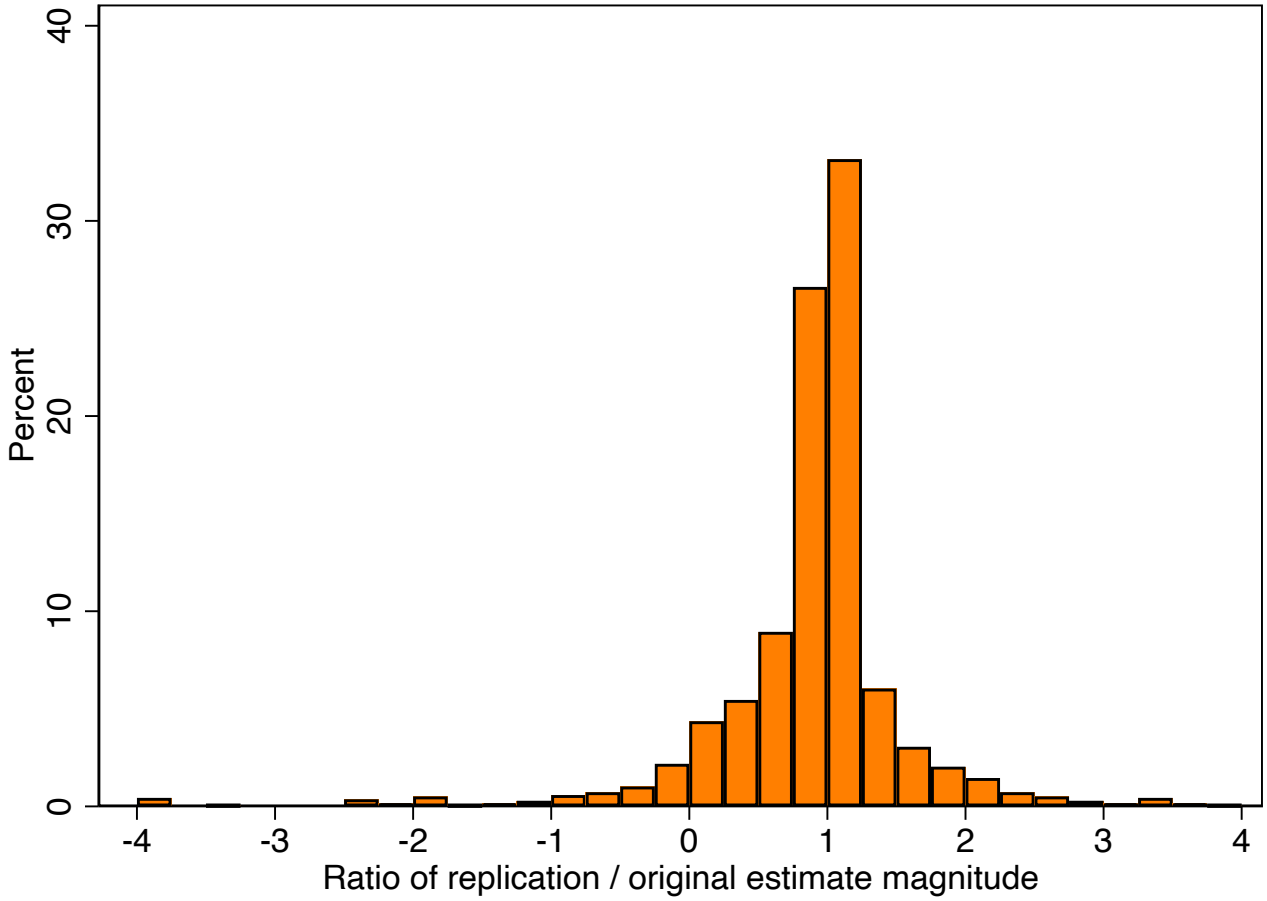
Notes: The sample is restricted to re-analyses for which the replicators changed the weighting scheme. This figure illustrates the difference in p-values of the robustness reproduction/replication and original estimates.

Figure 38: Use New Data: Distribution of P-Values



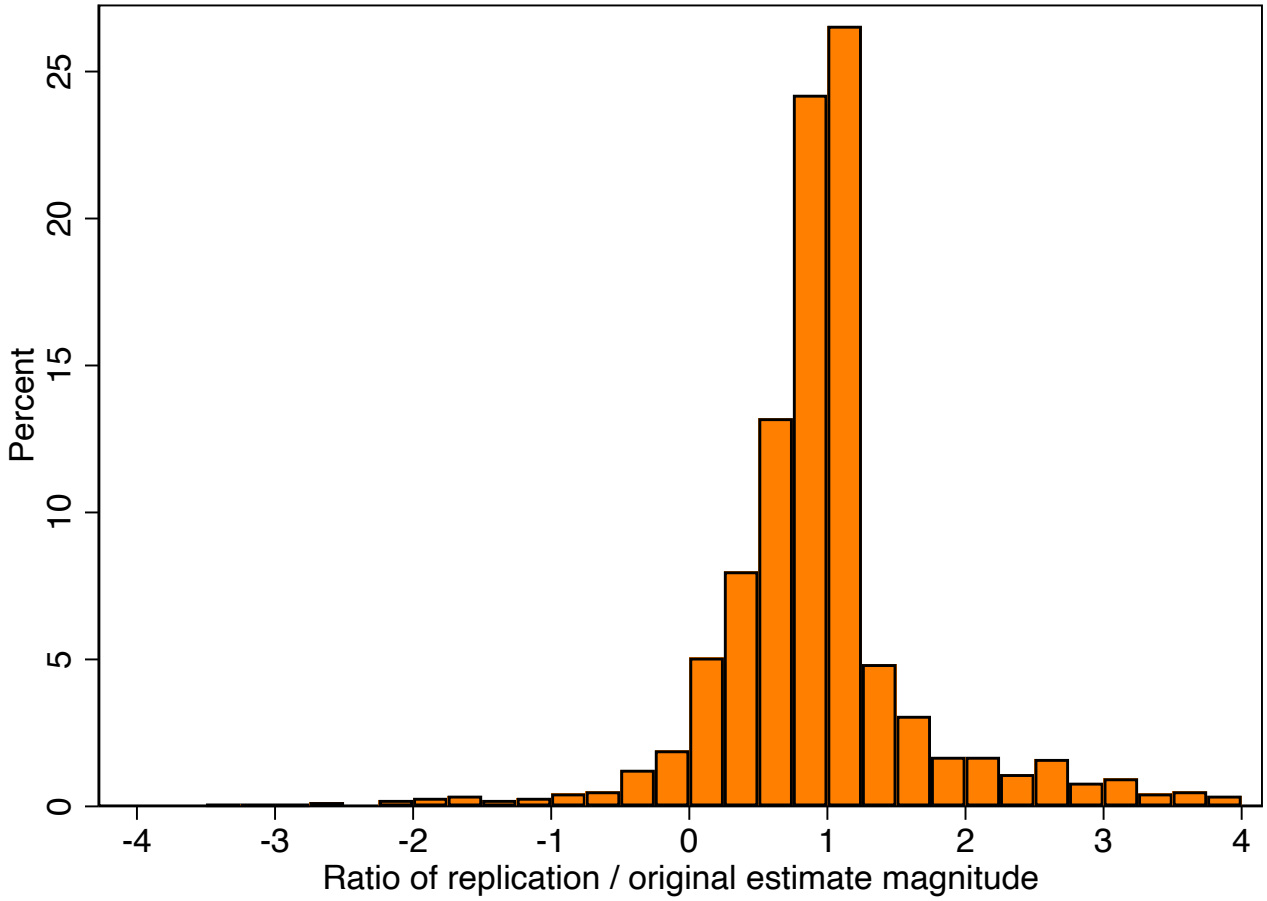
Notes: The sample is restricted to re-analyses for which the replicators used new data. This figure illustrates the difference in p-values of the robustness reproduction/replication and original estimates.

Figure 39: Alternative Control Variables: Relative Effect Size



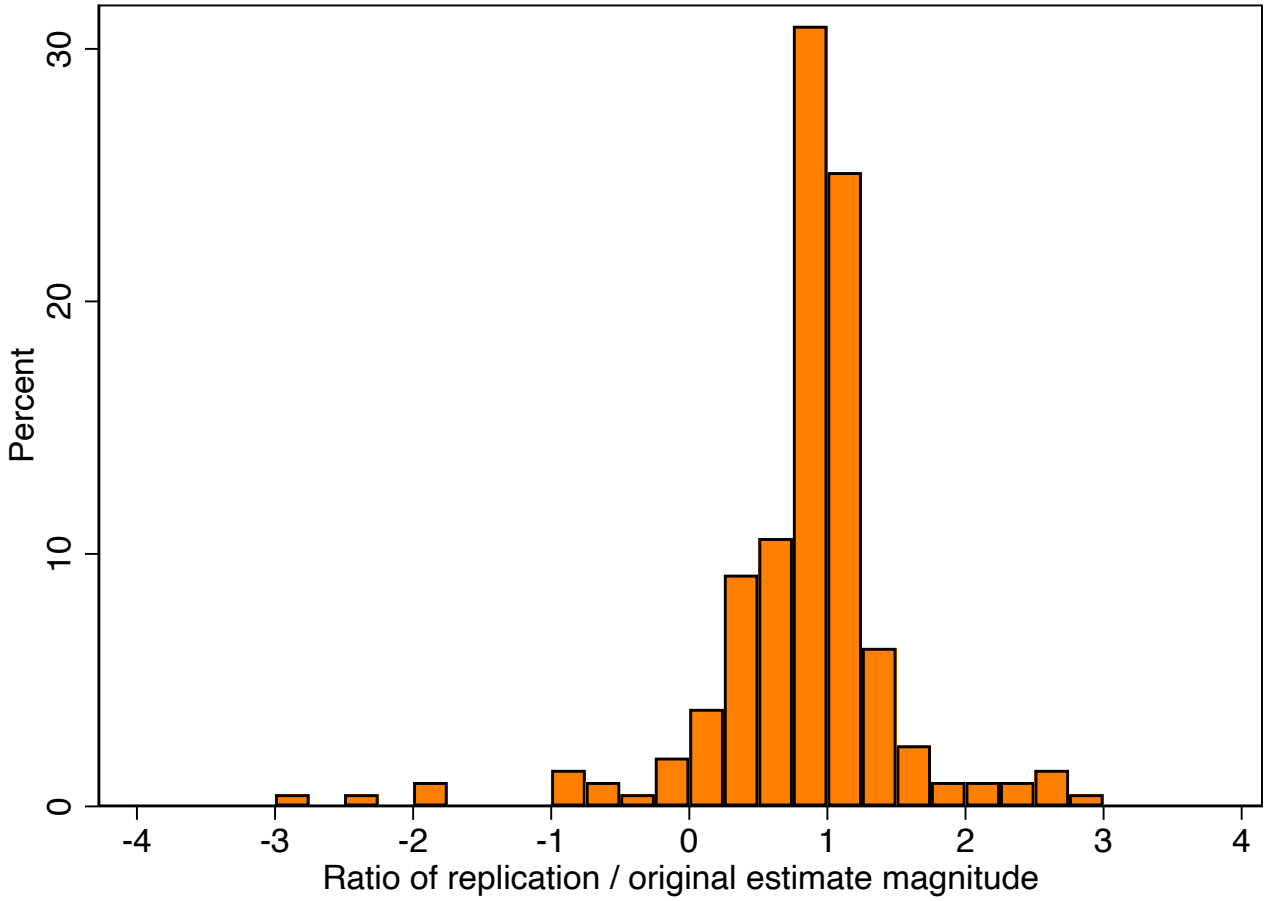
Notes: The sample is restricted to re-analyses for which an alternative control variables is made by the replicators. This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. 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.

Figure 40: Changing Sample: Relative Effect Size



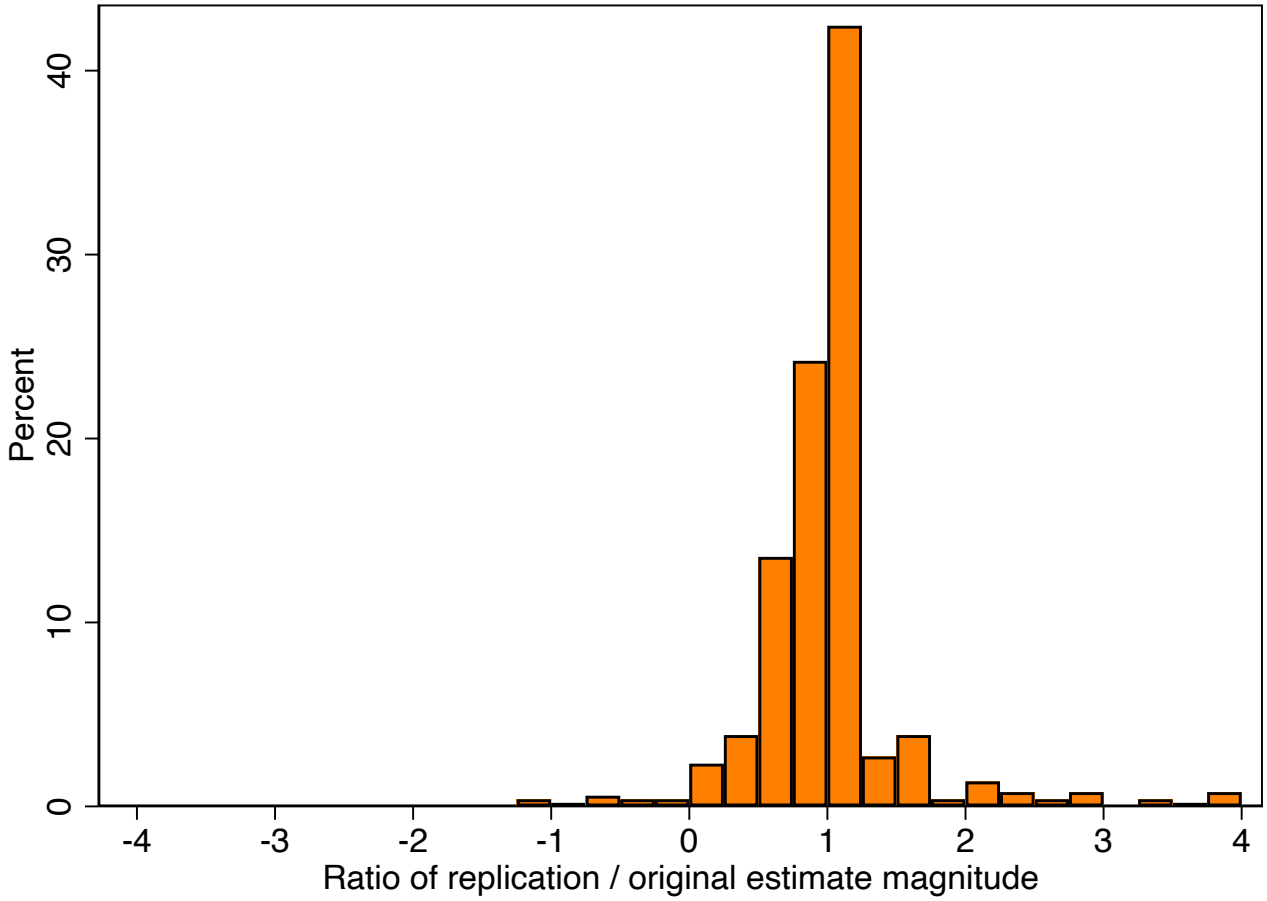
Notes: The sample is restricted to re-analyses for which the replicators changed the sample. This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. A positive value indicates 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.

Figure 41: Changing Main Independent Variable: Relative Effect Size



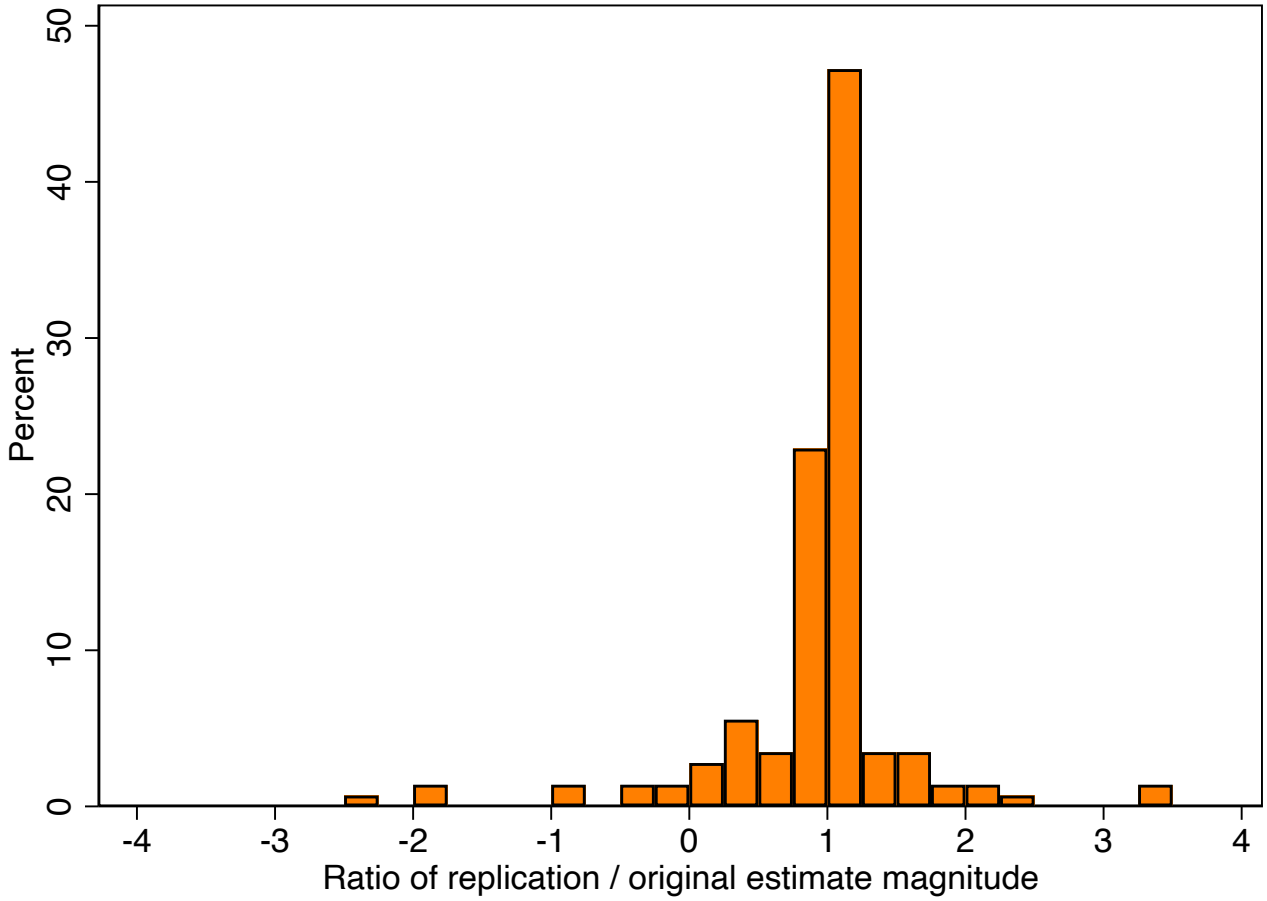
Notes: The sample is restricted to re-analyses for which the replicators changed the (coding of) the main independent variable. This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect 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.

Figure 42: Changing Estimation Method: Relative Effect Size



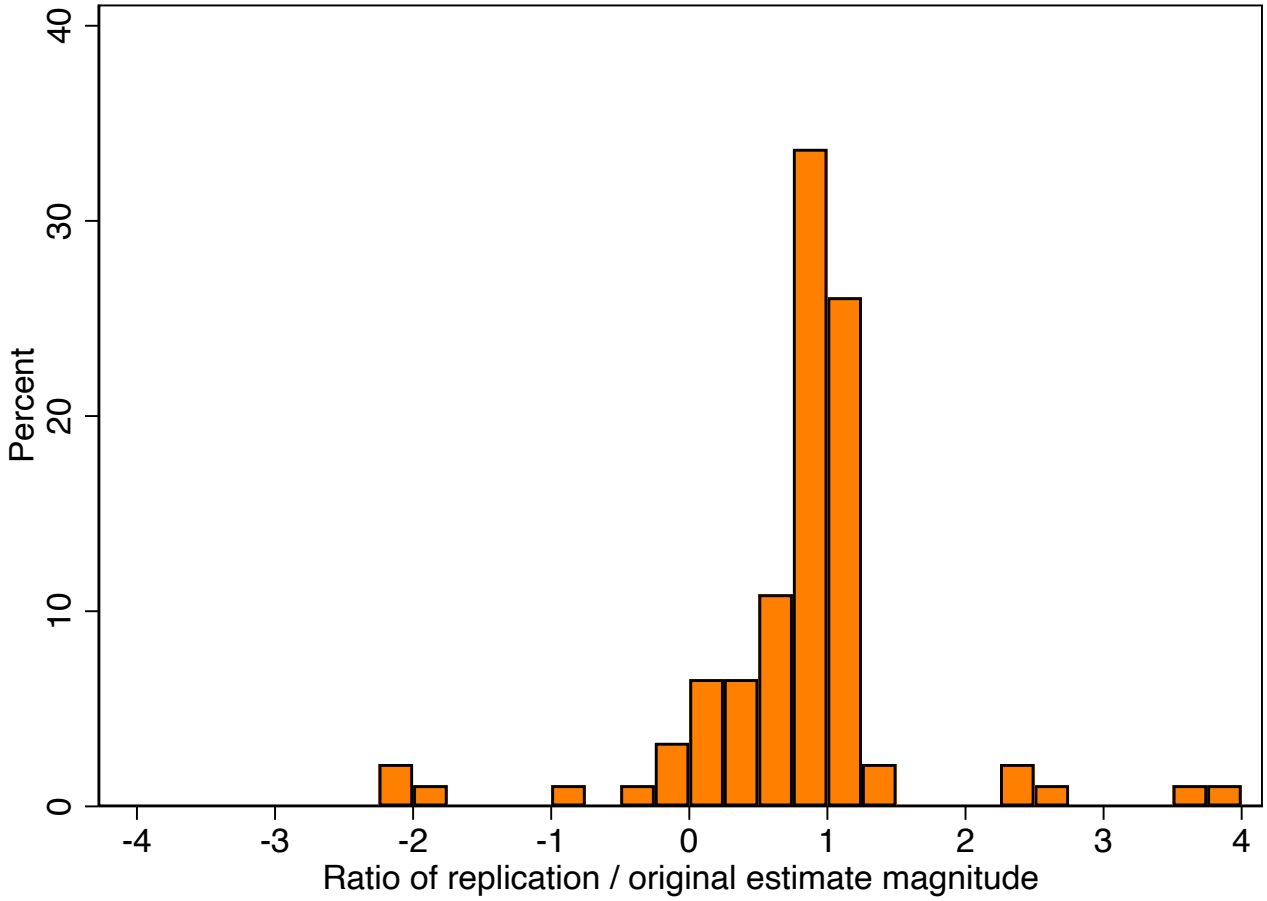
Notes: The sample is restricted to re-analyses for which the replicators changed the estimation method. This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. A positive value 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.

Figure 43: Changing Inference Method: Relative Effect Size



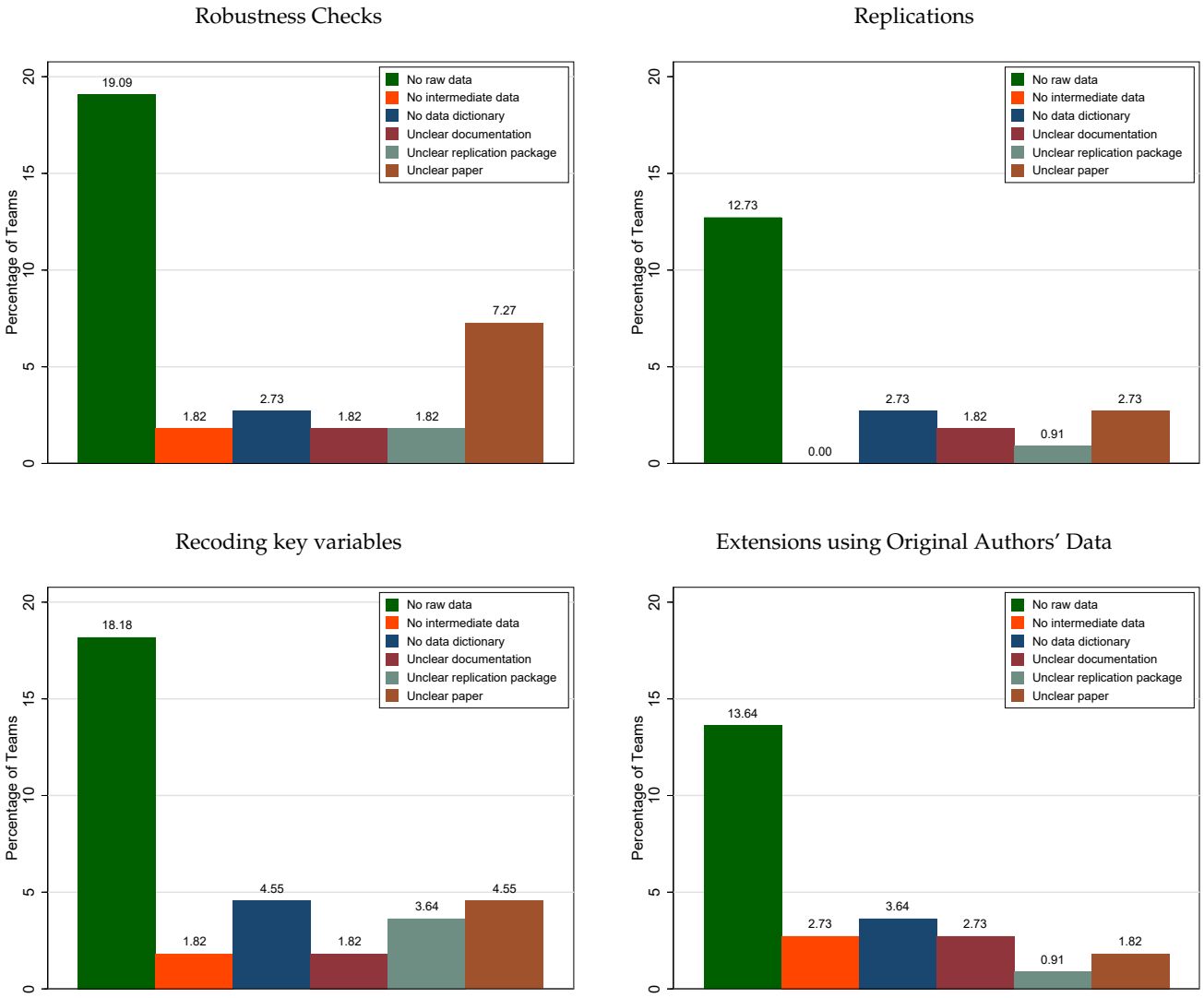
Notes: The sample is restricted to re-analyses for which the replicators changed the inference method. This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. A positive value 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.

Figure 44: Changing Weighting Scheme: Relative Effect Size



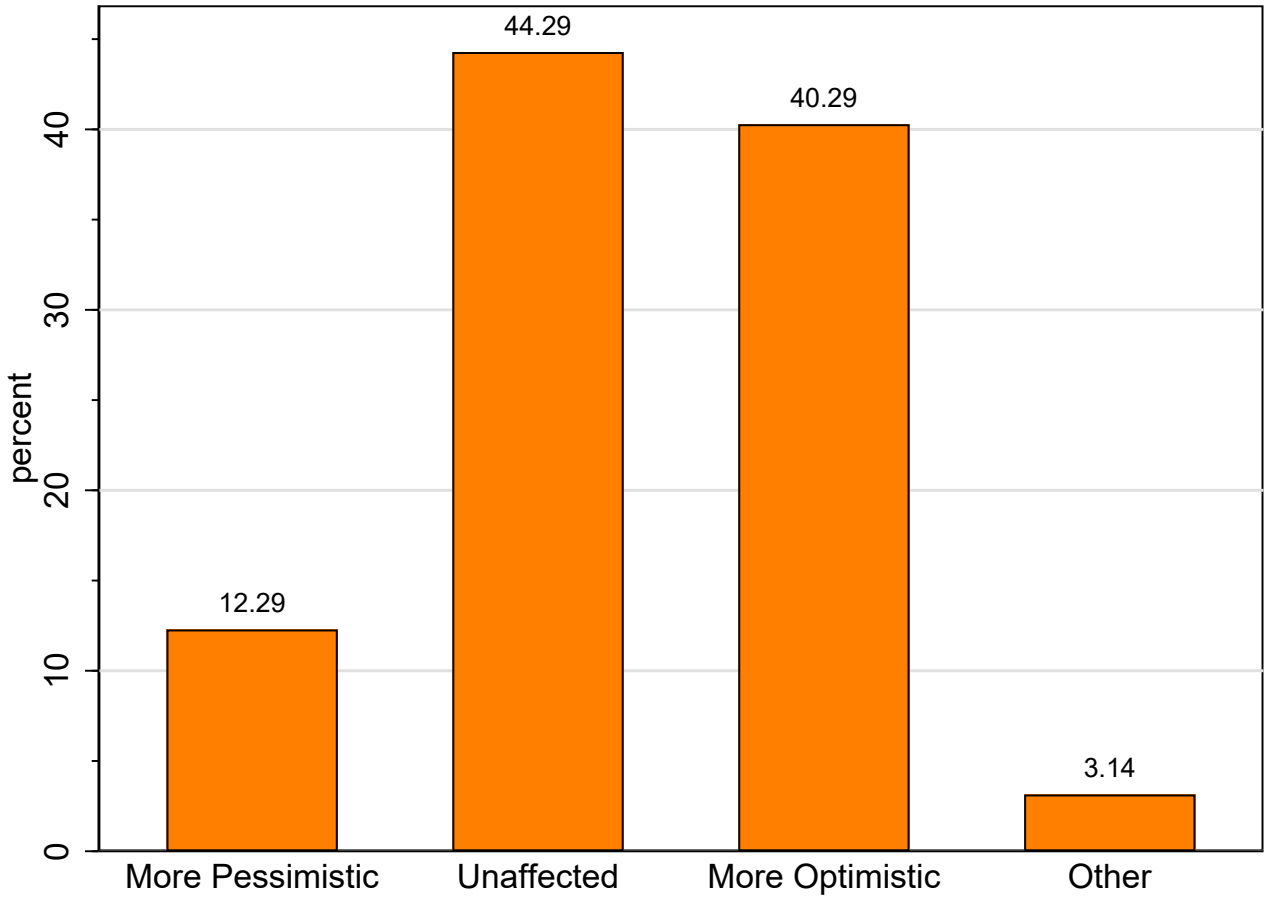
Notes: The sample is restricted to re-analyses for which the replicators changed the weighting scheme. This figure illustrates the ratio of robustness reproduction/replication and original estimates. The standardized effect sizes are normalized so that 1 equals the original effect size. A positive value 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.

Figure 45: 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)



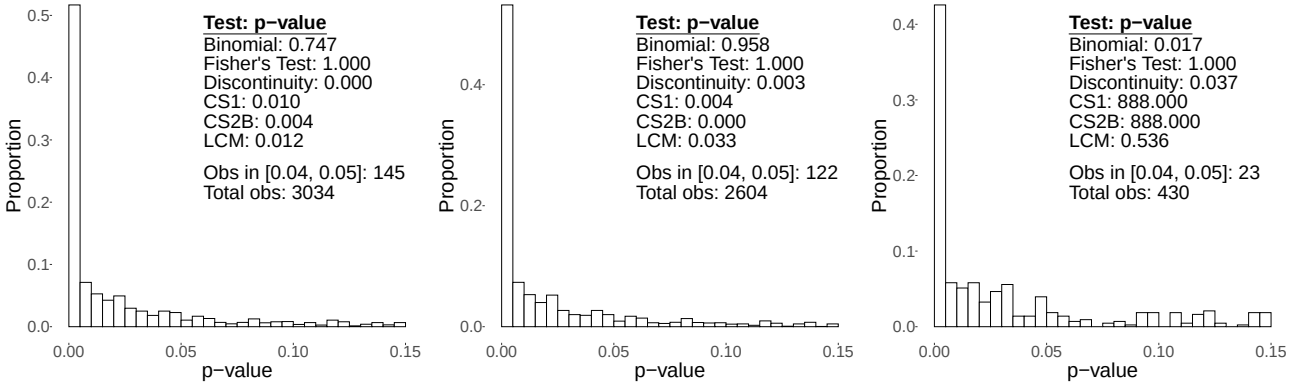
Notes: This Figure illustrates the share of teams who were unable to perform robustness checks (top-left), replications (top-right), key variable recodes (bottom-right) or extensions (bottom-left) for various reasons represented by the different coloured bars.

Figure 46: How does the quality of the replication package affect your view of the discipline as a whole?



Notes: This figure illustrates the responses to the question: "How does the quality of the replication package affect your view of the discipline as a whole?"

Figure 47: Applying Elliott et al. (2022)'s Tests



Notes: This figure present p-curves and results for the battery of p-hacking tests proposed in Elliott et al. (2022) for the full sample in the first panel, for the economics subsample in the second, and the political science subsample in the third. An error code of "888.00" represents an inability for that test to be calculated.

Appendix Tables

Table 7: Communication with Original Authors

	# Authors Contacted (1)	% Responded (2)	% Short Note (3)	% Feedback (4)	% Formal Response (5)
Economics	75	93%	11%	61%	28%
Political Science	31	97%	14%	53%	33%
Total	106	94%	11%	59%	30%

Notes: This table provides information about original authors' responses. The second column shows that 94% of original authors that A.B. reached out to responded to his email. The remaining columns restrict the sample to those that responded.

Table 8: JEL Codes in our Sample

Top 10 JEL Codes in our Sample	Our Sample (All)		Representative Sample	
	Rank	%	Rank	%
D: Microeconomics	1	54.4	1	15.2
J: Labor and Demographic Economics	2	33.8	5	8.4
O: Economic Dev., Innov., Tech. Change, and Growth	3	33.8	6	7.9
I: Health, Education, and Welfare	4	29.4	10	6.3
H: Public Economics	5	17.6	9	6.3
N: Economic History	6	17.6	15	1.4
C: Mathematical and Quantitative Methods	7	16.2	2	15.1
E: Macroeconomics and Monetary Economics	8	13.2	4	10.7
L: Industrial Organization	9	13.2	11	5.6
G: Financial Economics	10	5.8	3	13.9
Q: Ag. and NR Econ & Envr. and Ecological Econ	11	7.4	7	7.7
P: Pol. Econ. and Comp. Economic Systems	12	5.8	17	0.8
Z: Other Special Topics	13	8.3	16	1
M: Bus. Admin and Bus. Econ & Mktg & Accg & Personnel Econ	14	3.3	13	1.8
R: Urban, Rural, Regional, Real Estate, and Trans. Economics	15	5.8	12	2.9
F: International Economics	16	2.5	8	7.6
K: Law and Economics	17	8.3	14	1.4
A: Gen. Econ & Teaching	18	NA	18	0.4
B: History of Econ Thought, Methodol., Heterodox Approaches	19	NA	19	0.4
Y: Miscellaneous Categories	20	NA	20	0.2

Notes: This table compares the JEL Codes in our sample and in a representative sample of economics papers (Hoces de la Guardia et al. (2024)). The JEL Codes are only available for some of the economic journals.

Table 9: Levels of 10-point Computational Reproducibility Scale

	Availability of materials, and reproducibility									
	Analysis Code		**Analysis Data**		**CRA**	**Cleaning Code**		**Raw Data**		**CRR**
	P	C	P	C		P	C	P	C	
L1: No materials	–	–	–	–	–	–	–	–	–	–
L2: Only code	✓	✓	–	–	–	–	–	–	–	–
L3: Partial analysis data & code	✓	✓	✓	–	–	–	–	–	–	–
L4: All analysis data & code	✓	✓	✓	✓	–	–	–	–	–	–
L5: Reproducible from analysis	✓	✓	✓	✓	✓	–	–	–	–	–
L6: All cleaning code	✓	✓	✓	✓	–	✓	✓	–	–	–
L7: Some raw data	✓	✓	✓	✓	–	✓	✓	✓	–	–
L8: All raw data	✓	✓	✓	✓	–	✓	✓	✓	✓	–
L9: All raw data + CRA	✓	✓	✓	✓	✓	✓	✓	✓	✓	–
L10: Reproducible from raw data	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Computationally Reproducible from Analytic data (CRA): The output can be reproduced with minimal effort starting from the analytic datasets. Computationally Reproducible from Raw data (CRR): The output can be reproduced with minimal effort from the raw datasets. **P** denotes ”partial”, **C** denotes ”complete”.

Table 10: Recoding Using Same or Different Softwares

	Identical (1)	Minor Differences (2)	Major Differences (3)	Total (4)
Same Software (Without Looking)	2	2	1	5
Different Software (Without Looking)	1	1	0	2
Different Software (Looking)	8	7	2	17
Total	10	10	3	23

Notes: This table illustrates the number of reports recoding the analysis (i) in the same software without looking at the authors' code/programs, (ii) using a different software language without looking at the authors' code/programs or (iii) using a different software language looking at the authors' code/programs.

Table 11: Shifts in Statistical Significance Regions

	Re-Analysis Significance Level					
Original Significance Level	Sign Change	Not Sig.	Sig. at 10%	Sig. at 5%	Sig. at 1%	Total
Not Significant	4.23	23.31	1.43	1.22	0.90	31.09
Significant at 10%	0.50	3.30	2.04	0.93	0.50	7.27
Significant at 5%	0.58	5.87	2.54	8.64	3.41	21.04
Significant at 1%	2.01	5.23	1.80	3.28	28.28	40.60
Total	7.32	37.72	7.80	14.06	33.10	100.00

Notes: This table illustrates shifts across significance and insignificance regions. Each row focuses on an initial level of statistical significance. Each column reports the share of re-analyses that ended up in each statistical significance region.

Table 12: Shifts in Statistical Significance Regions (Frequency)

	Re-Analysis Significance Level					
Original Significance Level	Sign Change	Not Sig.	Sig. at 10%	Sig. at 5%	Sig. at 1%	Total
Not Significant	160	882	54	46	34	1176
Significant at 10%	19	125	77	35	19	275
Significant at 5%	22	222	96	327	129	796
Significant at 1%	76	198	68	124	1070	1536
Total	277	1427	295	532	1252	3783

Notes: This table illustrates shifts across significance and insignificance regions. Each row focuses on an initial level of statistical significance. Each column reports the number of re-analyses that ended up in each statistical significance region.

Table 13: Robustness Reproducibility and Replicability Rates (with counts)

	(1) Full Sample	(2) Change Control	(3) Dep. Var.	(4) Change Estim.	(5) Infer. Method	(6) Ind. Var.	(7) Change Sample	(8) Change Weights	(9) New Data
Rep. if Orig. Sig. 5%									
Estimates	0.71	0.76	0.45	0.76	0.74	0.78	0.64	0.74	0.87
Confidence Intervals	[0.70,0.73]	[0.73,0.79]	[0.35,0.55]	[0.72,0.81]	[0.67,0.82]	[0.72,0.85]	[0.61,0.67]	[0.64,0.85]	[0.84,0.91]
Observations	2552	833	96	348	121	160	945	66	370
Rep. if Orig. Not Sig. 5%									
Estimates	0.88	0.92	0.80	0.85	0.88	0.77	0.86	0.97	0.83
	[0.87,0.90]	[0.89,0.94]	[0.64,0.96]	[0.80,0.90]	[0.83,0.94]	[0.64,0.89]	[0.83,0.89]	[0.91,1.03]	[0.75,0.91]
Observations	1453	594	25	174	129	47	468	33	83
Rep. if Orig. Sig. 10%									
Estimates	0.75	0.78	0.45	0.83	0.74	0.80	0.70	0.73	0.89
Confidence Intervals	[0.74,0.77]	[0.75,0.81]	[0.36,0.55]	[0.79,0.86]	[0.67,0.82]	[0.74,0.86]	[0.67,0.73]	[0.63,0.83]	[0.86,0.92]
Observations	2826	932	106	373	137	168	1068	74	382
Rep. if Orig. Not Sig. 10%									
Estimates	0.85	0.88	0.93	0.82	0.84	0.54	0.82	0.92	0.75
Confidence Intervals	[0.83,0.87]	[0.85,0.91]	[0.80,1.06]	[0.76,0.88]	[0.77,0.91]	[0.38,0.70]	[0.78,0.86]	[0.81,1.03]	[0.64,0.85]
Observations	1179	495	15	149	113	39	345	25	71

Notes: Robustness reproducibility and replicability rates for four definitions by type of re-analyses. Columns present robustness reproducibility rates by type of re-analyses, which are not mutually exclusive. Columns 1-8 do not include re-analysis that use new data, while column 9 does. In (2), the re-analysis changed the control variables. In (3), the re-analysis changed the dependent variable. In (4), the re-analysis changed the estimation method. In (5), the re-analysis changed the inference method. In (6), the re-analysis changed the main independent variable. In (7), the re-analysis changed the sample. In (8), the re-analysis changed the weights applied, or applied weights for the first time. In (9), we present robustness replicability rates for re-analyses that introduced new data. 95% confidence intervals presented in square brackets.

Table 14: Robustness Reproducibility and Replicability Rates (with counts, weighted)

	(1) Full Sample	(2) Change Control	(3) Dep. Var.	(4) Change Estim.	(5) Infer. Method	(6) Ind. Var.	(7) Change Sample	(8) Change Weights	(9) New Data
Rep. if Orig. Sig. 5%									
Estimate	0.67	0.68	0.42	0.71	0.69	0.83	0.62	0.81	0.91
Confidence Intervel	[0.66,0.69]	[0.65,0.71]	[0.32,0.52]	[0.66,0.76]	[0.60,0.77]	[0.77,0.89]	[0.59,0.65]	[0.71,0.91]	[0.88,0.94]
Observations	2552	833	96	348	121	160	945	66	370
Rep. if Orig. Not Sig. 5%									
Estimate	0.88	0.92	0.74	0.87	0.88	0.72	0.85	0.95	0.83
Confidence Interval	[0.86,0.90]	[0.89,0.94]	[0.57,0.92]	[0.82,0.92]	[0.82,0.94]	[0.59,0.85]	[0.82,0.88]	[0.88,1.03]	[0.75,0.91]
Observations	1453	594	25	174	129	47	468	33	83
Rep. if Orig. Sig. 10%									
Estimate	0.72	0.71	0.42	0.81	0.69	0.84	0.69	0.81	0.91
Confidence Interval	[0.70,0.73]	[0.68,0.74]	[0.33,0.52]	[0.77,0.85]	[0.62,0.77]	[0.79,0.90]	[0.66,0.71]	[0.72,0.90]	[0.89,0.94]
Observations	2826	932	106	373	137	168	1068	74	382
Rep. if Orig. Not Sig. 10%									
Estimate	0.84	0.88	0.93	0.84	0.82	0.53	0.80	0.91	0.74
Confidence Interval	[0.82,0.86]	[0.85,0.90]	[0.80,1.06]	[0.78,0.90]	[0.75,0.89]	[0.37,0.69]	[0.76,0.84]	[0.80,1.02]	[0.64,0.84]
Observations	1179	495	15	149	113	39	345	25	71

Notes: This is the same as Table 13 except applying article weights. Robustness reproducibility and replicability rates for four definitions by type of re-analyses. Columns present robustness reproducibility rates by type of re-analyses, which are not mutually exclusive. Columns 1-8 do not include re-analysis that use new data, while column 9 does. In (2), the re-analysis changed the control variables. In (3), the re-analysis changed the dependent variable. In (4), the re-analysis changed the estimation method. In (5), the re-analysis changed the inference method. In (6), the re-analysis changed the main independent variable. In (7), the re-analysis changed the sample. In (8), the re-analysis changed the weights applied, or applied weights for the first time. In (9), we present robustness replicability rates for re-analyses that introduced new data. 95% confidence intervals presented in square brackets.

Table 15: Please indicate the degree to which your experience with I4R has contributed to your improvement in the following areas (select all which apply):

	Nothing	A Little	Moderately	A Lot	Don't Know	Not Applicable
Networking	10.40	46.82	27.17	10.69	2.89	2.02
Coding Skills	19.08	40.17	26.88	10.98	1.73	1.16
Capacity to write a good replication package	5.19	21.90	46.97	23.63	1.15	1.15
Learning difference between reproduction and replication	6.65	19.36	36.71	33.53	3.47	0.29
Further ability as a researcher	5.20	39.02	38.15	17.05	0.29	0.29
Communicate issues with a paper to others	3.75	28.82	41.50	23.05	0.58	2.31

Notes: This table provides information on replicators' feelings about how I4R contributed to their improvement in various areas. Each row represents a different category. Values are percentages and all rows in a category sum to 100. All values are unweighted.

Table 16: Many-Analysts' Replication Rate And Replicator Characteristics For Published Results Originally Statistically Significant at the 10% Level

	Category				
RQ	Neg. & Sig.	Neg. & Not Sig.	Pos. & Not Sig.	Pos. & Sig.	Total
1	28.33	68.89	2.78	0.00	100.00
2	37.96	37.04	16.67	8.33	100.00
3	0.00	47.22	50.00	2.78	100.00
4a	0.00	8.33	33.33	58.33	100.00
4b	16.67	8.33	41.67	33.33	100.00
4c	8.33	58.33	0.00	33.33	100.00
5a	5.56	19.44	25.00	50.00	100.00
5b	16.67	36.11	30.56	16.67	100.00
5c	13.89	69.44	0.00	16.67	100.00
6	0.00	16.67	66.67	16.67	100.00
7	8.33	0.00	55.56	36.11	100.00
8	0.00	16.67	75.00	8.33	100.00

Notes: Six many-analyst teams independently answered eight pre-registered research questions concerning the possible relationship between replication rate and selected author/replicator characteristics. This table restricts the analysis to originally published estimates that were statistically significant at the 10% level. The **columns** represent one of four categories that a many-analyst could classify their analysis; if a many-analyst found a relationship that was statistically significant (at the 5% level) and positive, it was included in the column 'Pos. & Sig.' The **rows** represent eight pre-registered research questions (two have three sub-questions). They are: 1- Does reproducibility/replicability rate depend on replicators' experience coding? 2- Does reproducibility/replicability rate depend on replicators' academic experience? 3- Does reproducibility/replicability rate depend on the authors' experience? 4a- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (i) Are reproducibility/replicability rate higher when authors' experience is high, and replicators' experience is low (in comparison to similar levels)? 4b- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (ii) Are reproducibility/replicability rate higher when authors' experience and replicators' experience is similar (in comparison to dissimilar levels)? 4c- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (iii) Are reproducibility/replicability rate higher when authors' experience is low, and replicators' experience is high (in comparison to similar levels)? 5a- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (i) Are reproducibility/replicability rate higher when authors' have high prestige, and replicators' experience have low prestige (in comparison to similar levels)? 5b- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (ii) Are reproducibility/replicability rate higher when authors' and replicators' prestige is similar (in comparison to dissimilar levels)? 5c- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (iii) Are reproducibility/replicability rate higher when authors' have low prestige, and replicators' experience have high prestige (in comparison to similar levels)? 6- Does reproducibility/replicability rate depend on the original authors providing raw data? 7- Does reproducibility/replicability rate depend on the original authors providing raw or intermediate data? 8- Does reproducibility/replicability rate depend on the original authors providing cleaning code? Most many-analysts provide more than one estimate per research question, this table weights many-analysts equally.

Table 17: Many-Analysts' Replication Rate And Replicator Characteristics For Published Results Originally **Not** Statistically Significant at the 5% Level

	Category				
RQ	Neg. & Sig.	Neg. & Not Sig.	Pos. & Not Sig.	Pos. & Sig.	Total
1	0.00	3.33	88.33	8.33	100.00
2	0.00	35.19	64.81	0.00	100.00
3	0.00	11.11	88.89	0.00	100.00
4a	0.00	33.33	50.00	16.67	100.00
4b	0.00	41.67	41.67	16.67	100.00
4c	0.00	25.00	50.00	25.00	100.00
5a	0.00	16.67	69.44	13.89	100.00
5b	5.56	61.11	25.00	8.33	100.00
5c	0.00	29.17	40.28	30.56	100.00
6	8.33	66.67	25.00	0.00	100.00
7	0.00	58.33	33.33	8.33	100.00
8	16.67	58.33	19.44	5.56	100.00

Notes: Six many-analyst teams independently answered eight pre-registered research questions concerning the possible relationship between replication rate and selected author/replicator characteristics. This table restricts the analysis to originally published estimates that were not statistically significant at the 5% level. The **columns** represent one of four categories that a many-analyst could classify their analysis; if a many-analyst found a relationship that was statistically significant (at the 5% level) and positive, it was included in the column 'Pos. & Sig.' The **rows** represent eight pre-registered research questions (two have three sub-questions). They are: 1- Does reproducibility/replicability rate depend on replicators' experience coding? 2- Does reproducibility/replicability rate depend on replicators' academic experience? 3- Does reproducibility/replicability rate depend on the authors' experience? 4a- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (i) Are reproducibility/replicability rate higher when authors' experience is high, and replicators' experience is low (in comparison to similar levels)? 4b- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (ii) Are reproducibility/replicability rate higher when authors' experience and replicators' experience is similar (in comparison to dissimilar levels)? 4c- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (iii) Are reproducibility/replicability rate higher when authors' experience is low, and replicators' experience is high (in comparison to similar levels)? 5a- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (i) Are reproducibility/replicability rate higher when authors' have high prestige, and replicators' experience have low prestige (in comparison to similar levels)? 5b- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (ii) Are reproducibility/replicability rate higher when authors' and replicators' prestige is similar (in comparison to dissimilar levels)? 5c- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (iii) Are reproducibility/replicability rate higher when authors' have low prestige, and replicators' experience have high prestige (in comparison to similar levels)? 6- Does reproducibility/replicability rate depend on the original authors providing raw data? 7- Does reproducibility/replicability rate depend on the original authors providing raw or intermediate data? 8- Does reproducibility/replicability rate depend on the original authors providing cleaning code? Most many-analysts provide more than one estimate per research question, this table weights many-analysts equally.

Table 18: Many-Analysts' Replication Rate And Replicator Characteristics For Published Results Originally **Not** Statistically Significant at the 10% Level

	Category				
RQ	Neg. & Sig.	Neg. & Not Sig.	Pos. & Not Sig.	Pos. & Sig.	Total
1	0.00	11.67	71.67	16.67	100.00
2	0.00	35.19	64.81	0.00	100.00
3	0.00	36.11	63.89	0.00	100.00
4a	0.00	16.67	75.00	8.33	100.00
4b	0.00	38.89	52.78	8.33	100.00
4c	0.00	16.67	66.67	16.67	100.00
5a	0.00	45.83	29.17	25.00	100.00
5b	0.00	66.67	25.00	8.33	100.00
5c	0.00	37.50	37.50	25.00	100.00
6	0.00	83.33	16.67	0.00	100.00
7	0.00	61.11	30.56	8.33	100.00
8	16.67	58.33	16.67	8.33	100.00

Notes: Six many-analyst teams independently answered eight pre-registered research questions concerning the possible relationship between replication rate and selected author/replicator characteristics. This table restricts the analysis to originally published estimates that were not statistically significant at the 10% level. The **columns** represent one of four categories that a many-analyst could classify their analysis; if a many-analyst found a relationship that was statistically significant (at the 5% level) and positive, it was included in the column 'Pos. & Sig.' The **rows** represent eight pre-registered research questions (two have three sub-questions). They are: 1- Does reproducibility/replicability rate depend on replicators' experience coding? 2- Does reproducibility/replicability rate depend on replicators' academic experience? 3- Does reproducibility/replicability rate depend on the authors' experience? 4a- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (i) Are reproducibility/replicability rate higher when authors' experience is high, and replicators' experience is low (in comparison to similar levels)? 4b- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (ii) Are reproducibility/replicability rate higher when authors' experience and replicators' experience is similar (in comparison to dissimilar levels)? 4c- Does reproducibility/replicability rate depend on the interaction of the authors' experience and replicators' experience? In particular, (iii) Are reproducibility/replicability rate higher when authors' experience is low, and replicators' experience is high (in comparison to similar levels)? 5a- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (i) Are reproducibility/replicability rate higher when authors' have high prestige, and replicators' experience have low prestige (in comparison to similar levels)? 5b- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (ii) Are reproducibility/replicability rate higher when authors' and replicators' prestige is similar (in comparison to dissimilar levels)? 5c- Does reproducibility/replicability rate depend on the interaction of the authors' prestige and replicators' prestige? In particular, (iii) Are reproducibility/replicability rate higher when authors' have low prestige, and replicators' experience have high prestige (in comparison to similar levels)? 6- Does reproducibility/replicability rate depend on the original authors providing raw data? 7- Does reproducibility/replicability rate depend on the original authors providing raw or intermediate data? 8- Does reproducibility/replicability rate depend on the original authors providing cleaning code? Most many-analysts provide more than one estimate per research question, this table weights many-analysts equally.

Table 19: Many-Analysts' Replication Rate And Replicator Characteristics - Only if Analyst Indicated the Effect Size was Meaningful

Dependent Variable: Original Result Statistically Significant at 5% Level					
	Category				
RQ	Neg. & Sig.	Neg. & Not Sig.	Pos. & Not Sig.	Pos. & Sig.	Total
1	54.17	45.83	0.00	0.00	100.00
2	47.33	28.67	14.00	10.00	100.00
3	0.00	27.78	38.89	33.33	100.00
4a	0.00	0.00	50.00	50.00	100.00
4b	20.00	0.00	40.00	40.00	100.00
4c	16.67	50.00	16.67	16.67	100.00
5a	0.00	16.67	52.78	30.56	100.00
5b	12.50	25.00	37.50	25.00	100.00
5c	33.33	41.67	0.00	25.00	100.00
6	0.00	30.00	50.00	20.00	100.00
7	20.00	6.67	53.33	20.00	100.00
8	0.00	34.00	66.00	0.00	100.00

Dependent Variable: Original Result Statistically Significant at 10% Level					
	Category				
RQ	Neg. & Sig.	Neg. & Not Sig.	Pos. & Not Sig.	Pos. & Sig.	Total
1	50.00	50.00	0.00	0.00	100.00
2	55.00	25.00	10.00	10.00	100.00
3	0.00	41.67	25.00	33.33	100.00
4a	0.00	0.00	12.50	87.50	100.00
4b	25.00	0.00	25.00	50.00	100.00
4c	8.33	58.33	0.00	33.33	100.00
5a	6.67	13.33	20.00	60.00	100.00
5b	25.00	25.00	25.00	25.00	100.00
5c	16.67	63.33	0.00	20.00	100.00
6	0.00	20.00	60.00	20.00	100.00
7	20.00	0.00	26.67	53.33	100.00
8	0.00	37.50	50.00	12.50	100.00

Dependent Variable: Original Result *Not* Statistically Significant at 5% Level					
	Category				
RQ	Neg. & Sig.	Neg. & Not Sig.	Pos. & Not Sig.	Pos. & Sig.	Total
1	0.00	0.00	83.33	16.67	100.00
2	0.00	100.00	0.00	0.00	100.00
3	0.00	41.67	58.33	0.00	100.00
4a	0.00	33.33	33.33	33.33	100.00
4b	0.00	33.33	33.33	33.33	100.00
4c	0.00	33.33	33.33	33.33	100.00
5a	0.00	0.00	72.22	27.78	100.00
5b	11.11	72.22	0.00	16.67	100.00
5c	0.00	37.50	16.67	45.83	100.00
6	12.50	75.00	12.50	0.00	100.00
7	0.00	50.00	33.33	16.67	100.00
8	50.00	33.33	5.56	11.11	100.00

Dependent Variable: Original Result *Not* Statistically Significant at 10% Level					
	Category				
RQ	Neg. & Sig.	Neg. & Not Sig.	Pos. & Not Sig.	Pos. & Sig.	Total
1	0.00	0.00	83.33	16.67	100.00
2	0.00	100.00	0.00	0.00	100.00
3	0.00	75.00	25.00	0.00	100.00
4a	0.00	0.00	83.33	16.67	100.00
4b	0.00	50.00	33.33	16.67	100.00
4c	0.00	12.50	75.00	12.50	100.00
5a	0.00	12.50	50.00	37.50	100.00
5b	0.00	83.33	0.00	16.67	100.00
5c	0.00	37.50	25.00	37.50	100.00
6	0.00	87.50	12.50	0.00	100.00
7	0.00	38.89	44.44	16.67	100.00
8	33.33	50.00	0.00	16.67	100.00

Notes: This table presents the same analysis as in Tables 6, 16, 17, and 18 while only including analyst results that were indicated by the analysis that "in your opinion, is the estimated effect size economically meaningful?" The first panel corresponds to Table 6. The second panel corresponds to Table 16. The third panel corresponds to Table 17. The fourth panel corresponds to Table 18. The rows correspond to the same research questions, and the columns represent the same effect sign and statistical significance categories. The cells remain weighted in the same manner.

Table 20: Randomization Tests, Significance at 5% Level

	Original Analysis
Proportion Significant in .05 $\pm.04$	0.776
One-Sided p-value against 0.50	0.000
Number of Tests in .05 $\pm.04$	1861.000
Proportion Significant in .05 $\pm.03$	0.748
One-Sided p-value against 0.50	0.000
Number of Tests in .05 $\pm.03$	1243.000
Proportion Significant in .05 $\pm.02$	0.680
One-Sided p-value against 0.50	0.000
Number of Tests in .05 $\pm.02$	747.000
Proportion Significant in .05 $\pm.01$	0.677
One-Sided p-value against 0.50	0.000
Number of Tests in .05 $\pm.01$	394.000

Notes: Following Brodeur et al. (2020), in this table we present the results of binomial proportion tests where a success is defined as a statistically significant observation at the 5% level. In the first panel we use observations where $(0.01 < p < 0.09)$. The lower panels use smaller windows. We test if the proportion is statistically greater than 0.50. The associated p-values are then reported. We also include the number of observations in the third row. We do not weight articles.

Table 21: Caliper Tests, Significance at 5% Level

	Significant at 5% Level			
	(1)	(2)	(3)	(4)
Re-Analysis=1	-0.089***	-0.111**	-0.092*	-0.129*
	(0.031)	(0.046)	(0.051)	(0.072)
Observations	1,861	1,243	747	394
Threshold	0.05	0.05	0.05	0.05
Window	0.04	0.03	0.02	0.01

Notes: The dependent variable takes a value of one if $p \leq 0.05$. The variable Re-Analysis takes a value of one if the p-value is associated with a re-analysis, and zero if it is associated with the original publication. For example, in column 1 a Re-Analysis p-value is 8.9% less likely to be statistically significant than an original publication p-value at the 5% level in the small window of $0.01 \leq p \leq 0.09$. Standard errors clustered at the paper level. Estimated using probit, with marginal effects presented.

Table 22: Caliper Tests, Significance at 10% Level

	Significant at 10% Level			
	(1)	(2)	(3)	(4)
Re-Analysis=1	-0.059	-0.064	-0.085	-0.132
	(0.050)	(0.059)	(0.062)	(0.095)
Observations	766	590	410	192
Threshold	0.10	0.10	0.10	0.10
Window	0.04	0.03	0.02	0.01

Notes: The dependent variable takes a value of one if $p \leq 0.10$. The variable Re-Analysis takes a value of one if the p-value is associated with a re-analysis, and zero if it is associated with the original publication. Standard errors clustered at the paper level. Estimated using probit, with marginal effects presented.

Table 23: Caliper Tests, Significance at 1% Level

	Significant at 1% Level			
	(1)	(2)	(3)	(4)
Re-Analysis=1	0.040	0.027	0.028	0.016
	(0.030)	(0.028)	(0.029)	(0.023)
Observations	4,261	4,034	3,783	3,392
Threshold	0.01	0.01	0.01	0.01
Window	0.04	0.03	0.02	0.01

Notes: The dependent variable takes a value of one if $p \leq 0.01$. The variable Re-Analysis takes a value of one if the p-value is associated with a re-analysis, and zero if it is associated with the original publication. Standard errors clustered at the paper level. Estimated using probit, with marginal effects presented.

Table 24: Applying Andrews and Kasy (2019)

	μ	τ	df	$[0, 1.645]$	$(1.645, 1.96]$	$(1.96, 2.576]$
Original Analysis	0.0006	0.0024	1.2705	0.1716	0.3829	1.0740
Re-Analysis	0.0001	0.0000	1.2508	0.3102	0.7157	0.9481
Original Economics	0.0002	0.0011	1.1969	0.1522	0.3910	1.0556
Re-Analysis Economics	0.0000	0.0000	1.1676	0.3056	0.6592	0.9873
Original Political Science	0.0155	0.0254	2.1907	0.3078	0.3496	1.1846
Re-Analysis Political Science	0.0185	0.0404	3.1388	0.3984	0.9350	0.9363

Notes: An application of Andrews and Kasy (2019). The columns μ, τ, and df represent the model's estimated parameters (using an underlying t-distribution and symmetric sign probabilities). The fourth column $[0, 1.645]$ presents the relative publication probability for a t-statistic in the $[0, 1.645]$ interval compared to one in the reference interval of $(2.576, \infty)$.

B ONLINE APPENDIX B

B.1 List of Replication Reports

B.1.1 Replication Report

Title Original Study: Antinormative Messaging, Group Cues, and the Nuclear Ban Treaty

doi: https://doi.org/10.1086/714924, Journal of Politics

Abstract: Herzog, Baron, and Gibbons (2022) explore the effects of exposure to official elite rhetoric and group cues on public support against the international nuclear weapons prohibition norm. The authors find that elite cues, in particular security and institutional cues, increase individuals' opposition to the Treaty on the Prohibition of Nuclear Weapons (TPNW). However, elite cues do not seem to have an effect on changing individuals' broader attitudes towards nuclear weapons, as measured by individuals' existing opposition to nuclear arms. We replicate and expand the authors' methods and results to test the robustness of the effects found in the study. First, we reproduce the main finding using the authors' original data and method. We do not find any coding errors that undermine the authors' analysis or conclusions. Second, we test the robustness of the results by (1) using a different operationalization of party identity, and (2) calculating additional subgroup analysis for gender. We find no significant differences between our replicated and the original results, however females' support for the TPNW is more responsive to security cues, while males' support is more responsive to institutions cues.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/97.htm

Replication Package: https://osf.io/xbvzg/

Link to Original Authors' Response: https://econpapers.repec.org/paper/zbwi4rdps/98.htm

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml;jsessionid=5bc4beb0bd5aef9d7a5ba5284fc6?persistentId=doi%3A10.7910%2FDVN%2FGLT4FX&version=&q=&fileTypeGroupFacet=%22Text%22&fileAccess=&fileSortField=size

B.1.2 Replication Report

Title Original Study: Ascriptive Characteristics and Perceptions of Impropriety in the Rule of Law: Race, Gender, and Public Assessments of Whether Judges Can Be Impartial

doi: https://doi.org/10.1111/ajps.12599, American Journal of Political Science

Abstract: Ono & Zilis (2022) investigated the effects of ascriptive characteristics of US American judges, such as race, gender, and ethnicity, on citizens' perceptions of the judges' professional impropriety and bias in their rulings. They conducted two studies, comparing citizens' perceptions of different ascriptive characteristics and judgments about the judges' biases and the need for recusal from cases. They found that political and ideological predispositions shape perceptions of judicial impropriety. In this comment, we recode the analysis using a different software and conduct robustness checks. We were able to reproduce the main results.

Link to Full Report: https://osf.io/yf48r/

Replication Package: https://osf.io/yf48r/

Link to Original Authors' Response: No response.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/ZHOL6Y

B.1.3 Replication Report

Title Original Study: Assortative Matching at the Top of the Distribution: Evidence from the World's Most Exclusive Marriage Market

doi: https://doi.org/10.1257/app.20180463, American Economic Journal: Applied Economics

Report's Abstract: Using novel data on peerage marriages in Britain, I find that low search costs and marriage-market segregation can generate sorting. Peers courted in the London Season, a matching technology introducing aristocratic bachelors to debutantes. When Queen Victoria went into mourning for her husband, the Season was interrupted (1861–1863), raising search costs and reducing market segregation. I exploit exogenous variation in women's probability to marry during the interruption from their age in 1861. The interruption increased peer-commoner intermarriage by 40 percent and reduced sorting along landed wealth by 30 percent. Eventually, this reduced peers' political power and affected public policy in late nineteenth-century England.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/47.htm

Link to Replicators' Package: https://osf.io/pqsem/

Original Author's Response: "I now reviewed the report carefully and with interest, and I am glad to see that the authors succeeded in replicating all results and found no coding errors. I hope the replication package was clear and easy to work with. I am also happy to see that they performed several additional robustness checks and heterogeneity analysis, and that these show that the original estimates "are robust and are not significantly affected using these alternative specifications" (p. 1). Given this, and the replicators' conclusion that "the study's main findings demonstrate robustness and reliability" (p. 7), I think that there is nothing substantial for me to write in a response in the form of a discussion paper. This is because both the replication exercise and the additional analysis found no major issues in the original work to respond to. I would also like to thank the authors for the fairness and professionalism of their report, and also for the time and effort they put in producing it, from which I ultimately benefit — as it adds to the credibility of my original paper — as well as the profession as a whole benefits — as making replication exercises more common is important for economics.

Please let me know if I can be of any further assistance regarding this replication report in the future. I am at your or the authors' disposal, in case I can be of help in clarifying anything in the replication package or in the analysis of the original paper. As I stated above, I believe that increasing replication rates is important for our field, as it is making original datasets publicly available — even when, as in the case of my paper, the data collection is an important part of my contribution, and in this situation, many do not grant public access to the original data."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/140921/version/V1/view

B.1.4 Replication Report

Title Original Study: Black Workers in White Places: Daytime Racial Diversity and White Public Opinion

doi: https://doi.org/10.1086/716289, Journal of Politics

Report's Abstract: In this replication study, we revisit the main empirical claims of Hamel and Wilcox-Archuleta's (HW) 2022 study on the impact of daytime racial diversity on White Americans' voting behavior and racial attitudes. HW introduce a novel zip code level measure of racial diversity that accounts for the influx of Black workers during daytime, showing that conventional purely residential based measures often underestimate the true degree of experienced racial diversity. Using survey data from the CCES, their findings suggest a negative correlation between racial flux and White Americans' Democratic voting tendencies and a positive correlation with racial resentment and opposition to affirmative action, all while controlling for the residential share of Blacks in the zip code. We assess the replicability of these findings by: (1) replicating the main results using the provided replication code, (2) reconstructing the racial flux measure and survey from raw data, (3) conducting multiverse analyses, and (4) replicating the analysis using an alternative data source. Our replication validates the robustness and accuracy of HW's initial conclusions, emphasizing the role of daytime racial diversity in shaping White Americans' political and racial attitudes.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/61.htm

Link to Replicators' Package: https://osf.io/ue4pm/

Original Authors' Response: "We enjoyed reading the replication, and don't see a need to write a response.

Thank you for doing this important work."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml;jsessionid=da88db7b3367419a2d1a87a9e687?persistentId=doi%3A10.7910%2FDVN%2FFMOR6K&version=&q=&fileTypeGroupFacet=&fileAccess=&fileSortField=type

B.1.5 Replication Report

Title Original Study: Brahmin Left Versus Merchant Right: Changing Political Cleavages in 21 Western Democracies, 1948–2020

doi: https://doi.org/10.1093/qje/qjab036, Quarterly Journal of Economics

Report's Abstract: Gethin, Martínez-Toledano and Piketty (2022) analyze the long-run evolution of political cleavages using a new database on socioeconomic determinants of voting from approximately 300 elections in 21 Western democracies between 1948 and 2020. They find that, in the 1950s and 1960s, voting for the "left" was associated with lower-educated and low-income voters. After that, voting for the "left" has gradually become associated with higher-educated voters, while highincome voters have continued to vote for the "right". In the 2010s, there is a disconnection between the effects of income and education on voting. In this replication, we first conduct a computational reproduction, using the replication package provided by the authors. Second, we do a robustness replication testing to what extent the original results are robust to i) restricting the sample to "core" left and right parties, ii) analyzing the top 80% versus bottom 20%, iii) weighting by population, iv) dropping control variables, and v) using country fixed effects. The main results of the paper are found to be largely replicable and robust.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/19.htm

Link to Replicators' Package: https://osf.io/2hpeq/

Original Authors' Response: "Thank you for your mail and for your interesting report! We are happy to see that you were able to easily replicate our results and that our main conclusions were found to be largely robust. In this context, we do not think that an answer from our side would be particularly useful: we are happy with the report as it is.

Thank you for the very valuable work that your institute is producing in testing the replicability and robustness of published studies!"

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/XUSWG6

B.1.6 Replication Report

Title Original Study: Bubbles, Crashes, and Economic Growth: Theory and Evidence

doi: https://doi.org/10.1257/mac.20220015, American Economic Journal: Macroeconomics

Report's Abstract: Guerron-Quintana, Hirano, and Jinnai (2023) explore the short-, medium-, and long-run effects of financial bubbles on economic growth by way of a macroeconomic general equilibrium framework. In their model, a key theoretical result is that, in net terms, the "crowding in" of capital investment during a bubble ushers the economy onto a higher balanced growth path post-bubble than it was on pre-bubble (Figure 10), thus (seemingly) suggesting that economic bubbles are growth-enhancing. In turn, the main result of the paper is that this positive view of bubbles is a fallacy so long as the latter are recurrent, namely because a counterfactual economy in which bubbles never occur in the first place grows at a significantly faster pace (Figure 10). The reason for this is that the expectation of future bubbles stifles capital investment and, as such, reduces economic growth in the long run.

We successfully reproduce the paper's main figures using the original code provided in the replication package. Given the hard-coded nature of all empirical data used in the paper, most of our efforts are devoted to reproducing the employed empirical data itself and, in turn, conducting a direct replication with our own measures. Using various specifications of the HP filter, we are successful in qualitatively, but not quantitatively reproducing the paper's main time series (stock-market-to-GDP ratio). Nevertheless, even without updating the model's parameterization, the paper's main empirical findings (i.e. Figures 8-10) are largely robust to our own measure. In turn, we are successful in quantitatively reproducing the second key time series (credit-to-GDP ratio), albeit only with a highly unusual specification of the HP filter's smoothing parameter (10^{10} instead of 1600 for quarterly data). We find that, unlike in the case of the stock-to-GDP ratio, the paper's (auxiliary) findings are not robust to our own credit-to-GDP series

Link to Full Report: https://osf.io/d76tn/

Link to Replicators' Package: https://osf.io/d76tn/

Original Authors' Response: Provided a short response and answered a question. Waiting for their final response.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/173441/version/V1/view

B.1.7 Replication Report

Title Original Study: Campaign Contributions and Roll-Call Voting in the U.S. House of Representatives: The Case of the Sugar Industry

doi: https://doi.org/10.1017/S0003055422000466, American Political Science Review

Report's Abstract: In their study, Grier et al. (2023) explore the causal relationship between campaign contributions and roll-call voting. Their analysis focuses on the influence of campaign contributions on two specific anti-sugar votes conducted in 2013 and 2018. The authors identify a substantial increase in inflationadjusted sugar contributions from the sugar industry to incumbent politicians between these two voting events. The aim of our research is to replicate and validate the authors' main models. In addition to cross-platform replication, we conduct several robustness checks to further examine the reliability of their findings. These include (1) clustering the standard errors, (2) utilizing an Ordinary Least Squares (OLS) model instead of the authors' logistic regression, and (3) altering the dependent variable to represent the change in the vote from 2013 to 2018. Our results largely confirm the authors' findings and reveal additional insights regarding the money buys vote hypothesis.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/57.htm

Link to Replicators' Package: https://osf.io/4hjb9/

Original Authors' Final Response: "We thank the Institute for Replication for their diligent work replicating and performing some extensions to our 2023 APSR paper. Replication is an important and often undervalued work in the scientific process. Of course we are quite pleased to see that our results do replicate and that the extensions performed largely support the results and ideas we advanced in our paper. Keep up the good work!"

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/2IFZR9

B.1.8 Replication Report

Title Original Study: Can Information Reduce Ethnic Discrimination? Evidence from Airbnb

doi: https://doi.org/10.1257/app.20190188, American Economic Journal: Applied Economics

Report's Abstract: Laouénan & Rathelot (2022) investigate the mechanism underlying ethnic discrimination using self-collected panel data from Airbnb between 2014 and 2017. They find that hosts from minority groups charge 3.2% less than those from the majority group within the same neighbourhood. Using a theoretical framework, they estimate that the ethnic price gap vanishes as more information (reviews) become available conditional on observables. The point estimates for their main results are statistically significant at the 1% level. This finding suggests that ethnic discrimination is due to statistical discrimination rather than taste-based discrimination. First, we reproduce the original article's main findings using R, whereby the authors of the original article use STATA. We can reproduce the main findings in R except for a few marginal discrepancies at the second or third decimal place. Second, we extend two robustness analyses reported in the original article. These robustness analyses impose restrictions on the sample and these restrictions are not justified in the article. Once these restrictions are not imposed, the picture becomes more complex and the robustness analysis warrants more discussion. However, only a small fraction of the observations causes some ambiguity and there might be good reasons to impose restrictions. Transparently presenting the robustness analyses with and without restrictions, motivating the restrictions and discussing its implications for the main findings would have been desirable. Generally, the original article does a great job with regard to reproducibility by providing data, code and documentation that ease the reproduction of a complex analysis. We conclude that our reproduction and replication support the main findings of the original article.

Link to Full Report: https://osf.io/zn98a/

Link to Replicators' Package: https://github.com/TuanNguyen04/Replication_Airbnb

Original Authors' Response: The authors provided initial feedback which the replicators took it into account.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/120078/version/V1/view

B.1.9 Replication Report

Title Original Study: Can Technology Solve the Principal-Agent Problem? Evidence from China's War on Air Pollution

doi: https://doi.org/10.1257/aeri.20200373, American Economic Review: Insights

Report's Abstract: Greenstone et al. examine the effect of the introduction of automatic air pollution monitoring on the reporting of local air pollution in China. Using 654 regression discontinuity designs (RDDs) based on city-level variation in the day that monitoring was automated, they find an immediate and lasting increase of 35 percent in reported PM10 concentrations post-automation. Moreover, they find that automation's introduction increases online searches for face masks and air filters by 200 percent and 28 percent, respectively, using an RDD. Results are consistent when using an event study design. First, we were able to computationally replicate the results. Second, we find that results are robust to more flexible specifications of the weather variables, to re-constructed weather variables using the same matching procedure as the authors (i.e., closest station) and meteorological data with additional weather stations, to alternative construction of the weather variables using an inverse distance weighted approach of the surrounding weather stations, and to more flexible choices of fixed effects (up to the city level). Finally, we find limited evidence of discontinuity in objective measures of ground pollution (i.e., AOD) for a sub-sample using alternative weather variables. The estimate, however, is economically insignificant. Moreover, no discontinuity is observed in the full sample. Therefore, we believe this result does not invalidate the original study's findings.

Link to Full Report: https://osf.io/b7dn2/

Link to Replicators' Package: https://osf.io/m8hfr/?view_only=9f6632ec96c0451daf0f8889b9ad2b25

Original Authors' Response: Ongoing back and forth.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/125321/version/V1/view

B.1.10 Replication Report

Title Original Study: Can't We All Just Get Along? How Women MPs Can Ameliorate Affective Polarization in Western Publics

doi: https://doi.org/10.1017/S0003055422000491, American Political Science Review

Report's Abstract: We present a replication and extension of Adams et al. (2023), examining the influence of women Members of Parliament (MPs) on affective polarization. Conducted during the 2023 Montreal Replication Games, our analysis reaffirms the original findings through the authors' base R code and a tidyverse simplification. Our results highlight that the mitigating effect on polarization is predominantly observed among left-wing respondents, with null effects noted for centrist and right-wing parties. This discrepancy is attributed to left-wing parties' explicit commitment to gender equality. Further analysis reveals the study's robustness across different countries and years (1996-2007) while addressing data structure and imputation methods to ensure reliability. Our findings underscore the nuanced role of women MPs in political dynamics, particularly among left-wing voters, against democratic backsliding concerns.

Link to Full Report: https://osf.io/69px3/

Link to Replicators' Package: https://osf.io/69px3/

Original Authors' Response: Thank you for replicating our paper Can't We All Just Get Along? How Women MPs Can Ameliorate Affective Polarization in Western Publics (APSR 2023) as part of the Montreal Replication Games. We appreciate the attention to detail and rigor applied to the replication project. We are pleased that our initial results replicate well. We appreciate your robust approach to testing the stability of our findings using a country and year 'leave-one-out' cross-validation strategy. We also thank you for catching the coding error which dropped a handful of cases from the original analysis; we are glad that the results remain substantively the same when this error is corrected. We also are interested in the results from the extension you undertook, finding that our results are primarily driven by left-wing parties' supporters, in particular parties from the green, radical left and social Democratic parties. On the other hand, the point estimates are positive for all parties excepting the conservative and radical right parties, which can be expected to have the most conservative views on gender roles. We note that the authors' interpretation, that "the portion of women MPs affects the attitudes of left-wing voters and not the attitudes of the voters most likely to undermine democracy" is true, but that the results also suggest that far-right parties, who most aggressively challenge liberal democratic norms, may be able to "soften" their image among left-wing voters by running female candidates. This is consistent with the argument made by Catalano Week et al (2023), that radical right parties strategically run women to broaden their appeal. Again, we deeply appreciate your replication and insightful extension of our research.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/AHQRVR

B.1.11 Replication Report

Title Original Study: Changing Hearts and Minds? Why Media Messages Designed to Foster Empathy Often Fail

doi: https://doi.org/10.1086/719416, Journal of Politics

Report's Abstract: This paper focuses on computational reproducibility and robustness replicability of Gubler et al.'s(2022) studies which examine the effect of media messages on empathic concern, dissonance, and out-group policy attitudes. The original paper tests four hypotheses using two online experiments with large samples from one US state ($N1 = 5{,}800$; $N2 = 2{,}200$). Regarding the first experiment, we successfully reproduced the effect that initial antipathy weakens the effect of humanizing treatment on empathic concern (H1). However, we show that the moderating effect is negligible and has little practical significance. Moreover, the individual effect estimates in our analyses slightly differed from the original paper due to different procedure of data cleaning and minor coding errors in the original paper. The most relevant difference was the opposite effect of gender than reported in the original paper. We also show that empathic concern might mediate the effect of humanizing treatment on attitudes toward immigrants (H3). The original study rejected the mediation hypothesis due to not finding a total effect of humanizing treatment on attitudes. In contrast, we found that humanization treatment has a positive indirect effect on attitudes through empathic concern. At the same time, it also has a direct negative effect on attitudes. For the second experiment (H1, H2a, H2b, H3), we attempted to reproduce the results using a different software. We partially succeeded once receiving support from the authors of the original study. We note throughout the report issues we have encountered.

Link to Full Report: https://repec.econ.muni.cz/mub/wpaper/wp/econ/WP_MUNI_ECON_2024-02.pdf

Link to Replicators' Package: See Report's Online Appendix for the codes.

Original Authors' Response: Ongoing back and forth.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/FUCDTT

B.1.12 Replication Report

Title Original Study: Changing Tides: Public Attitudes on Climate Migration

doi: https://doi.org/10.1086/715163, Journal of Politics

Report's Abstract: See entry below.

Link to Full Report: https://www.socialsciencereproduction.org/reproductions/791/published/index

Link to Replicators' Package: https://github.com/alexkustov/Replication-of-Arias-and-Blair-2021

Original Authors' Response: "Thank you very much for reaching out! We are very pleased to hear that the results of our study were replicated, and do not need to provide an answer."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml;jsessionid=e19065118cc12d43f1b412109d41?persistentId=doi%3A10.7910%2FDVN%2FFDML2N&version=&q=&fileAccess=&fileTag=&fileSortField=name&fileSortOrder=desc

B.1.13 Replication Report

Title Original Study: Checking and Sharing Alt-Facts

doi: https://doi.org/10.1257/pol.20210037, American Economic Journal: Economic Policy

Report's Abstract: Henry, Zhuravskaya, and Guriev (2022) examine whether people are willing to share "alternative facts" espoused by right-wing populist parties before the 2019 European elections in France and how this interacted with the availability of fact-checking information. They find that both imposed and voluntary fact-checking reduce the likelihood of sharing false statements by approximately 45%, and that imposed and voluntary fact-checking have similar effect sizes. We reproduce these findings and introduce several alternative estimates to assess the robustness of the original results, including resolving an inconsistency in the handling of pre-treatment controls. Overall, our results align with the results of the original paper. The differences we find are small in absolute magnitude but, since many effects were small, not always trivial in terms of relative differences. This replication supports the conclusions of the original paper.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/34.htm

Link to Replicators' Package: https://doi.org/10.5281/zenodo.7858829

Link to Original Authors' Response: "Many thanks! No, we won't be writing a response."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/140161/version/V1/view

B.1.14 Replication Report

Title Original Study: Child Marriage Bans and Female Schooling and Labor Market Outcomes: Evidence from Natural Experiments in 17 Low- and Middle-Income Countries

doi: https://doi.org/10.1257/pol.20200008, American Economic Journal: Economic Policy

Report's Abstract: By studying child marriage bans in 17 developing countries, Wilson (2022) finds that raising the minimum legal age of marriage to 18 successfully increased the age at first marriage, the age at first birth, and the likelihood of employment. Additionally, the bans reduced child marriage and increased educational attainment in urban areas. We replicate these findings by collecting the raw data from the same sources as the paper and analysing the data following the procedures described in the paper, without referring to the data and codes provided by the author. Our findings are consistent with the results of the paper in terms of the statistical significance of point estimates and differ in magnitude by a negligible amount.

Link to Full Report: https://osf.io/5yhxc/

Link to Replicators' Package: https://livewarwickac-my.sharepoint.com/personal/u2084980_live_warwick_ac_uk/_layouts/15/onedrive.aspx?id=%2Fpersonal%2Fu2084980%5Flive%5Fwarwick%5Fac%5Fuk%2FDocuments%2FAAA%20Warwick%20University%2FReplication%20Games%2FWilson%282022%29%2DReplicationCodes%2DShared&ga=1

Original Authors' Response: We could not reach out the author.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/130784/version/V1/view

B.1.15 Replication Report

Title Original Study: Concentration Bias in Intertemporal Choice

doi: https://doi.org/10.1093/restud/rdab043, Review of Economic Studies

Report's Abstract: Dertwinkel-Kalt et al. (2022) examine the effect of concentration bias - the tendency to overweight advantages that are concentrated in time relative to costs that are spread over multiple time periods - on intertemporal choice in a laboratory experiment. In their preferred empirical specification, the authors report that concentration bias leads to a 22.4% higher willingness to work than explained by a standard model of intertemporal discounting. We conduct a computational replication of the main results of the paper using the same procedures and original data. Our results confirm the sign, magnitude and statistical significance of the author's reported estimates across each of their five main findings.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/42.htm

Link to Replicators' Package: https://osf.io/d42xr/

Original Authors' Response: "We thank Deer, Ellingsrud, Heuer, and Kordt (2023) for conducting the replication report and appreciate that their "results confirm the sign, magnitude and statistical significance of [our] reported estimates across each of [our] five main findings" (p. 1). We don't have anything substantive to add to this. "

Original Authors' Package: https://zenodo.org/records/5091975

B.1.16 Replication Report

Title Original Study: Cooperative Property Rights and Development: Evidence from Land Reform in El Salvador

doi: https://doi.org/10.1086/717042, Journal of Political Economy

Report's Abstract: Montero (2022) explores a discontinuity in a land reform in El Salvador and reports two main findings. First, relative to outside-owned haciendas operated by contract workers, the productivity of worker-owned cooperatives is higher for staple crops and lower for cash-crop. Second, cooperative property rights increase workers' incomes and compress wage distributions. In this comment, we show that the latter result rests on two mistakes: three-quarters of the observations are duplicates and income inequality is calculated over too few workers to be meaningful. When corrected, the data sources and research design provide no credible evidence regarding the causal effects of ownership structure on income levels and inequality.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/20.htm

Link to Replicators' Package: https://doi.org/10.7910/DVN/AMD3NO

Link to Original Authors' Response: https://www.journals.uchicago.edu/doi/10.1086/725234

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/717042/suppl_file/20190161data.zip

B.1.17 Replication Report

Title Original Study: Decentralization Can Increase Cooperation among Public Officials

doi: https://doi.org/10.1111/ajps.12606, American Journal of Political Science

Report's Abstract: Molina-Garzón, Grillos, Zarychta, and Andersson (2022) examine how health sector decentralization affects cooperation between public officials. Using a public goods game conducted in Honduras, they find that officials who work under decentralized regimes contributed 0.8 more lempiras per round to a group solidarity fund, compared to officials who work under centralized regimes. They also find that most of this increase in investment under decentralized regimes occurred during rounds of the game in which the participants were able to communicate with each other. Finally, they find that decentralization was associated with a 14 percentage point increase in the proportion of potential cross-level network ties between participants that were realized. In this paper, I examine whether these results are robust to (1) the omission of some individual-level controls that may have been affected by the decentralization treatment, and (2) the use of a linear regression model instead of a Poisson regression model for the network analysis. I find that omitting the individual-level controls leads to similar conclusions about the effect of decentralization on individual contributions in the public goods game, but the interaction effect between decentralization and communication becomes statistically insignificant at the 0.05 level. For the network analysis, I find that using a linear regression instead of a Poisson regression has little bearing on the magnitude of the effect of decentralization on the proportion of ties realized, though the effect of decentralization becomes statistically insignificant for one version of the network model.

Link to Full Report: https://osf.io/q3dpt/

Link to Replicators' Package: https://osf.io/q3dpt/

Link to Original Authors' Response: https://osf.io/q3dpt/

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/ZLHYSZ

B.1.18 Replication Report

Title Original Study: Declining Worker Turnover: The Role of Short-Duration Employment Spells

doi: https://doi.org/10.1257/mac.20190230, American Economic Journal: Macroeconomics

Report's Abstract: Using a Diamond-Mortensen-Pissarides (DMP) model with noisy signals on worker-firm match quality calibrated on data from 30 US states for 1999 and 2017, Pries and Rogerson argue that improved screening may explain the decrease in short-term employment spells observed in the US labor market. Using a decomposition exercise in a "reduced form" model, the authors show that changes in short-term employment spells (and) are almost entirely accounted for by changes in the rate of learning on match quality and in the probability of a good match . Then, using a decomposition exercise in a "structural" model, they show in their main calibration strategy that changes in and are mainly driven by changes in and , parameters pertaining to learning about match quality. First, we reproduce the authors' codes in R and Python, two popular free open source programming languages. We find identical results to the paper. Second, we test the robustness of results to (1) using an earlier starting year, (2) adding additional states in the analysis, and (3) increasing the value of the 1999 mean vacancy duration parameter. The direction and relative size of the effect of each parameter on and is preserved in all robustness tests, corroborating the authors' argument.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/93.htm

Link to Replicators' Package: https://github.com/AlexandrePavlov/PriesRogerson2022Replication

Original Authors' Response: Declined to respond.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/120568/version/V1/view

B.1.19 Replication Report

Title Original Study: Digital Addiction

doi: https://doi.org/10.1257/aer.20210867, American Economic Review

Report's Abstract: Using an original economic model of digital addiction and a randomized experiment, Hunt Allcott, Matthew Gentzkow, and Lena Song (2022) isolate the effect of habit formation and self-control problems on how people use their smartphones. They find a persistent effect of temporary incentives on reducing social media usage. With the model-free results, the study shows that (after the incentive was in effect), participants in the bonus group reduced use by 56, 19 and 12 minutes in periods 3, 4 and 5,respectively, suggesting a persistent effect. But before the incentive was in effect in period 2, social media use reduced use by 5.1 minutes per day. Participants who used the limit functionality reduced FITSBY use by over 20 minutes per day, suggesting an impact of self-control problems on social media use. All these estimates are statistically significant. We perform a direct replication of the paper. Upon re-calculating the core dependent variable (FITSBY use by period), we find a small but concerning discrepancy: For a small number of observations, the aggregated dependent variable does not equal the sum of the disaggregated categories. Thankfully, this discrepancy does not have a major effect on the results. Using the provided data, we re-coded the core figures from scratch and found that we could replicate them all. We also compare the pre-analysis plan (PAP) with the main study to identify gaps and perform computational reproduction/replication of the structural model and model-free analysis. We only find minor differences between the PAP and the main paper, almost all of which are acknowledged in the paper.

Link to Full Report: https://osf.io/8kvdf/

Link to Replicators' Package: https://osf.io/8kvdf/

Link to Original Authors' Response: https://osf.io/8kvdf/

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/163822/version/V2/view

B.1.20 Replication Report

Title Original Study: Do Thank-You Calls Increase Charitable Giving? Expert Forecasts and Field Experimental Evidence

doi: https://doi.org/10.1257/app.20210068, American Economic Journal: Applied Economics

Report's Abstract: Samek and Longfield estimate the effect of 'thank you calls' on the extensive and intensive margins of subsequent donations. Based on a series of experimental interventions, the authors find no statistically discernable effect of thank-you calls on either the likelihood of donating again, or on the size of any subsequent donations made within the period of the study. In a companion exercise the researchers quantify the ability of experts in charitable fundraising and non-experts (using the Understanding America Survey) to predict the behaviours elicited by the experiment. Experts and non-experts (incorrectly) make the same predictions of an increase to the extensive margin of donation behaviour induced by the thank you call, and while both groups overestimate the intensive margin, the non-experts overestimated by a smaller magnitude. We were able to reproduce the papers findings completely, discovering only one difference in an appendix table related to the average gift amount — treatment for experiment 1 where only the constant term of the regression was affected. Upon careful examination of the code we found a few small errors that did not affect the results (one of the errors in the code did not seem to be carried through and used anywhere). Finally, we conducted several extensions of the original analysis which demonstrated that the findings are robust to heterogeneity of treatment effect by initial donation size, as well as different specifications of the regression analysis.

Link to Full Report: To be made available soon. Waiting for the authors' response.

Link to Original Authors' Response: Waiting for the authors' response.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/149481/version/V1/view

B.1.21 Replication Report

Title Original Study: Do Transitional Justice Museums Persuade Visitors? Evidence from a Field Experiment

doi: https://doi.org/10.1086/714765, Journal of Politics

Report's Abstract: Balcells et al. (2022) explore the effect of transitional justice museums through a field experiment in Santiago, Chile, and attendance at the government's remembrance museum, the Museum of Memory and Human Rights which looks at the time of Pinochet's dictatorship. The authors want to understand how such experiences shape an individual's perceptions of trust in government institutions, and transitional justice policies, and how they are affected emotionally. Additionally, they seek to measure how long they last over time. They do this by creating treatment (museum attendance) and control (non-attendance) groups and administering pre-and post-treatment surveys and estimating the 'complier average causal effect' (CACE). They find that satisfaction with the current government significantly increases for the treatment group, looking over the entire population ($= 0.15, p = .04$) as measured with a 4-point Likert scale and support for a military government significantly drops by 11% ($= 0.11, p = .002$) across ideological stances. We first reproduce their results and find no major coding errors. Second, we test the robustness of the effects by 1) testing for heterogeneous effects by gender, 2) we combine the emotion variables into two indices, a mobilization and demobilization index, and 3) conduct a causal mediation analysis to see how confidence in the church may mediate effects found in the study.

Link to Full Report: https://osf.io/m3hwg/

Link to Replicators' Package: https://osf.io/m3hwg/

Original Authors' Response: "We thank all involved for their interest in our work. We are happy to hear that the results from our paper successfully replicated. We are intrigued by the additional analyses performed by the replicators. We hope their insights and results can inform future theorizing and empirical studies of the impact of Transitional Justice."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml;jsessionid=a651e0dbc9a5c140152aa84be2e0?persistentId=doi%3A10.7910%2FDVN%2FTNFDDX&version=&q=&fileTypeGroupFacet=&fileAccess=Public&fileSortField=type

B.1.22 Replication Report

Title Original Study: Does Competence Make Citizens Tolerate Undemocratic Behavior?

doi: https://doi.org/10.1017/S0003055422000119, American Political Science Review

Report's Abstract: We replicate the analysis conducted by Frederiksen, 2022a. We focus on assessing the computational and robustness replicability of their work. We find that their main exhibits and supplementary analysis are replicable, both when running their original Stata replication package, and when we attempt to replicate their findings from scratch in R. We also conduct additional robustness checks by estimating additional specifications and by subsetting the dataset by the time taken by the respondent to complete the survey. We again find that their work is robust to our battery of alternative specifications.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/28.htm

Link to Replicators' Package: https://github.com/tjbrailey/nottingham_replication_2023

Link to Original Authors' Final Response: "Thanks a lot for this initiative and not least for replicating my results."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/NGFLRO

B.1.23 Replication Report

Title Original Study: Does Patient Demand Contribute to the Overuse of Prescription Drugs?

doi: https://doi.org/10.1257/app.20190722, American Economic Journal: Applied Economics

Report's Abstract: We replicate Lopez et al.'s (2022) study on gatekeeping costs and the potential evidence for patient-driven and doctor-driven demand. Using their publicly available source materials, we first re-run their analysis "as is" to see if their results can be exactly replicated. We then expand the analysis to include patients previously excluded for not being acutely ill, offering a broader perspective on medication demand among all patient types. The findings confirm Lopez et al.'s results.

Link to Full Report: https://osf.io/x7g9z/

Link to Replicators' Package: https://osf.io/x7g9z/

Link to Original Authors' Response: Provided feedback to an initial report. Final response: " Thank you very much for sharing the updated report. We appreciate that the authors of the replication reworked the paper and have no further response or comments."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/126722/version/V1/view

B.1.24 Replication Report

Title Original Study: Does Public Opinion Affect the Preferences of Foreign Policy Leaders? Experimental Evidence from the UK Parliament

doi: https://doi.org/10.1086/719007, Journal of Politics

Report's Abstract: The study by Chu and Recchia (2022) tests the hypothesis that providing public opinion information can shift policymakers' opinions in the direction of what the public favors. They surveyed 101 British Members of Parliament (MPs) about their views regarding the United Kingdom's presence in the South China Sea. Their results demonstrated that MPs who received information about the public opinion poll expressed viewpoints closer to that of public opinion. The authors reported an effect that is "substantively meaningful and statistically significant at the .10 level." Our computational replication of the original study found that the paper is fully computationally reproducible. We successfully replicated the authors' results but found that the main findings are no longer significant when analyzed using unweighted data (see Table 1). We also conducted several robustness checks on sub-samples of the data to examine the key analyses both with and without weights. Here, we found that the results are once again robust and significant when weights are used, but no longer significant when weights are not used. As a further robustness check, we found no moderating effect of gender. Overall, our replication efforts suggest that the main finding of the original study may be sensitive to the use of survey weights.

Link to Full Report: https://osf.io/bqz6w/

Link to Replicators' Package: https://osf.io/vwt2n/?view_only=84e52a7c684942a4880410b3c89ff4c6

Original Authors' Response: " Thank you for your note and engaging with our work. We don't have a formal reply, though this is an honest question: isn't it standard practice to use weights when using YouGov's data, since making valid claims about representativeness depends on using their weights? YouGov's MP panels operate similarly to their public opinion poll, in that their claims to representativeness rely on using weights, provided by YouGov. I [Chu] think your write-up mentioned that there's a debate about using weights, and cited MTurk data, but I think that MTurk is quite different, and yes, I agree I do not use weights for MTurk data except unless requested by a reviewer for robustness checks, etc.. But I don't think MTurk and the MP representative poll we used is a good comparison in the context of evaluating the validity of weighting. In any case, happy to adapt if there is a clear consensus on this. Thanks again."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/BNINNL

B.1.25 Replication Report

Title Original Study: Effective for Whom? Ethnic Identity and Nonviolent Resistance

doi: https://doi.org/10.1017/S0003055421000940, American Political Science Review

Report's Abstract: Manekin and Mitts (2022) investigate the success chances of minority ethnic groups when engaging in non-violent protests demanding political change. First, using observational data, the authors find that the success rate for nonviolent campaign tactics is lower for excluded/minority ethnic groups than for non-excluded/majority ethnic groups. Second, the authors use two original survey experiments to show that non-violent protest by ethnic minorities is perceived as more violent and requiring more policing than identical protest by majorities. This report reproduces the paper computationally and conducts several sensitivity analyses for both the observational and the experimental parts of the paper. We can confirm the general direction of the postulated effects, but evidence becomes less consistent (effect magnitudes and significance levels are not robust to some of the changes).

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/86.htm

Link to Replicators' Package: https://zenodo.org/records/10193470

Original Authors' Response: Cannot provide a response.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/SHHVCA

B.1.26 Replication Report

Title Original Study: Enabling or Limiting Cognitive Flexibility? Evidence of Demand for Moral Commitment

doi: https://doi.org/10.1257/aer.20201333, American Economic Review

Report's Abstract: We computationally reproduce Saccardo and Serra-Garcia (2023) where subjects exploit cognitive flexibility by viewing their incentives first and partially ignoring product quality information, and hence, recommend the incentivized product. We find one major coding error for the variable Selfishness. Additionally, two of the "moral cost" questions more likely capture spitefulness. After correcting the erroneous coding or dropping the two questions, we find stronger support for the authors' main conclusion regarding Selfishness driving incentive information avoidance with double effect size. Finally, we find weak evidence that subjects update their posterior beliefs differently depending on the product they are incentivized to recommend.

Link to Full Report: https://osf.io/nwds7

Link to Replicators' Package: https://osf.io/yfdet/

Link to Original Authors' Response: https://www.aeaweb.org/doi/10.1257/aer.20201333.appx

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/180741/version/V1/view

B.1.27 Replication Report

Title Original Study: Entertaining Beliefs in Economic Mobility

doi: https://doi.org/10.1111/ajps.12702, American Journal of Political Science

Report's Abstract: In Entertaining Beliefs in Economic Mobility (AJPS 2023) Kim finds that watching "rags-to-riches" style reality TV programs strengthens Americans' belief in the American dream. Through thoughtful and clever experimental and observational analysis, she demonstrates that exposure to television programs containing everyday people working hard to earn large prizes increases Americans' belief that success can be internally attributed and that economic mobility is possible. We computationally replicate Kim's results, finding no major errors in her coding or statistical procedure. We also include several robustness checks. First, we merge her two experimental samples, which increases the precision of her main quantity of interest such that it attains conventional levels of statistical significance. Second, we recreate tables and visualizations for alternative specifications of her main observational results. The original results are robust to these alternative models, but we do find that if sports programming is operationalized in the same manner as "rags-to-riches" programming, the sign, magnitude, and significance of watching either programming type are similar. We also uncover a partisan interaction effect, as only Democrats change their beliefs in economic mobility with increased TV viewing.

Link to Full Report: https://osf.io/xf5w2/

Link to Replicators' Package:

Original Author's Response: "Thanks for this! I have no particular response per se. I'm grateful for your collective efforts to make social science much more transparent."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/FVRZYU

B.1.28 Replication Report

Title Original Study: Evaluating Deliberative Competence: A Simple Method with an Application to Financial Choice

doi: https://doi.org/10.1257/aer.20210290, American Economic Review

Report's Abstract: Ambuehl et al. (2022) explore ways to evaluate interventions designed to enhance decision-making quality when individuals misjudge the outcomes of their choices. The authors propose a novel outcome metric that can distinguish between interventions better than conventional metrics such as financial literacy and directional behavioral responses. The proposed metric, which transforms price-metric bias into interpretable welfare loss measures, can be applied to evaluate various training programs on financial products. Table 4 of the paper reports the authors' significant main point estimates at the 1% level. In this replication exercise, we first replicate the main findings of the original paper. Then, we modify the clustering method by using k-means with demographic variables as inputs, then we re-calculate standard errors with jackknife estimators. Finally, we include subjects who were excluded by the authors due to multiple switching in the multiple price lists. We find that all of these replications result in robust findings. Additionally, we successfully replicate Figure 4 from the paper. Notably, this replication demonstrates the insensitivity of the results to the choice of distance metric.

Link to Full Report: https://osf.io/scgbt/

Link to Replicators' Package: https://osf.io/scgbt/

Link to Original Authors' Response: Authors provided feedback.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/171681/version/V1/view

B.1.29 Replication Report

Title Original Study: Exposure and Preferences: Evidence from Indian Slums

doi: https://doi.org/10.1111/ajps.12570, American Journal of Political Science

Report's Abstract: Successful computational reproducibility. The replicators could not conduct the robustness checks without the help of the author.

Link to Full Report: No report.

Original Author's Response: "Thanks for your email. I am not interested in participating."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/AV8PLT

B.1.30 Replication Report

Title Original Study: Finance and Green Growth

doi: https://doi.org/10.1093/ej/ueac081, Economic Journal

Report's Abstract: De Haas and Popov (2023) estimate the effect of country-level financial sector size and structure on decarbonization to show that countries with relatively more equity versus debt financing have more emission-efficient economies. We uncover multiple coding errors that change the magnitude and the precision of the coefficients of interest. These coding errors include misreporting of standard errors, and misspecifying generalized method of moments (GMM) estimators. We further provide robustness tests of the results to (1) restricting the sample to consistent sets of countries across the country and country-by industry samples, and (2) using a limited information maximum likelihood (LIML) estimator to address a weak-instrument problem. We find that the results from the robustness checks are qualitatively different from the original results but similar to the corrected results.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/95.htm

Link to Replicators' Package: https://osf.io/h8ct2/

Link to Original Authors' Response: https://econpapers.repec.org/paper/zbwi4rdps/96.htm

Original Authors' Package: https://zenodo.org/records/7220094

B.1.31 Replication Report

Title Original Study: Flight to Safety: COVID-Induced Changes in the Intensity of Status Quo Preference and Voting Behavior

doi: https://doi.org/10.1017/S0003055421000691, American Political Science Review

Report's Abstract: Bisbee and Honig (2022) examine the effect of the COVID-19 pandemic on voting for Bernie Sanders in the 2020 Democratic Party primary using a difference-in-differences design, finding evidence that exposure to COVID-19 resulted in a 7-15 percentage point increase in voting for Biden. The study also uses a regression design with district-level fixed effects to estimate the effect of the COVID-19 pandemic on voting for anti-establishment candidates during the US 2020 House primaries. It finds evidence that an increase in COVID cases was associated with a decline in voting for anti-establishment candidates in general, and for those endorsed by the Tea Party. We re-run the code for all tests in this paper, successfully reproducing its results in a preliminary replication. We then use the De Chaisemartin and D'Haultfoeuille difference-in-differences estimator to replicate their main results, finding that though the coefficient remains negative, the results are not statistically significant. We also replicate their tests regarding US House primary candidates using a different measure of anti-establishment candidates. Here, we find that the interaction term between anti-establishment candidates and COVID-19 remain statistically significant, with the same sign. Finally, we employ an expanded dataset that includes Congressional primary candidates that were omitted in the initial dataset, as well as a re-coded extremism variable that also includes candidates endorsed by Donald Trump. These updated findings corroborate the paper's initial results. However, due to a restrictive number of observations that interfered with our application of the De Chaisemartin and D'Haultfoeuille estimator, we believe that the expanded U.S. House primary results constitute the more robust half of our replication.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/36.htm

Link to Replicators' Package: https://github.com/Dmscates/Bisbee-and-Honig-2022-Flight-to-Safety-Replication

Original Authors' Response: "You guys are amazing. Thank you for doing this! We are impressed by your rigor and grateful for the introduction to DCD'H DiD estimator that we'll have to add to the repertoire. We were working on the conditional accept when the flurry of generalized DiD work (Goodman-Bacon, Callaway, etc.) was blowing up [...] We also appreciate the manner in which you communicated with us during the course of your re-analysis, and the thoughtfulness of your report. [...] Although I'm sure it is a logistical nightmare and likely would add even more delays to the publication pipeline, it would be very pro-science if this type of replication were part of a journal's own pre-publication process. (This is what I naively thought replication meant back when I got my first publication, and have been disappointed in the process ever since.)"

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/S5YMS7

B.1.32 Replication Report

Title Original Study: Gender Differences in Cooperative Environments? Evidence from The U.S. Congress

doi: https://doi.org/10.1093/ej/ueab069, Economic Journal

Report's Abstract: Gagliarducci and Paserman (2022) study gender differences in cooperative behavior among politicians using information from the U.S. House of Representatives between 1988 and 2010 on (i) the number of co-sponsors on bills and (ii) the share of co-sponsors from the rival party. Through different empirical strategies, they show that women-sponsored bills tend to have more co-sponsors, but the gap is only statistically significant among Republicans. Moreover, Republican women recruit a significantly larger share of co-sponsors from the rival party than Republican men, whereas the opposite is true among Democrats. GP argue that the observed pattern is consistent with a commonality of interest driving cooperation, rather than gender per se, since during this period Republican women were ideologically closer to the rival party than their male colleagues, while female Democrats were further away. We examine the robustness of these findings to (i) the correction of some errors in two control variables of the dataset used by GP and (ii) clustering the standard errors at the individual level, instead of individual-term. These changes have a relatively minor impact on results: most coefficients are still statistically significant and the main conclusions from the analysis are confirmed. Furthermore, we extend the analysis to the 2011-2020 period. The analysis of gender differences in bipartisan cooperation confirms GP's hypothesis that ideological distance plays an important role. However, results are slightly different when we analyze overall cooperation. The gender gap in favor of women is larger in magnitude than in GP and it is statistically significant in several specifications, providing support for the hypothesis that gender also matters for cooperation.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/75.htm

Link to Replicators' Package: https://www.dropbox.com/scl/fo/dmvgx5wlgql3tz98dx47u/h?rlkey=af83xiqrkw70rbjicdunoach9&dl=0

Link to Original Authors' Response: https://osf.io/w48tf/

Original Authors' Package: https://zenodo.org/records/5111360

B.1.33 Replication Report

Title Original Study: Good Reverberations? Teacher Influence in Music Composition since 1450

doi: https://doi.org/10.1086/718370, Journal of Political Economy

Report's Abstract: Borowiecki (2022) studies the influence of teachers on the style of their students in the domain of musical composition. The author finds that realized student-teacher pairs are on average 0.2-0.3 standard deviations more similar to unrealized, but possible, student-teacher pairs. In this report we provide the results of our replication of Borowiecki (2022). We direct our attention to the following tasks: 1) Replicating the outcome variables used in the paper, starting from the raw data, and generating alternative measures of similarity between students and teachers 2) Testing the validity of the random teacher-student pairing, a key assumption for the validity of the estimation strategy employed in the paper. We can replicate most of the outcome variables, but not all of them, due to incomplete raw data. Our alternative measures of similarity confirm the robustness of the original results. We find significantly different characteristics between paired and unpaired students, suggesting that matching between students and teachers does not occur randomly. However, controlling for these characteristics in the main regressions leads to quantitatively similar results to the ones reported in the original paper.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/27.htm

Link to Replicators' Package: https://www.dropbox.com/scl/fo/6hecmgjsq3mjo5ekkv8lm/h?rlkey=ftuoe4mf5f9jon0hiabb4brtn&dl=0

Link to Original Authors' Response: https://osf.io/79e2z/

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/718370/suppl_file/20210405data.zip

B.1.34 Replication Report

Title Original Study: Hate Crimes and Gender Imbalances: Fears over Mate Competition and Violence against Refugees

doi: https://doi.org/10.1111/ajps.12595, American Journal of Political Science

Report's Abstract: Dancygier et al. (2022) ascribe anti-refugee hate crime in Germany from 2015 to 2017 to the fear of mate competition felt by native German men, amplified by growing refugee populations and existing gender gaps. In a replication of this article, we discovered that the substantively and statistically significant relationship between perceptions of mate competition and support for anti-refugee violence found in a 2016–17 survey of adults in Germany were robust when analyzed with ensembles of regression trees permitting arbitrary interactions in a large design matrix. However, statistically significant pairwise comparisons between survey respondents' perceptions of mate competition across strata of the municipality-level gender gap as recorded by German censuses were not robust to controlling the family-wise Type I error rate. Moreover, statistically significant relationships between the gender gap and the incidence of hate crime in Germany in the authors' panel regressions vanished in a wide range of specifications with municipality fixed effects—in certain cases, being replaced with statistically significant estimates of the opposite sign.

Link to Full Report (and Initial Version of the Report): https://osf.io/5n3ds/

Link to Replicators' Package: https://osf.io/5n3ds/

Link to Original Authors' Response: https://osf.io/5n3ds/

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/QXJDJ5

B.1.35 Replication Report

Title Original Study: Historical Lynchings and the Contemporary Voting Behavior of Blacks

doi: https://doi.org/10.1257/app.20190549, American Economic Journal: Applied Economics

Report's Abstract: Williams (2022) ties the political participation of Blacks to historical lynchings that occurred in the United States. Her findings document lower Black voter registration rates in southern counties with greater number of historical lynchings. We show that this effect is driven by four outlier counties with relatively high Black lynching rates. Excluding these counties from the analysis yields a point estimate that is no longer statistically significant. Dropping the ninety-fifth percentile lynching rates and correcting the errors in voter registration rates rule out the effect size reported by Williams (2022), which now becomes close to zero and statistically insignificant. We also show that the main results are highly sensitive to the way lynching and voter registration rates are measured.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/32.htm

Link to Replicators' Package: https://osf.io/hv7wp/

Original Author's Response: No response to emails.

Original Author's Package: https://www.openicpsr.org/openicpsr/project/136741/version/V1/view

B.1.36 Replication Report

Title Original Study: Hobo Economicus

doi: https://doi.org/10.1093/ej/ueab103, Economic Journal

Report's Abstract: Peter Leeson, August Hardy and Paola Suarez (2022) test maximizing behaviour of panhandlers at several Metrorail stations in Washington, D.C. Their main findings are that "stations with more panhandling opportunities attract more panhandlers" (the first statement) and that "cross-station differences in hourly panhandling receipts are statistically indistinguishable from zero" (the second statement). We test computational reproducibility and robustness replicability of their results. We can reproduce both statements, in Stata and R. Our robustness replications for the first statement confirm the authors' results in the vast majority of cases (replication was successful in 91% of the cases). Our robustness replications for the second statement might raise doubts on this finding. We run weighted ANOVA tests, we change the bounds in minutes used by authors by 5 minutes in their robustness checks, we run Bartlett's tests of equality of variances of means, and run pair-wise tests of equality of means. In three out of four cases we cannot replicate the results, and the differences (of either means, medians or variances of donations) across Metrorail stations are statistically different from zero. We hypothesize that panhandlers have a general idea about which stations have more passers-by, and will rationally go more often there. However, they are unlikely to have information about smaller variations in the number of passers-by (e.g., variations in passers-by at the same station over time due to non-public events), and therefore might find it difficult to perfectly maximize donations.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/55.htm

Link to Replicators' Package: https://osf.io/s4bca/

Original Authors' Response: Declined to respond.

Original Authors' Package: https://zenodo.org/records/5719541

B.1.37 Replication Report

Title Original Study: How Do Beliefs about the Gender Wage Gap Affect the Demand for Public Policy?

doi: https://doi.org/10.1257/pol.20200559, American Economic Journal: Economic Policy

Report's Abstract: We conduct a replication of Settele (2022), a online survey experiment designed to find out how individual's beliefs about the gender wage gap affect their policy preferences. We reproduce Results 1 and 2 of the study: how prior beliefs around the wage gap are distributed among individuals and how a information treatment causally affects the policy demand. Our re-coded replication shows that the reported results are robust.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/12.htm

Link to Replicators' Package: https://osf.io/j2ubt/

Original Authors' Response: "I very much appreciate the effort of you and your team to replicate not just my paper, but many others too. It is quite impressive to see the scope of your project and I am curious about your future plans with this initiative.

I just read the replication report for my paper and I think it is great. In particular, Figures 2 and 3 are really cool. (They are actually new, and offer a really insightful way of looking at the data. I should have come up with them myself!)

Regarding your question, I don't think the replication report requires a formal response from my side. I fully agree with the authors' interpretation of the results, and just want to say thank you for their great work. Please go ahead and publish the report whenever it suits you."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/134041/version/V1/view

B.1.38 Replication Report

Title Original Study: How Effective Are Monetary Incentives to Vote? Evidence from a Nationwide Policy?

doi: https://doi.org/10.1257/app.20200482, American Economic Journal: Applied Economics

Report's Abstract: Successful computational reproducibility. No re-analyses conducted.

Link to Original Authors' Response: Not contacted.

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/720458/suppl_file/20190733data.zip

B.1.39 Replication Report

Title Original Study: How Much Should We Trust the Dictator's GDP Growth Estimates?

doi: https://doi.org/10.1086/720458, Journal of Political Economy

Report's Abstract: In this brief commentary, we have conducted a robustness reproducibility and replicability of Martinez's 2022 paper entitled "How much should we trust the dictator's GDP growth estimates?" by selecting different clusters and omitting fixed effect terms. Concurrently, we conduct sub-sample analyses and employ alternative measurements for the sake of robustness and direct replicability. Our results are generally robust, yet they also raise some intriguing questions. First, we attempt to remove the year fixed effect in the model specifications, but the elimination of the year fixed effect from the baseline equation did not account forunobserved variables across year, suggesting the variable bias by Oster (2019). Second, the entirety of the baseline results is influenced by the periods 2007-2013 (for a five-year interval) and 2010-2013 (for a three-year interval). Third, when utilizing a more varied dataset for the autocracy measurement, the effect vanished for countries that are partially unfree.

Link to Full Report: https://osf.io/4sk52/

Link to Replicators' Package: https://osf.io/4sk52/

Original Author's Final Response: "As before, please extend my gratitude to the replicators for their thoughtful work and for taking into consideration my previous comments. I am reassured by the fact that they were able to replicate all the original results in the paper. I also find it reassuring that the results prove robust to additional robustness tests concerning alternative clustering structures for the standard errors or alternative data sources on political regimes (albeit with some loss of precision). The heterogeneous effects by subperiod are also quite intriguing and potentially reflect changes in the geopolitical incentives to overstate GDP growth in non-democracies. My paper is certainly not the final word on this topic and these results could be the first step towards new and exciting research."

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/720458/suppl_file/20190733data.zip

B.1.40 Replication Report

Title Original Study: Ideological Asymmetries and the Determinants of Politically Motivated Reasoning

doi: https://doi.org/10.1111/ajps.12624, American Journal of Political Science

Report's Abstract: Guay and Johnston (2022) examine asymmetric politically motivated reasoning on the part of liberals and conservaites. In our replication of the paper we examine four potential issues with the analysis: confounding in the numeracy task, heterogeneity across ideological constraints, the use of control variables, and heterogenity in the moderator index items. None of these potential issues are in fact issues. The results are quite robust. We found only one minor issue with the codebook, which does not affect the results.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/79.htm

Link to Replicators' Package: https://osf.io/mh5sk/

Link to Original Authors' Response: "Thank you again for examining our paper so closely [...] we changed the codebook and appreciate this replication effort."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/CGHTPZ

B.1.41 Replication Report

Title Original Study: Immigration and Redistribution

doi: https://doi.org/10.1093/restud/rdac011, Review of Economic Studies

Report's Abstract: Alesina et al. (2023) examine how people perceive the number and characteristics of migrants and how those perceptions affect their support for redistribution. They find that respondents from the United States, United Kingdom, Sweden, Italy, Germany and France markedly overestimate the share of immigrants in each country, with the average respondent in all countries except Sweden overestimating by more than a factor of two. We reproduce these results using the original code and data and test the robustness by (i) including participants excluded for time to complete the survey, (ii) extending the analysis of misperceptions to all survey respondents, and (iii) using alternative authoritative estimates of the proportion of immigrants. We find that these checks marginally change the estimates of the size of the misperception but do not change the conclusions to be drawn from the analysis. Alesina et al. (2023) also test the effect on support for redistribution of showing videos on immigrant characteristics. We computationally reproduced the treatment effects on support for redistribution.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/40.htm

Link to Replicators' Package: https://osf.io/ajm9g/

Original Authors' Response: "Dear Institute for Replication team,

Thank you very much for taking the time to replicate our paper. We appreciate the important work you do. We are happy to see that our results replicated well and that our robustness checks were confirmed.

With best wishes, Armando Miano and Stefanie Stantcheva"

Original Authors' Package: https://zenodo.org/records/5997521

B.1.42 Replication Report

Title Original Study: Indecent Disclosures: Anticorruption Reforms and Political Selection

doi: https://doi.org/10.1111/ajps.12646, American Journal of Political Science

Report's Abstract: This short report summarises a replication exercise performed on data from Szakonyi (2021). The original work applies a difference-in-differences design to the case of an anti-corruption reform implemented in Russia for local election candidates, mandating financial disclosures. The author applies this design by comparing the electoral outcomes of municipalities that happened to hold elections right after the reform with those that held elections right before the reform. For both groups, the design uses information from the previous electoral cycle as a pre-treatment benchmark. Using only data provided by the author in the original dataset, I first verified that results are reproducible when using alternative software. Second, I performed two simple placebo tests to obtain evidence on violations of the design's identifying assumptions. These placebo tests return null results, reassuring on the reproducibility of the original findings.

Link to Full Report: https://osf.io/gx4d6/

Link to Replicators' Package: https://osf.io/gx4d6/

Original Author's Response: "Many thanks to the replicator for taking the time to replicate and extend the paper. The placebo tests are very helpful in illuminating whether the identifying assumptions hold. I will make sure to run versions of these in future analyses."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/KDUMRM

B.1.43 Replication Report

Title Original Study: Inflammatory Political Campaigns and Racial Bias in Policing

doi: https://doi.org/10.1093/qje/qjac037, Quarterly Journal of Economics

Report's Abstract: Grosjean et al. (2023) (GMY2023) estimate the causal effect of a Trump rally on the number of black drivers stopped by police officers, using a difference-in-difference approach. In their preferred specification, the authors find that after a Trump rally, the probability that a stopped driver is black increases by 5.74%. This effect is significant at the 1% level. In this report we focus on reproducing the main claim of the paper. First, we reproduce the paper's main findings and uncover an issue with counties that experience multiple Trump rally treatments, given the original modelling choices taken in GMY2023. When we remove counties that experience multiple rallies, the estimated effect size drops to 2.46% and loses statistical significance. Second, we attempt to conduct a direct replicability check, by employing a new data set as a source for the dependent variable. We use data from the National Incident Based Reporting System (NIBRS). We observe no effect of Trump rallies both on the original data, covered by NIBRS and on the NIBRS data. Third, we conduct a robustness replicability exercise by coding an event-study difference-in-difference design at the day level. We estimate the event-study in a $[7; +7]$ window. We do not discover any systematic effect of Trump rallies on the dependent variable from GMY2023.

Link to Full Report: https://osf.io/xadb6/

Link to Replicators' Package: https://osf.io/c7j58/

Original Authors' Response: Provided comments, but declined to respond.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/A3B9HE

B.1.44 Replication Report

Title Original Study: Interaction, Stereotypes, and Performance: Evidence from South Africa

doi: https://doi.org/10.1257/aer.20181805, American Economic Review

Report's Abstract: Corno, La Ferrara and Burns (2022) exploit the random allocation of freshman roommates in a large South African university to gauge the impact of a roommate's race on racial attitudes as measured by an implicit association test, and on school performance. They notably find that (a) white students randomly assigned to black roommates have less negative racial stereotypes, and (b) black students randomly assigned to live with white students have higher GPAs. We first reproduce all regression tables in Corno et al. (Corno et al. (2022)), and then test for robustness by varying the controls and conducting influential analysis. Overall, we find the results for finding (a) and (b) and robust in 15% and 40% of the robustness checks we ran, and the t/z scores are on average 78% and 85% as large as the original study.

Link to Full Report: To be made available soon. Ongoing back and forth with the authors.

Link to Replicators' Package: https://osf.io/kr9nx/

Link to Original Authors' Response: Ongoing back and forth with the replicators.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/174501/version/V1/view

B.1.45 Replication Report

Title Original Study: Interventions and Cognitive Spillovers

doi: https://doi.org/10.1093/restud/rdab087, Review of Economic Studies

Report's Abstract: In the paper of, Altmann et al. (2022) the authors investigate whether positive effects which are due to behavioral policy interventions in policytargeted domains come along with negative effects in policy non-targeted domains. Using lab and online experiments where subjects have to solve one policy-focused decision task and one non-focused background task, the authors show that increasing incentives or steering attention to the former led to higher attention spans, lower default adherence rates, and a higher choice quality in the decision task. However, because of steering participants focus to the decision task, lower choice quality and lower attention spans in the background task emerged as a consequence, which was particularly pronounced among individuals with lower cognitive capabilities and complex decision tasks. Essentially, the authors also describe that the negative effects in the background tasks offset the positive effects in the decision task, ultimately yielding a net-zero effect overall. Therefore, the authors emphasize policymakers to also consider the potential negative cognitive spillovers in order to not overestimate the benefits of behavioral policy interventions. All the results the authors in the main text report are significant on 5% and 1% significance levels. All findings presented in the main text of the paper can be replicated using the original Stata code and verified thoroughly using R. Additionally, we performed two robustness tests to ensure the reliability of the paper's main results, and they remained consistent. Hence, the reported findings in the paper appear to be robust.

Link to Full Report: https://www.econstor.eu/bitstream/10419/272845/1/I4R-DP043.pdf

Link to Replicators' Package: https://osf.io/kugbs/

Original Authors' Response: "We do not have any comments regarding the replication. We just want to briefly express our gratitude for the thorough and excellent work of the authors of the replication study."

Original Authors' Package: https://zenodo.org/records/5652808

B.1.46 Replication Report

Title Original Study: Jumping the Gun: How Dictators Got Ahead of Their Subjects

doi: https://doi.org/10.1093/ej/ueac073, Economic Journal

Report's Abstract: Hariri and Wingender add new nuance to the traditional wisdom that economic modernisation is a path to democracy. They show that the diffusion of repressive, military technologies, causes a decline in the number of democratisations in the following years, and argue that this is because of a greater ability to forcefully oppress popular dissent. We conduct a robustness replication exercise, focussed on three tests: i) Are findings robust to alternative weightings of individual technologies in the instrument for country-aggregate military technology? ii) Is high leverage in individual countries, regions or time periods driving the global findings? iii) Are the strength of the IV and its independence of important macroeconomic indicators a chance occurrence? The main findings of the paper are largely robust to these tests.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/50.htm

Link to Replicators' Package: https://osf.io/4cx86/

Original Authors' Response: "we do not have any comments to the replication report, so I'm just sending you this email to applaud the initiative. You and the Institute for Replication is doing a great service to the profession."

Original Authors' Package: https://zenodo.org/records/7077694

B.1.47 Replication Report

Title Original Study: Liquidity Constraints in the U.S. Housing Market

doi: https://doi.org/10.1093/restud/rdab063, Review of Economic Studies

Report's Abstract: Successful computational reproducibility. No re-analyses conducted.

Link to Original Authors' Response: Authors provided feedback and suggestions.

Original Authors' Package: http://doi.org/10.5281/zenodo.5112964

B.1.48 Replication Report

Title Original Study: Local Elites as State Capacity: How City Chiefs Use Local Information to Increase Tax Compliance in the Democratic Republic of the Congo

doi: https://doi.org/10.1257/aer.20201159, American Economic Review

Report's Abstract: Balán et al. (2022) evaluate the impact of "local elites" involvement in local tax collection in a large city in the Democratic Republic of Congo. Using a randomized controlled trial to vary the identities of tax collectors, they find that local elites' involvement raises tax compliance and total revenue by 50 and 44 percent, respectively. The paper argues that the primary mechanism behind the results is better targeting made possible by local elites' superior information about property holders' willingness and ability to pay. In this replication comment, we first reproduce the paper's main results. Then, we assess the robustness of the results by (1) employing randomization inference for statistical tests; (2) controlling for baseline characteristics that are not balanced; and (3) using an alternative method to examine the claims supporting the preferred mechanism of better targeting. We find robust estimates in (1). However, the results are less robust both in terms of statistical significance and magnitude for (2) and (3). We conclude that the average treatment effect is robust, while the main claim about mechanisms, the information channel, is less robust to alternative estimation approaches. We contextualize and discuss the significance of these results, including the negligible revenue potential even under full compliance.

Link to Full Report: Final report to be made available shortly. Ongoing back and forth between authors and replicators.

Link to Replicators' Package: https://github.com/SossouAdjisse/LocalTaxReplicationProject.git

Link to Original Authors' Response: Ongoing back and forth between authors and replicators.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/147561/version/V1/view

B.1.49 Replication Report

Title Original Study: Major Reforms in Electricity Pricing: Evidence from a Quasi-Experiment

doi: https://doi.org/10.1093/ej/ueab076, Economic Journal

Report's Abstract: Labandeira et al. (2022) examine the effect of a policy in Spain that modified the electricity bill structure for all Spanish households. The policy simultaneously increased fixed costs and decreased marginal costs on household electricity bills. Using fixed effects and instrumental variables (IV) specifications, the main causal finding in the paper is that the reform reduced house- hold electricity consumption for Spanish households by 15%. Their point estimate is statistically significant at the 1% level. In a similar specification, they find the reform reduced household expenditures on electricity by 9.8%, statistically significant at the 1% level. The code provided by the authors is computationally reproducible. We found two coding errors in different IV specifications, which had served as robustness checks to their main results. Correcting the errors removes statistical significance in two of four IV results, but increases the point estimates and statistical significance in the other two IV results. We also perform robustness checks. The IV estimates lose statistical significance in two of four robustness checks (with point estimates changing 1.1% to -39%). However, the OLS regressions are robust to changing covariates (sign and significance remained for 12 of 14 tests of the OLS specification, with changes in the estimates ranging from -157% to 64%, but averaging -3.3%).

Link to Full Report: https://osf.io/bysa7/

Link to Replicators' Package: https://osf.io/bysa7/

Original Authors' Response: Original authors provided feedback. Multiple rounds of back and forth with replicators.

Original Authors' Package: https://zenodo.org/records/5423782

B.1.50 Replication Report

Title Original Study: Market Access and Quality Upgrading: Evidence from Four Field Experiments

doi: https://doi.org/10.1257/aer.20210122, American Economic Review

Report's Abstract: Bold et al. (2022b) investigate the effect of providing access to a market (i.e. a buyer) which rewards quality with a premium on farm productivity and farming incomes from smallholder maize farmers in western Uganda, using a series of randomized experiments and a difference-in-differences approach. We successfully reproduce the results of this study using the publicly provided replication packet. Then test the robustness of these results by re-defining treatment and outcome variables, testing for model misspecification and the leverage of outliers, and testing for non-random selection in the Fisher-permutation process. Our results show that the findings in Bold et al. (2022b) are robust to a variety of decisions in the research process. This evokes confidence in the internal validity of the findings.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/72.htm

Link to Replicators' Package: https://journaldata.zbw.eu/dataset/bold-et-al-american-economic-review-2022

Original Authors' Response: "Thank you very much for sharing the report (and taking the time to replicate the study). We have no comments."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/158401/version/V1/view

B.1.51 Replication Report

Title Original Study: Market-Based Monetary Policy Uncertainty

doi: https://doi.org/10.1093/ej/ueab086, Economic Journal

Report's Abstract: Bauer et al. (2022) derive market-based monetary policy uncertainty and uncover an 'FOMC uncertainty cycle' characterized by a fall of uncertainty after FOMC announcements and its subsequent built-up. Then, the authors show that the financial markets' response to monetary policy announcements depends on the level of short-rate uncertainty on the day before the FOMC announcement. First, we reproduced the paper's findings, though with Matlab version-specific issues. Second, we tested the robustness of the two main results of the paper. We show that the uncertainty cycle in the monetary policy uncertainty is confirmed when the crisis period is included in the sample or when the median instead of the average of changes in the monetary policy uncertainty is considered. However, the FOMC uncertainty cycle does not appear when the monetary policy uncertainty index (Husted et al. 2020) or the daily economic policy uncertainty index (Baker et al. 2016) are used as uncertainty proxies.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/77.htm

Link to Replicators' Package: https://osf.io/qx8aw/

Original Authors' Response: "Thank you, glad to see that this work found our results to be rock solid!

We won't write a response. Do let us know if you have any other questions about our work."

Original Authors' Package: https://zenodo.org/records/5566246

B.1.52 Replication Report

Title Original Study: Market-Based Monetary Policy Uncertainty

doi: https://doi.org/10.1093/ej/ueab086, Economic Journal

Report's Abstract: This report replicates and examines Bauer et al.'s (2021) paper on monetary policy transmission to financial markets. The paper introduces novel measures of monetary policy uncertainty and analyses its drivers. It also investigates the impact of uncertainty changes on interest rates and financial asset prices. We assess reproducibility, consolidate market uncertainty measures using PCA and Factor Analysis, and rigorously test the reduction of uncertainty after Federal Market Open Committee (FOMC) announcements. Our findings support the paper's claim of reduced uncertainty on meeting days. Additionally, we explore the implications of the uncertainty channel on various financial assets, such as Gold, the Swiss Franc, European stock indexes, and Bitcoin.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/76.htm

Link to Replicators' Package: https://github.com/YaolangZhong/Nottingham_Replication_Game/tree/main/replication_code

Original Authors' Response: "Thank you, glad to see that this work found our results to be rock solid!

We won't write a response. Do let us know if you have any other questions about our work."

Original Authors' Package: https://zenodo.org/records/5566246

B.1.53 Replication Report

Title Original Study: Measuring the Welfare Effects of Shame and Pride

doi: https://doi.org/10.1257/aer.20190433, American Economic Review

Report's Abstract: This replication report examines and extends the research conducted by Butera, Metcalfe, Morrison, and Taubinsky (2022) on "The Welfare Effects of Pride and Shame." The original paper explores the welfare implications of public recognition as a motivator for desirable behavior and introduces an empirical methodology to measure Public Recognition Utility (PRU), which quantifies the utility individuals experience when their actions are publicly recognized. This report focuses on the real effort experiment reported in the paper that was conducted using a classroom sample, a lab sample, and an online sample. I computationally reproduce the original results and verify their robustness. While reproducing the results, I found two minor coding errors in the replication package. Correcting these errors slightly changes some estimates reported in the paper but does not turn over any results. The main treatment effect findings are further robust to using different sets of controls and sample selection criteria. Moreover, I conduct a heterogeneity analysis which reveals significant variations in how participants value public recognition. Overall, the replication study confirms the original conclusions while providing additional insights into the heterogeneity of PRU shapes on an individual level.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/64.htm

Link to Replicators' Package: https://github.com/tilmanfries/welfare-shame-pride-replication-report

Original Authors' Final Response: "Thanks again for all your hard work on this."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/145141/version/V1/view

B.1.54 Replication Report

Title Original Study: Mental Health Costs of Lockdowns: Evidence from Age-Specific Curfews in Turkey

doi: https://doi.org/10.1257/app.20200811, American Economic Journal: Applied Economics

Report's Abstract: This report presents a replication of Altindag et al. (2022) performed at the Olso Replication Games in 2022. Altindag et al. (2022) estimate the effects of an age-specific lockdown on mental health outcomes and mobility among adults aged 65 and older in Turkey, using a regression discontinuity design. The authors find a decline in mobility with a one-day decrease in the number of days being outside and an increase in the probability of never going out by 30 percentage points. These point estimates are statistically significant at the 1% level. The mobility restrictions lead to a worsening in mental health outcomes of approximately 0.2 standard deviations, statistically significant at the 10% level in their preferred specification. In this paper we accomplish two things. First, we successfully reproduce Altindag et al.'s main findings. Second, we test the ro-bustness of the results to a small number of changes to their preferred estimations by (1) not clustering the standard errors on the running variable, (2) not including control variables, and (3) calculating the optimal bandwidth using another technique. Point estimates for mobility outcomes are stable to all three manipulations, and standard errors only change marginally. Point estimates and standard errors for the mental health outcomes are somewhat more sensitive, especially to changing the optimal bandwidth selection method. However, the observed changes are reason-ably expected when applying data-driven model selection methods to noisy data (to avoid over-fitting, it is likely preferable to apply a less data-driven approach like the original authors did). Our general impression is that the original analyses and results are both theoretically plausible and credible, despite some defensible model dependencies.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/16.htm

Link to Replicators' Package: https://osf.io/25u7b/

Link to Original Authors' Response: https://econpapers.repec.org/paper/zbwi4rdps/17.htm

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/131981/version/V1/view

B.1.55 Replication Report

Title Original Study: Mortality, Temperature, and Public Health Provision: Evidence from Mexico

doi: https://doi.org/10.1257/pol.20180594, American Economic Journal: Economic Policy

Report's Abstract: Cohen and Dechezleprêtre (2022) investigate the heterogeneous impact of temperature on mortality across Mexico, and how affordable healthcare services that target the low-income population attenuate the mortality effects of weather events. They find that while extreme temperatures are more dangerous than less extreme temperatures, the increased frequency of non-extreme temperatures mean these temperatures cause more deaths. First, we reproduce the paper's main findings, uncovering a minor coding error that has a trivial effect on the main results. Second, we test the robustness of the results to clustering at the state level, omitting precipitation, and using a different weighting scheme. The original results are robust to all of these changes.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/90.htm

Link to Replicators' Package: https://osf.io/q52e4/

Original Authors' Response: Cohen: "We thank The Institute for Replication. Next time, I will make sure I do not forget Feb. 29th in the code!"

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/125201/version/V1/view

B.1.56 Replication Report

Title Original Study: Motivated Beliefs and Anticipation of Uncertainty Resolution

doi: https://doi.org/10.1257/aeri.20200829, American Economic Review: Insights

Report's Abstract: Drobner (2022) examines the effect of manipulating experimental subjects' expectations about uncertainty resolution in learning about their performance on their belief updating patterns in an ego-relevant domain. In their preferred empirical specification, the author finds that individuals update their beliefs optimistically as they exhibit a higher belief adjustment in response to good compared to bad news only when they do not expect resolution of underlying uncertainty about their performance in an IQ test and neutrally when they know they will find out their relative performance at the end of the experiment. First, we reproduce the all of the paper's findings without identifying any coding errors. Second, we test the robustness of the results to (1) adding individual covariates and (2) excluding subjects who exhibit a fundamental error in their belief updating from the analysis. We find no substantial changes in the main coefficients of interest with the inclusion of demographic variables in the analysis, consistent with demonstrated balance in covariates between the two experimental groups. Yet, several of the main estimates lose statistical significance and change from conservatism (under-updating) to over-inference (over-updating) in some conditions on the subset of participants excluding those who exhibit fundamental errors in belief updating.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/65.htm

Link to Replicators' Package: https://osf.io/evt3a/

Original Authors' Response: "Thanks for sharing the report. I think it's a great initiative and feel free to publish this report on your webpage. I will not be able to provide an "answer"."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/139262/version/V1/view

B.1.57 Replication Report

Title Original Study: Multinationals' Sales and Profit Shifting in Tax Havens

doi: https://doi.org/10.1257/pol.20200203, American Economic Journal: Economic Policy

Report's Abstract: We perform a robustness replication analysis of Laffitte and Toubal (2022), which considers how multinational corporations shift profit to "tax havens", jurisdictions where they face lower tax burdens. We find that the main results of Laffitte and Toubal (2022), are fairly robust to alternative versions of three important researcher choices: i) the definition of tax havens; ii) the use of a continuous measure of tax-friendliness rather than a binary classification of tax havens; and iii) a sample that omits two small but "extreme" tax havens: Bermuda and Barbados. In all cases, results remain of the same sign and retain statistical significance, though the magnitudes are somewhat attenuated in our robustness exercises.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/37.htm

Link to Replicators' Package: https://osf.io/3sbmr/

Original Authors' Response: "Thanks for your email and for replicating our exercise. Your work is useful. We recognize that the results remain consistent even when considering different interpretations of the haven concept and a smaller sample of observations.

We are also pleased to hear that the replication file we shared with the AEJ: Policy has proven helpful."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/148301/version/V1/view

B.1.58 Replication Report

Title Original Study: Multiracial identity and political preferences

doi: https://doi.org/10.1086/714760, Journal of Politics

Report's Abstract: The growing concern regarding reproducibility and replicability of social science re- sults has powered the adoption of open data and code requirements at journals and norms among researchers. However, even when these norms and requirements are fol- lowed, changes to the software used in data cleaning and analysis can render papers non-reproducible. This paper details the challenges of reproducibility in the face of software updates. We present a case study of a published article whose results are no longer reproducible due to changes in the software used. We then discuss the tools and techniques researchers can use to ensure that their research remains reproducible despite changes in the software used.

Link to Full Report: https://osf.io/ecymu/

Link to Replicators' Package: https://github.com/taylorjwright/r_and_p_versioning

Original Authors' Response: Ongoing back and forth with the authors.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/BLVJJH

B.1.59 Replication Report

Title Original Study: News Shocks Under Financial Frictions

doi: https://doi.org/10.1257/mac.20170066, American Economic Journal: Macroeconomics

Report's Abstract: Görtz et al. (2022) estimate the effects of innovations to future total factor productivity (TFP) on financial markets. In a Bayesian vector autoregression, they identify a TFP news shock as one that explains the largest share of 40- quarter ahead forecast error variance (FEV) of TFP. Their estimated impulse responses functions show that a positive news shock significantly decreases credit market spreads and increases credit market supply. They also find that a shock that explains the maximum of the FEV of the "excess bond premium" (EBP) (Gilchrist and Zakrajsek 2012) causes similar responses. These results are consistent with an estimated DSGE model with financial frictions. We estimate the main IRFs of the study using the original data and a frequentist estimation approach. We obtain similar point estimates for the dynamic responses to TFP news and EBP max-share shocks. We also update their macroeconomic and financial time series, as some of the data has been revised substantially since their original estimate. We use the updated data to re-estimate the above-mentioned IRFs, and we find that the results are robust to this change in the data. Finally, we investigate the computational reproducibility of their DSGE results, and find that their provided code (consistent with warnings in their README file) does not execute in the most recent version of Dynare or Matlab. Using the version indicated in their replication files, we encounter issues estimating the posterior mode.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/51.htm

Link to Replicators' Package: https://github.com/gionikola/replication-game-ucsd

Original Authors' Final Response: "Thank you for the update and considering our work for replication."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/130141/version/V1/view

B.1.60 Replication Report

Title Original Study: Non-Linearities, State-Dependent Prices and the Transmission Mechanism of Monetary Policy

doi: https://doi.org/10.1093/ej/ueab049, Economic Journal

Report's Abstract: Ascari and Haber (2022) fill the gaps in the literature by showing evidence in favor of the state-dependent sticky price model's predictions using the macro-aggregates. We report a replication and robustness check of the study. We employ several additional macroeconomic control variables and different alternative measurements for monetary policy shocks and find that the original results remain qualitatively robust. Our analysis further shows that the turbulent periods of inflation in the 1970s and 1980s have an important role in claiming the robustness of the original results.

Link to Full Report: https://osf.io/kbwap/

Link to Replicators' Package: https://osf.io/kbwap/

Original Authors' Response: No response.

Original Authors' Package: https://oup.silverchair-cdn.com/oup/backfile/Content_public/Journal/ej/132/641/10.1093_ej_ueab049/1/ueab049_replication_package.zip?Expires=1704922076&Signature=LWVbDI23c3c8fbYXPnP1BoN9pWtEiXHPj5BnotI-VjHHz7LhS23a~cm37gqC~5XTf~PntqGRlMOySoOD5Y9MH-cq8ScUPMoEhbPoQGqmbeZpbPmto6siB3LZRg3sEYqHO196h2Awonsc0vctdkzGqH3JHt~luPUe1mS6z2oHqMirnzzcN~578kQ8lT2kv7-lNVsqB6xwPocgbZq4WJpS07-Q4fp-r3IDXGsvZlGQRYxEJZ65yqEf~teKjFjFhNxeYI8w~~WuJOKQzSMUs82yAmFPA9lVzELymhr37M9lREz9-OycBI~4sPQ8MA0b4jQ9oPk~M4qBGRqcFIyc1tVy6g__&Key-Pair-Id=APKAIE5G5CRDK6RD3PGA

B.1.61 Replication Report

Title Original Study: Not All Elections Are Created Equal: Election Quality and Civil Conflict

doi: https://doi.org/10.1086/714778, Journal of Politics

Report's Abstract: Utilizing a time-series cross-sectional dataset on developing countries, Donno et al. (2022) examine how variation in election quality shapes opportunities and incentives for civil conflict. Across a number of models in their analysis, they find that civil conflict is more likely when elections are not free and fair. They also find that for countries with low integrity elections, the probability of conflict occurring is higher if it has experienced conflict before. We begin by reproducing Donno et al.'s (2022) main models and findings, which yielded no coding errors or differences in effect estimates. Afterwards, for replication purposes we run a series of robustness and conceptual replication tests. For our first replication, we examine the heterogeneous effect between electoral integrity and ethnic fractionalization on conflict. Our second test examines whether a subsample of authoritarian regimes should have been included in the authors' original analysis.

Link to Full Report: https://osf.io/unhkr/

Link to Replicators' Package: https://drive.google.com/drive/folders/1Vlwfr3_Q7c56XFPsQuKUNSnEU_lMFUQz?usp=sharing

Link to Original Authors' Response: https://osf.io/unhkr/

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/8B31FG

B.1.62 Replication Report

Title Original Study: Parties as Disciplinarians: Charisma and Commitment Problems in Programmatic Campaigning

doi: https://doi.org/10.1111/ajps.12638, American Journal of Political Science

Report's Abstract: Hollyer, Klašnja, and Titiunik (2022) analyse the trade-off that political parties face between running programmatic campaigns and fielding charismatic candidates, whose electoral appeal may come at the cost of undermining the party brand. They argue that higher electoral volatility prompts parties to rely on charismatic candidates, even though they might not be as loyal to the party's programmatic stance. They substantiate their argument with a cross-national dataset and a quantitative case study in Brazil. We computationally reproduced and conducted further robustness tests for their cross-national study by translating the Stata code to R. Next, we conducted a computational reproduction and some additional robustness tests for the quantitative case study. We find that their cross-national analysis is reproducible, albeit with some minor discrepancies. The quantitative case study is also largely reproducible and both are robust in several ways. We conclude by making some suggestions about data dissemination and robustness checks for authors of regression discontinuity designs.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/54.htm

Link to Replicators' Package: https://osf.io/93gfx/?view_only=7063353244d646ffaf7bfd53013e3143

Original Authors' Response: "Thanks for your note and for all the work of Kelly, Odermatt, and Metson in replicating our paper. [...] Our read of the Replication Reports that the findings in our paper hold in the Kelly et al replication. [...] Our sense is that the discrepancies between the replication and original paper are sufficiently small, and the task of comparing the replication R code to the original Stata code is likely to be sufficiently demanding of time, that the opportunity cost of a thorough response is high. So, I think we'll forgo the opportunity to draft a response, and just let the replication stand without reply.

We'll leave it to any sufficiently interested parties with expertise in both Stata and R to iron out the discrepancies between the replication and original paper."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/AWSQTW

B.1.63 Replication Report

Title Original Study: Patience, Risk-Taking, and Human Capital Investment Across Countries

doi: https://doi.org/10.1093/ej/ueab105, Economical Journal

Report's Abstract: Hanushek et al. (2021) test how country-level measures of patience and risk-taking from the Global Preference Survey predict student performance on the Programme for International Student Assessment (PISA) math test. They find that country-level patience positively predicts math test scores and country-level risk-taking negatively predicts math test scores. They find similar results when holding country of residence characteristics constant and focusing on the preferences of the country of origin of migrants. We have checked the computational reproducibility and find that the data and analysis script provided by the authors allowed us to exactly reproduce the main tables in the paper. We also checked the robustness replicability by testing how robust the results are to decisions about imputation, weighting, operationalization of dependent variables, choice of control variables, and the inclusion of highly leveraged observations. We see that results are generally robust, though statistical significance of the risk-taking coefficient in the migrant analysis hinges on whether a control for OECD country of residence is included. Finally, we check the conceptual replicability of the results by using data from the Trends in International Mathematics and Science Study (TIMSS) instead of PISA - a different dataset with a different standardized test. This exercise shows that their results are robust to expanding the analysis to countries participating in both PISA and TIMSS.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/48.htm

Link to Replicators' Package: https://osf.io/kgt8z/

Link to Original Authors' Response: "We would like to thank the replicators and compliment them for their thoughtful replication and extension of our paper. We are particularly impressed by the extension to the TIMSS data, which is actually great support for the underlying idea. We do not see a reason to formulate a formal response for your website.

Thank you all for your valuable work!"

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/153101/version/V2/view

B.1.64 Replication Report

Title Original Study: Peer Effects in Academic Research: Senders and Receives

doi: https://doi.org/10.1093/ej/ueac031, Economical Journal

Report's Abstract: In this report, we provide an overview from reproducing and replicating Bosquet et al. (2022). As a first step, we successfully reproduce all the results in the paper, as well as figure A1. All results were fully reproducible and match the published version of the paper. Next, we carry out three sensitivity analysis. We examine how the main results change from the weights used, additional controls, and author-university pairs. The main results are robust to these checks.

Link to Full Report: https://osf.io/czkgw/

Link to Replicators' Package: https://osf.io/czkgw/

Link to Original Authors' Response: The authors responded to the replicators' questions. Bosquet then responded to the final report: "I would simply thank the team of replicators and I am happy to see that the tested results are robust to the tested alternatives. As written in my previous email, I think those kinds of efforts are very useful for the community and the credibility of published results so thanks as well for that."

Original Authors' Package: https://zenodo.org/records/6457037

B.1.65 Replication Report

Title Original Study: Playing Politics with Environmental Protection: The Political Economy of Designating Protected Areas

doi: https://doi.org/10.1086/718978, Journal of Politics

Report's Abstract: Mangonnet et al. (2022) examine whether political alignment at the national and sub-national levels explain the spatial designation of Protected Areas (PAs) in Brazil. Their identification relies on spatial discontinuities in political alignment across municipalities. They find that a president-mayor coalition alignment reduces the incidence of PAs by about one percentage point, whereas they find no party alignment effects. We were able to reproduce the paper's findings using the same code and software. Alternative software routines reproduce their results with small and inconsequential numerical differences. Moreover, robustness replications find consistent results for one out the two treatments. Finally, we find no evidence of fabrication of data.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/73.htm

Link to Replicators' Package: https://osf.io/t76jd/

Original Authors' Response: "We are grateful to Laura Villalobos, Jill Caviglia-Harris, Tharaka Jayalath, and the team at the Institute for Replication for generously replicating our work. We encourage readers to follow their alternative software routines for faster estimations."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/N6LIMH

B.1.66 Replication Report

Title Original Study: Policy Deliberation and Voter Persuasion: Experimental Evidence from an Election in the Philippines

doi: https://doi.org/10.1111/ajps.12566, American Journal of Political Science

Report's Abstract: I would characterize my robustness replication as almost entirely successful. The design checks I report all support a straightforward understanding of the design. My effect and uncertainty estimates barely differ from the original estimates (when compared with like estimation procedures), with any discrepancies attributable to simulation error. One small area of difference was the weighting scheme employed by the authors to correct for "over-representation" of meeting attendees in the treatment group. As discussed below, I do not understand the design reason for this choice and when I simulate its properties, I can obtain small amounts of bias. The net consequence of their approach was usually to make coefficient estimates smaller, so we don't have a major difference in conclusion except perhaps in a secondary analysis of mechanisms.

Link to Full Report: https://osf.io/y8ubt/

Link to Replicators' Package: https://osf.io/y8ubt/

Link to Original Authors' Response: https://osf.io/y8ubt/

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/S3HACJ

B.1.67 Replication Report

Title Original Study: Political Turnover, Bureaucratic Turnover, and the Quality of Public Services

doi: https://doi.org/10.1257/aer.20171867, American Economic Review

Report's Abstract: The politically motivated replacement in local governments is a pervasive fact in our modern democracies. Whether it has causal effects on the quality of public services, such as education, is a critical question and yet understudied. This paper uses a regression discontinuity design (RDD) for close elections to replicate Akthari, Moreira and Trucco (2022) who find negative effects on the quality of public education in Brazil (.05-.08 standard deviations of lower test scores). I first reproduce these main results, finding minor computational differences that have no effect on the conclusions. I also show that the estimates for Brazil are in general robust to different specifications following Brodeur, Cook and Heyes (2020). Finally, I implement the same RDD framework now applied to Chilean administrative records to find null effects on test scores. Taken together, these results suggest that political turnover has weakly negative effects on service quality.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/39.htm

Link to Replicators' Package: https://osf.io/q43vz/

Link to Original Authors' Response: https://osf.io/kv4pj/

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/150323/version/V1/view

B.1.68 Replication Report

Title Original Study: Pre-Colonial Warfare and Long-Run Development in India

doi: https://doi.org/10.1093/ej/ueab089, Economic Journal

Report's Abstract: We test the reproducibility and replicability of Dincecco et al. (2022), which reports a positive relationship between pre-colonial interstate warfare and long-run development patterns across India. Overall, we confirm that all of the study's estimates are computationally reproducible by using both the provided replication package in Stata and code written by the present authors in R. We test for and find no evidence of data manipulation in the final datasets. Concerning direct replicability, we consider different ways of measuring distance to conflicts and also alternative proxies for both the dependent variable and variables which capture channels by which the main effects operate. We are able to replicate the magnitude and significance of the estimated coefficient on conflict exposure in most of the tests, noting that while most estimates are substantively in line with the original study, some alternative measures of distance to conflict imply different magnitudes for estimates, and proxy estimates are sensitive to both the time period and type of conflict considered.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/35.htm

Link to Replicators' Package: https://osf.io/af6m2/

Link to Original Authors' Response: https://osf.io/af6m2/

Original Authors' Package: https://zenodo.org/records/5583263

B.1.69 Replication Report

Title Original Study: Public Infrastructure and Economic Development: Evidence from Postal Systems

doi: https://doi.org/10.1111/ajps.12594, American Journal of Political Science

Report's Abstract: Rogowski et al. (2022) use secondary data to study the impact of historic postal infrastructure on economic development, both cross-country and within the US. Their results suggest a large positive effect of post offices on economic development that is robust across various sensitivity checks. We successfully computationally reproduce all results. In a robustness assessment, we find the results to be robust to simple changes in the analysis but observe some sensitivity to accounting for spatial trends in the cross-country analysis. Additionally, we correct a coding inconsistency, showing that in the corrected version, one main robustness check for the US-analysis is no longer supporting the result. Despite this, we find the results to be overall robust given the numerous analyses and robustness checks in the original paper.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/92.htm

Link to Replicators' Package: https://osf.io/j3ydr/?view_only=ad14a07cb3a741ca9bbfab391ad7c6fe

Original Authors' Response: "Thanks so much for reproducing the findings in our paper and exploring extensions of our results. We also appreciate your sharing the report with us. [...] I [Rogowski] confirm that we are comfortable letting your report stand and that we will not write a response to it. "

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/33K3EF

B.1.70 Replication Report

Title Original Study: Re-Assessing Elite-Public Gaps in Political Behavior

doi: https://doi.org/10.1111/ajps.12583, American Journal of Political Science

Report's Abstract: Kertzer (2022) conducts a meta-analysis of parallel experiments on samples of political elites and ordinary citizens. He examines whether the average treatment effect for elites is significantly different from the average treatment effect for citizens, finding that only 19 of 162 (11.7%) difference-in-difference estimates are statistically significant after adjusting for the false discovery rate. He also finds that elites and masses hold similar foreign policy attitudes after controlling for their demographic characteristics. In this replication report, we begin by running robustness and heterogeneity tests for the first claim. We find that the results survive many robustness tests. We also find, however, that only a small number of the these treatments significantly affected masses (N=28) or elites (N=30). This low rate suggests the possibility that almost all of these experiments failed to successfully manipulate either masses or elites. If so, we may not be able to conclude that masses and elites respond similarly to experiments with confidence until political scientists produce more experiments with actual treatment effects or with successful manipulation checks in cases of null effects. In the second part of this replication report, we conceptually replicate the second Kertzer analysis, finding a strong correlation between elite and mass political decisions and attitudes, thus confirming Kertzer's analysis.

Link to Full Report: https://www.econstor.eu/bitstream/10419/266385/1/I4R-DP010.pdf

Link to Replicators' Package: https://osf.io/93urk/

Original Authors' Response: "Thank you for your email and for the invitation. [...] please send my appreciation to the authors for their interest in the manuscript; I find their analysis very interesting."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/LHOTOK

B.1.71 Replication Report

Title Original Study: Rebel on the Canal: Disrupted Trade Access and Social Conflict in China, 1650–1911

doi: https://doi.org/10.1257/aer.20201283, American Economic Review

Report's Abstract: Cao and Chen (2022a) study the role of disruption of trade on social conflict in China in the 19th century. Identification builds on the closure of China's Grand Canal in 1826 in a difference-in-differences framework. In their preferred analytical specification, the authors find that counties along the canal experienced a 117 percent increase in rebelliousness after the initial closure of the canal in 1826 relative to their non-canal counterparts. First, we reproduce the paper's main findings using the official replication package. Second, we examine whether a sub-sample of counties/prefectures/provinces drives the result. Third, we test the robustness of the results to alternative treatment periods.

Link to Full Report: https://osf.io/dhn6e/

Link to Replicators' Package: https://osf.io/dhn6e/

Link to Original Authors' Response: https://osf.io/dhn6e/

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/157781/version/V1/view

B.1.72 Replication Report

Title Original Study: Recessions, Mortality, and Migration Bias: Evidence from the Lancashire Cotton Famine

doi: https://doi.org/10.1257/app.20190131, American Economic Journal: Applied Economics

Report's Abstract: Vellore Arthi, Brian Beach and W. Walker Hanlon (2022) investigate the effect of the Lancashire Cotton Famine on mortality, accounting for the migration response to the downturn. They use difference-in-differences to estimate the effect of the cotton famine on mortality. To account for the migration response to the cotton famine, they construct a linked dataset giving mortality rates by district of residence during the cotton famine, rather than by district of residence at the time of death. They find that the cotton famine increased mortality in cotton-textile producing districts, and that accounting for migration matters, in the sense that their estimates would have been markedly different had they not accounted for it. I check that ABH results are fully reproducible using their data and code, and that their claims are robust to (1) decreasing the age window for building the linked dataset, (2) modifying the specification and (3) computing different standard errors. The only significant discrepancy in results is that I find stronger effects of the cotton famine when I decrease the age window for building the linked dataset, likely because this reduces measurement errors.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/25.htm

Link to Replicators' Package: https://www.openicpsr.org/openicpsr/project/192272/version/V1/view

Original Authors' Response: "Thanks for the interest in our work. We've had a chance to review the report and it looks like everything replicated. Since there are no outstanding queries, we are happy to sign off on this."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/128521/version/V1/view

B.1.73 Replication Report

Title Original Study: Reshaping Adolescents' Gender Attitudes: Evidence from a School-Based Experiment in India

doi: https://doi.org/10.1257/aer.20201112, American Economic Review

Report's Abstract: Dhar et al. (2022) examine the effect of a gender attitude change program in secondary schools in India. In their preferred specification, the authors show that the program made the students report more gender-egalitarian attitudes by 0.18 of a standard deviation, and shifted self-reported behaviors to be more aligned with gender-progressive norms by 0.20 standard deviations (both significant at 1% level). In contrast, they found no effect on girls' aspirations, as these were already high before the intervention. The effects did not attenuate between the first endline (right after the programme was completed) and the second (two years later). To put the paper's results in perspective, we first comment on the authors' deviations from their pre-registration and pre-analysis plans, provide detailed power calculations, and add multiple-hypothesis-testing-adjusted standard errors. Second, we show that the paper's results are perfectly reproducible. Third, we show that the results are robust to excluding control variables, and alternative ways of constructing indices and dealing with non-response.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/24.htm

Link to Replicators' Package: https://osf.io/r5jfe/

Final Original Authors' Response: "Thanks. the revision looks good. I actually don't think we need to have a formal response any more. [...] Thus, I don't think there is anything substantive for us to include in a discussion paper/response. That reflects the fact that the Replication Reports fair and there is nothing major to respond to, so it's good news, from both the perspective of the integrity of our original paper and the professionalism of the replication."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/149882/version/V1/view

B.1.74 Replication Report

Title Original Study: Run-off Elections in the Laboratory

doi: https://doi.org/10.1093/ej/ueab051, Economic Journal

Report's Abstract: Bouton et al. (2022) make a causal claim by manipulating the voting system under which participants vote (runoff or plurality) and examining whether this manipulation affects the proportion of strategic voting. They estimate the effect of the voting system on the proportion of strategic voting for the participant population, using random effect regression where standard errors are clustered on group level. Regarding replication results, we reproduced the original study's main findings. Our analysis confirms that there are minor and mostly non-significant disparities in electoral outcomes and voters' welfare between the two voting systems, consistent with the original study's conclusions. Specifically, we conducted tests to assess the study's computational reproducibility and direct replicability. While the authors provided the raw data, they did not include a script for cleaning it or a codebook describing its contents. Consequently, we developed a data cleaning script to ensure accurate and consistent data processing.

Link to Full Report: https://osf.io/a8cev/

Link to Replicators' Package: https://github.com/carinahausladen/runoff-elections

Original Authors' Response: The authors provided feedback which was taken into account.

Original Authors' Package: https://oup.silverchair-cdn.com/oup/backfile/Content_public/Journal/ej/132/641/10.1093_ej_ueab051/1/ueab051_replication_files.zip?Expires=1704993942&Signature=4IGTUYh-IfKsIvWJDcNRrfEdvehlL~h9QzAwuLIHhIm1K8lXbGdONIWK2OK77Fxq~GdQAlilJyP0-BlPHa0iBNn-Mv7acHnbCOBcH3XokNsUXz4oXnKRijyFul7nlqKnWjs3OcBjkqAKYKz9~F0NIfLXKnO0lqO9RuizzRE4vpwyfk2Bu~pOqGPi8O~Zd8qWBH1mBF5GxRQxUHYQxV1lTpiXfwY14LoNkOBEu-k3mhtEyfxThmUXObJpnpGJuwGJiqQUa4a91FjTE2CFjbfiBuiK-jWfdFMvDG40nBdBUj0glLuyHmx7rzmuiWJEHY7kz89ut6z8rv~jV3zNiQngdQ__&Key-Pair-Id=APKAIE5G5CRDK6RD3PGA

B.1.75 Replication Report

Title Original Study: School Spending and Student Outcomes: Evidence from Revenue Limit Elections in Wisconsin

doi: https://doi.org/10.1257/pol.20200226, American Economic Journal: Economic Policy

Report's Abstract: Baron (2022) explores the independent effects of operational expenditure and capital expenditure on student outcomes in school districts across Wisconsin from the outcomes of close referendum approvals. By utilizing a dynamic regression discontinuity framework and cubic specification, the author finds that narrowly passing an operational referendum, increases operational expenditure per pupil by $298 each year on average, following the referendum over a ten year period. From this $198 are spent on instructional expenses. These point estimates are statistically significant at the 10% and 5% level, respectively. We first reproduce the main results from the paper without any issues arising. Secondly, we conduct a robustness replicability to (1) dropping school districts from the top and bottom 5% of the revenue limits distribution, categorically, and (2) dividing the time frame of the study into two periods: 1996-2005 and 2005-2014. We find that dropping the top 5% of the school districts by revenue limits reduces the additional operational expenditure by $140 per pupil (lower by 50 percent) and the effects of passing an operational referendum were nearly double in the former period compared to the latter period. Lastly, we find that the estimated effects on student outcomes rely heavily on recent observations.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/88.htm

Link to Replicators' Package: https://osf.io/m2w4x/

Original Author's Response: "Thank you for sharing the replication report. Please pass on my thanks to the replicators for their important work. First and foremost, I'm glad to see that the results in the paper are reproducible without any issues arising. The report explores two additional sources of heterogeneity. I have no additional comments on these. I do briefly want to clarify the last sentence in the report's abstract, which reads "Lastly, we find that the estimated effects on student outcomes rely heavily on recent observations." While I am not entirely sure what the replicators are referring to, my guess is that they refer to Table 2 in the report. In this table, they discuss that they are unable to study heterogeneity in the impacts of passing a referendum on test scores and postsecondary enrollment from 1996-2005, because data on these outcomes are unavailable prior to 2005. The availability of each dataset was discussed in the published version of the paper (see, for example, Table 1). Perhaps a more accurate statement would be to explain that the replicators couldn't explore the impact of passing a referendum on these specific outcomes in the early period due to data constraints—and that this was acknowledged in the published version."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/125821/version/V1/view

B.1.76 Replication Report

Title Original Study: Social Class and (Un)Ethical Behaviour: Causal and Correlational Evidence

doi: https://doi.org/10.1093/ej/ueac022, Economic Journal

Report's Abstract: The relationship between social status and ethical behavior is a widely debated topic in research. In their study, Gsottbauer et al. (2022b) investigate whether higher socio-economic status is linked to lower ethical behavior, using data from two large survey experiments involving over 11,000 participants. In this replication project, we test the computational reproducibility and robustness to the replication of their study, using the provided data and code from the replication package (Gsottbauer et al., 2022a). Nearly all the figures and tables were reproducible-in the process of reproducing the results, some minor rounding or transcription errors were discovered. In testing the robustness replicability, we find consistent results for our extensions. The effort for the replication was manageable, even though the authors treat categorical variables as numeric, or use manually-coded interaction variables (i.e., in regression models). In summary, we applaud the transparency of Gsottbauer et al. (2022b) in facilitating replications, and make some general recommendations for further improvements for data-analysis studies.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/29.htm

Link to Replicators' Package: https://github.com/ha0ye/replication-gsottbauer-2022

Original Authors' Response: Declined to respond.

Original Authors' Package: https://zenodo.org/records/6226207

B.1.77 Replication Report

Title Original Study: Sorting or Steering: The Effects of Housing Discrimination on Neighborhood Choice

doi: https://doi.org/10.1086/720140, Journal of Political Economy

Report's Abstract: This comment revisits the analysis in Christensen and Timmins (2022). We identify two critical errors used in the original analysis, one with the data and the other with coding. When either error is corrected several major results in the paper change, either in statistical significance or in effect size. The data error is a result of including fixed effects for the string variable 'city'. The raw variable is case sensitive and has many spelling mistakes. The coding error involves assigning a value of zero for the variable "of color" to both individuals identified as 'white' and as 'other' in the raw data. The level of clustering in the paper is also arguably too fine. Many of the results are not robust to clustering at the city level, as opposed to the subject pair level. In total, we affirm the authors' overarching claim of substantial and nuanced housing discrimination against racial minorities generally, and African Americans in particular; however, the effect sizes and significance are generally (although not always) smaller than the original authors findings. Additionally, there are several instances where the effects of discrimination on African Americans are no longer statistically significant but the effect of discrimination on Hispanics becomes significant.

Link to Full Report: https://osf.io/vwgxd/

Link to Replicators' Package: https://github.com/mattdwebb/HUDreplication

Original Authors' Response: Authors mentioned that they are currently writing a response.

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/720140/suppl_file/20191181data.zip

B.1.78 Replication Report

Title Original Study: Spillover Effects of Intellectual Property Protection in the Interwar Aircraft Industry

doi: https://doi.org/10.1093/ej/ueab091, Economic Journal

Report's Abstract: We are attempting to reproduce the results of Hanlon and Jaworski (2022) based on their dataset. Our work is conducted in two different ways: (i) computational reproducibility, aiming to produce the same results using different software, notably R, with the given data; and (ii) checking the robustness of the results. For (i), the estimated coefficients are consistent based on the R software. For (ii), we carefully examine the given datasets of Hanlon and Jaworski (2022) and review the economic history of the US Interwar aircraft industry between 1918 and 1935 to identify potential confounding variables (apart from IPP strengthening in the year 1926) that might affect both the controls and error term, and thus the results. We identify some confounding variables that may affect the results and attempt to illustrate them before and after 1926 when IPP is strengthened. Overall, we find that the results are replicable by utilizing the codes and datasets of Hanlon and Jaworski (2022), which is encouraging.

Link to Full Report: https://osf.io/t4avf/

Link to Replicators' Package: https://osf.io/t4avf/

Link to Original Authors' Response: https://osf.io/t4avf/

Original Authors' Package: https://zenodo.org/records/5627298

B.1.79 Replication Report

Title Original Study: State Action to Prevent Violence against Women: The Effect of Women's Police Stations on Men's Attitudes toward Gender-Based Violence

doi: https://doi.org/10.1086/714931, Journal of Politics

Report's Abstract: Córdova and Kras (2022) examine how the existence of a women's police station (WPS) in the place of residence influences citizens' attitudes toward gender-based violence in Brazil. In their analytical specification, the authors find that men are more likely to reject violence against women (VAW) and support bystander intervention in municipalities with a WPS, especially if the WPS has been operating for a long time. This paper examines the replicability and robustness of Córdova & Kras' (2022) findings. First, we reproduce the paper's main findings and uncover one minor coding error and three estimates that have been reported with the opposite sign compared to that in our reproduction; neither is of consequence for the study's main results. Second, we test the robustness of the results by (1) recoding one of the main explanatory variables and several of the control variables to account for non-linear trends, (2) using alternative techniques to estimate clustered standard errors, (3) consistently applying a 95% confidence level in the presentation of the results, (4) altering the propensity score matching (PSM) procedure as well as the composition of the variables used in the PSM robustness check, (5) using an alternative technique to test for multicollinearity, (6) excluding potential endogenous control variables, and (7) using an alternative coding for computing margins. Reassuringly, the results are robust to most of these tests. However, two of the robustness checks challenge parts of the paper's main findings. First, allowing for non-linearity in the effect of time since the establishment of WPS shows (a) a non-linear effect on VAW and (b) no apparent changes in either male or female attitudes over time once the WPS has been established. Second, the inclusion of other variables in the PSM procedure renders part of the main estimates of interest statistically nonsignificant ($p < 0.1$). Our findings highlight the need for further re-analyses and replications for investigating the preventive effects of women's police stations.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/67.htm

Link to Replicators' Package: https://osf.io/yjwr8/

Link to Original Authors' Response: Responded to our emails but no formal response as of February 2024.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/D2WL5I

B.1.80 Replication Report

Title Original Study: Student Performance, Peer Effects, and Friend Networks: Evidence from a Randomized Peer Intervention

doi: https://doi.org/10.1257/pol.20200563, American Economic Journal: Economic Policy

Report's Abstract: Wu et al. (2023) estimate the effect of classroom seating arrangements in China using a randomized control trial with two treatment schemes. The first treatment scheme involves seating high and low achieving students together, and the second treatment involves this same seating arrangement with financial incentives for the high-achieving students, if their deskmates' test scores improved. All statistically significant impacts come from the incentivized treatment scheme. Wu et al. (2023) find that low-achieving students sitting next to incentivized high-achieving students perform 0.24 SD (p-value=0.018) better on math exams. In addition, being assigned to the incentive treatment scheme increased extraversion and agreeableness for low and high achieving students. Lastly, they do not find much evidence of peer effects on test scores nor personality traits. This study is computationally reproducible using their provided replication package. We ran their code using Stata 14, 17, and 18. After running their replication package, we further investigated Tables 2-5. The main conclusions are generally robust to various coding decisions. Notably, in investigating the peer effects, when we change the specification to also control for the difference in baseline scores between the student and their deskmate, we find that the more dissimilar deskmates are at baseline, the bigger the peer effects.

Link to Full Report: https://osf.io/9hx3b/

Original Authors' Response: The authors provided feedback which was taken into account.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/149262/version/V2/view

B.1.81 Replication Report

Title Original Study: Talking Shops: The Effects of Caucus Discussion on Policy Coalitions

doi: https://doi.org/10.1111/ajps.12636, American Journal of Political Science

Report's Abstract: In Talking Shops: The Effects of Caucus Discussion on Policy Coalitions, Zelizer analyzes the causal effect of caucus deliberations on legislative policy coalitions. In practice, political scientists have little empirical evidence on how policy discussions actually work among sitting legislators and whether these discussions have an effect on policy making and policy opinion. Taking on this challenge, Zelizer conducted two field experiments in an American state legislature. In short, the experiments randomized whether a bill was selected for discussion among a bi-partisan legislative caucus. The paper then measures and reports the corresponding effects of that discussion around the bill. Zelizer finds that deliberation increased the amount of co-sponsorship for a given bill, among both co-partisans and counter-partisans, but deliberation did not effect whether a bill was passed by the legislature or whether the bill received more amendments. We conduct a robustness replication of the main results of Talking Shops. Specifically, we reproduce Tables 3 and 4 of the paper under alternative specifications. We find that the main results of the paper are reproducible and robust to multiple alternative specifications.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/69.htm

Link to Replicators' Package: https://osf.io/tmfyj/

Link to Original Authors' Response: "One purpose of replication, among others, is to evaluate whether published results are sensitive to modeling decisions. Do alternative, reasonable approaches generate the same, or different, results? Did the author's approach provide an outlier estimate that is indicative of p-hacking or, to be kinder about it, sensitivity of results to modeling decisions? That seems incredibly useful. That purpose is not advanced, in my view, by testing 'incorrect' methods or models. We do not learn about the robustness of results from testing alternative approaches that introduce bias, or by estimating different estimands that are a combination of treatment effects and selection bias. While it doesn't seem to matter too much in this case — selection bias appears relatively small, and in the same direction as treatment effects — I think this issue matters for the exercise in general for several reasons. First, do the analyses justify the inferences being made? In my view, changing the estimand or estimating biased models cannot justify saying anything about the robustness of the original results. Second, what would have happened if the new results did not match the original? Are we willing to claim published results are not robust when applying estimators with known flaws generates different results? And third, shouldn't we just generally aim to use 'correct' estimators for a given situation? While IPW is not perfect, ignoring differential treatment probabilities is a conscious decision to ignore selection bias. Why would we want to run that model if our goal is inference about treatment effects? I appreciate the work everyone is doing on this enterprise. Hopefully these comments, whether correct or not, help advance the goal of publishing robust, valid empirical research."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/S3M5AX

B.1.82 Replication Report

Title Original Study: Targeting High Ability Entrepreneurs Using Community Information: Mechanism Design in the Field

doi: https://doi.org/10.1257/aer.20200751, American Economic Review

Report's Abstract: Hussam et al. (2022a) use a cash grant experiment in India to demonstrate that community knowledge can help target high-growth microentrepreneurs. In their preferred specification, the authors find that the average marginal return to the grant is 9.4 percent per month, while estimated returns for entrepreneurs reported by peers to be in the top third of the community are between 24 percent and 30 percent. First, we reproduce the paper's main findings and uncover one minor coding error, which affects the estimates for one of the main tables but does not change the overall conclusions of the paper. Second, we test the robustness of the results to: (1) different treatment of outliers, (2) dropping surveyor and survey month fixed effects, and (3) using quartiles instead of terciles for grouping the ranking of entrepreneurs. The paper's results are robust to these robustness checks. Finally, we test heterogeneity of results by gender, which was not reported in the original study.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/49.htm

Link to Replicators' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/DI7RR9

Link to Original Authors' Response: "We are very grateful to Isabella Masetto, Diego Ubfal, and to the team at I4R for their excellent work. We verified the coding error and we agree that it did not meaningfully alter the conclusion of our paper that community information is informative over and above the predictive power of observable characteristics. We will post a link to this correction on our websites and will consult the editors of the AER as to whether this error rises to the level of requiring a formal correction."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/151841/version/V1/view

B.1.83 Replication Report

Title Original Study: Teaching Norms: Direct Evidence of Parental Transmission

doi: https://doi.org/10.1093/ej/ueac074, Economic Journal

Report's Abstract: This paper is a replication study of Brouwer, T., Galeotti, F., & Villeval, M. C. (2023), using the original data. The study explores how social norms are transmitted from one generation to another, specifically from parents to children. The authors conducted a field experiment involving 601 parents of children aged 3 to 12 in Lyon, France, to examine whether parents engage more in norm enforcement in the presence of their child, and whether the nature of punishment changes in the presence of the child. The study found that parents do engage more in norm enforcement in the presence of their child, and tend to use more indirect punishment when their child is present. This study highlights the role that parents play in transmitting social norms to their children. The replication analysis was successful, with the results of the original study being robust to changes in the model specification.

Link to Full Report: https://osf.io/qnbfa/

Link to Replicators' Package: https://zenodo.org/records/8114738

Original Authors' Response: The replicators took into account the authors' feedback. They wrote at the end of the back and forth: "We thank you and the replication team for the replication and the successive interactions. We created an OSF project including the data replication package enabling the reproduction of the analysis presented in our article. The package comprises a source file (in Stata format and in TXT) and a Stata do-file that allows the reconstruction of the master file used in the replication package submitted to the Economic Journal."

Original Authors' Package: https://zenodo.org/records/7045559

B.1.84 Replication Report

Title Original Study: Technological Change and the Consequences of Job Loss

doi: https://doi.org/110.1257/aer.20210182, American Economic Review

Report's Abstract: Braxton and Taska (2023) find that technological change accounts for 45 percent of the decline in earnings after job loss. We first reproduce all regression tables in Braxton and Taska (2023), and then test for robustness by controlling for the initial level of wages, additional fixed effects, multi-way clustering, and conducting influential analysis. We find that the paper's original results are sensitive to controlling for initial wages and some additional fixed effects. Overall, we find the results are robust with a coefficient in the same direction and significant at 5% in 33% of the robustness checks we ran, with average t/z scores 28% as large as the original study.

Link to Full Report: https://osf.io/qws2p/

Link to Replicators' Package: https://osf.io/qws2p/

Original Authors' Response: Authors mentioned they would write a response in the coming weeks.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/181166/version/V1/view

B.1.85 Replication Report

Title Original Study: The Common-Probability Auction Puzzle

doi: https://doi.org/10.1257/aer.20191927, American Economic Review

Report's Abstract: Ngangoué and Schotter (2023) investigate common-probability auctions. By running an experiment, they find that, in contrast to the substantial overbidding found in common-value auctions, bidding in strategically equivalent common-probability auctions is consistent with the Nash equilibrium. We reproduce their results in R, conduct robustness checks on how their sample was constructed, and consider possible heterogeneity. We confirm their documented qualitative results.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/74.htm

Link to Replicators' Package: https://osf.io/7bq4s/

Original Authors' Response: "Thank you for putting the effort in replicating our study! Your results are also quite interesting to us as we haven't thought of all the robustness checks you've made. At this point, we do not have any major comments to make and refrain from submitting a response."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/184041/version/V1/view

B.1.86 Replication Report

Title Original Study: The Curious Case of Theresa May and the Public That Did Not Rally: Gendered Reactions to Terrorist Attacks Can Cause Slumps Not Bumps

doi: https://doi.org/10.1017/S0003055421000861, American Political Science Review

Report's Abstract: Holman et al. (2022; HMZ) propose women (compared to men) political leaders experience significant drops in public approval ratings after a transnational terrorist attack. After documenting how survey-based evaluations of then-Prime Minister Theresa May suffered after the 2017 Manchester Arena attack, HMZ assemble a country-quarter level panel database to explore the generality of their hypothesis. They report evidence suggesting women (compared to men) leaders systematically experience decreased public approval rates after major transnational terrorist attacks (p-value of 0.020). We find that result disappears once any of the following adjustments is implemented: (i) excluding election quarter covariates (p = 0.104); (ii) correcting objective coding errors in the election quarter covariates (p = 0.058); (iii) excluding the May-Manchester observation (p = 0.098); or (iv) clustering standard errors at the country level (p = 0.558). Exploring all 2^5 combinations of the five control groups HMZ incorporate in their specification, none of them clears the 5% threshold of statistical significance once the corrected election quarter variables are employed. We conclude that the empirical evidence does not provide sufficient support for HMZ's abstract claim that "conventional theory on rally events requires revision: women leaders cannot count on rallies following major terrorist attacks."

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/41.htm

Link to Replicators' Package: https://doi.org/10.5683/SP3/6SYCML

Link to Original Authors' Response: https://econpapers.repec.org/paper/zbwi4rdps/44.htm

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/VHNPUO

B.1.87 Replication Report

Title Original Study: The Dynamics and Spillovers of Management Interventions: Evidence from the Training within Industry Program

doi: https://doi.org/10.1086/719277, Journal of Political Economy

Report's Abstract: Bianchi and Giorcelli (2022) study the long-term and spillover effects of a management intervention program on firm performance in the US, between 1940 and 1945. The authors find that the Training Within Industry (TWI) program led to positive effects which lasted for at least 10 years. Firm sales of treated firms increased by 5.3% in the first year after implementation, peaking at 21.7% after 8 years, before reducing to 16% gains after a decade. The authors claim that the program generated long-lasting changes in managerial practices. Finally, the program also led to positive spillover effects on the supply chain of treated firms. First, we reproduce the paper's main findings. Second, we test the robustness of the results to (1) changing the main specification sample and (2) testing other difference-in-differences estimators, using the same data, provided by the authors. We find that the results are robust to these changes. All point estimates in the study remain statistically significant and of similar magnitude. While the paper's finding reproduce and replicate, challenges in reproducing results we encountered lead us to recommend improvements to journals' code policies.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/66.htm

Link to Replicators' Package: https://github.com/cwestheide/i4r_dp66_code

Original Authors' Final Response: "Thanks a lot for sharing the updated report with us. We don't have anything to add at this point."

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/719277/suppl_file/20200781data.zip

B.1.88 Replication Report

Title Original Study: The Economic Effects of Long-Term Climate Change: Evidence from the Little Ice Age

doi: https://doi.org/10.1086/720393, Journal of Political Economy

Report's Abstract: Waldinger (2022) finds significant negative economic effects (proxied by city size) from gradual climate change which occurred during the Little Ice Age (1600-1850) and offers two potential mechanisms (agricultural productivity and mortality) and two potential adaptations (trade and land use). In this comment, we show that while Waldinger (2022)'s findings can be replicated, the main result relies on a critical author assumption: Cities with 0 inhabitants in the original data are instead assumed to have 500. This assumption affects 23.5% of observations and 49.6% of cities in the sample. When these "missing data" are excluded from the analysis, the effect estimated by otherwise identical research methods is of similar magnitude and statistical significance but of opposite sign.

Link to Full Report: https://osf.io/tmn2j/

Link to Replicators' Package: https://osf.io/tmn2j/

Link to Original Authors' Response: https://osf.io/tmn2j/

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/720393/suppl_file/2015548data.zip

B.1.89 Replication Report

Title Original Study: The Effects of Banking Competition on Growth and Financial Stability: Evidence from the National Banking Era

doi: https://doi.org/10.1086/717453, Journal of Political Economy

Report's Abstract: Carlson et al. (2022) examine the causal impact of banking competition by investigating a unique circumstance in the National Banking Era of the nineteenth century in the US, where a discontinuity in bank capital requirements occurred. On the one hand, their findings suggest that banks operating in markets with fewer barriers to entry tend to increase their lending activities, promoting real economic growth. On the other hand, banks in less restricted markets also exhibit a higher propensity for risk-taking, posing risks to financial stability. First, we fully reproduce the paper's outcomes apart from a minor discrepancy in the estimate of Table 9 attributed to issues in the provided codes. Second, we test the robustness of the results by (i) changing the ranges used to select the sample of cities included in the analysis, (ii) adopting different options to address outliers' potential issues and (iii) introducing additional control variables. We observe that the estimation results remain mostly consistent when subjecting them to various robustness checks. However, it is worth highlighting that the results can be partially influenced by the criteria used to select the sample of cities and the inclusion of control variables.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/81.htm

Link to Replicators' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/BB864R

Original Authors' Final Response: "We thank the replication team (Andrea Calef, Sya In Chzhen, Marco Mandas, and Fabio Motoki) for the detailed replication report. We are glad to hear that the replicating team affirms the robustness of the paper's findings. We are also glad that the replicators were able to successfully replicate all tables and figures. We thank the replicating team for identifying various smaller issues regarding the code structure which fortunately did not affect our original findings. We believe that the report as such does not require us to respond in any further detail. We highly appreciate the effort of both the replicating team and the I4R."

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/717453/suppl_file/20200610data.zip

B.1.90 Replication Report

Title Original Study: The Geography of Repression and Opposition to Autocracy

doi: https://doi.org/10.1111/ajps.12614, American Journal of Political Science

Report's Abstract: Analytic data sets and analysis code are available and they produce the same results as presented in the paper (CRA). Robustness checks involve the (i) use of matching estimators to address possible bias from misspecification, based on propensity score estimated from a random forest model, (ii) doubly robust (TMLE) estimation to address possible bias from misspecification in either the propensity score or outcome regression stages, using a super learner ensemble with random forest, GAM, mean, and non-parametric regression models and averaged over repeated runs to minimize randomness, (iii) define treated comunas as those within a fixed physical distance radius of the nearest military base, rather than only those that contain it, and (iv) instead of using 2SLS to assess the causally mediated effect of military bases on plebiscite outcomes via repression, we propose to conduct mediation analysis (Tingley et al 2013), implemented in the R 'mediation' package.

Link to Full Report: https://www.socialsciencereproduction.org/reproductions/789/published/index?step=4

Link to Replicators' Package: https://github.com/pjesscarter/repression-replication

Link to Original Authors' Response: We are happy that the replicators successfully reproduced all the analysis in our published paper. Moreover, additional robustness checks within the quantitative framework of the paper further confirm the empirical results. Two extensions using propensity score matching give somewhat different results. Unfortunately, these additional estimators violate standard requirements for credible matching designs, i.e., overlap in the propensity score distribution across treatment and control groups. As shown by previous research, this lack of overlap leads to unstable estimators with variance that may explode in finite samples such as ours (Frölich 2004, Khan and Tamer 2010). In another extension, the replicators employ a mediation analysis to re-interpret the empirical evidence in our paper. To support the use of our method, i.e., instrumental variables, we rule out alternative explanations and provide a range of historical evidence. Without historical and contextual support for alternative assumptions, we believe that the method used by the replicators is hard to interpret.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/EYAWES

B.1.91 Replication Report

Title Original Study: The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund

doi: https://doi.org/10.1257/pol.20190299, American Economic Journal: Economic Policy

Report's Abstract: Jones and Marinescu (2022) study the employment effects of a universal cash transfer in Alaska. Using a synthetic control method, they find that the transfer had no negative effects on employment. We reproduce the results using their replication package and investigate if the results hold when using a different software to run the analysis. We also use different estimation techniques and perform sensitivity checks to assess robustness of the results. We find some differences in the size and significance of the average treatment effects on labor force participation and hours worked when we use a different software (R) and various extensions of the synthetic control method. We also find smaller coefficients on part-time employment when including more covariates. However, these differences do not contradict the main conclusion of the paper.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/80.htm

Link to Replicators' Package: https://osf.io/6atfw/

Original Authors' Final Response: "Thanks for putting in all this effort to evaluate the robustness of our results! I [Marinescu] think this is really a worthwhile endeavor."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/140121/version/V1/view

B.1.92 Replication Report

Title Original Study: The Long-Run Effects of Sports Club Vouchers for Primary School Children

doi: https://doi.org/10.1257/pol.20200431, American Economic Journal: Economic Policy

Report's Abstract: Marcus, Siedler and Ziebarth (2022 American Economic Journal: Economic Policy) examine the long-run health effects of a universal sports-club voucher program that was introduced in Saxony for primary school children in 2009. In 2018, the authors designed a survey that targeted the affected cohorts and nearby cohorts in Saxony and two neighboring states, and use a differences-in-differences identification strategy that exploits variation across states and cohorts in policy exposure. The authors document that treated individuals have knowledge of the program and recall receiving and redeeming the vouchers at higher rates, but find no effects on any health outcomes or behaviors. We successfully reproduce the main results of the paper exactly using data available in the paper's replication package and new Stata and R code. We also verify the robustness of the results using different outcomes, different control variables, different sample restrictions and different inference methods.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/46.htm

Link to Replicators' Package: https://osf.io/4bnjt/

Original Authors' Response: "We would like to thank the authors for their interest in our paper. We greatly appreciate their careful reading of the paper and the insightful robustness exercises they conducted. We are pleased that our results were successfully reproduced using different software packages, and that the additional robustness analyses performed by the authors further strengthen and support our conclusions."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/138922/version/V1/view

B.1.93 Replication Report

Title Original Study: The Long-Term Effects of Measles Vaccination on Earnings and Employment

doi: https://doi.org/10.1257/pol.20190509, American Economic Journal: Economic Policy

Report's Abstract: Atwood (2022) analyzes the effects of the 1963 U.S. measles vaccination on longrun labor market outcomes, using a generalized difference-in-differences approach. We reproduce the results of this paper and perform a battery of robustness checks. Overall, we confirm that the measles vaccination had positive labor market effects. While the negative effect on the likelihood of living in poverty and the positive effect on the probability of being employed are very robust across the different specifications, the headline estimate-the effect on earnings-is more sensitive to the exclusion of certain regions and survey years.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/33.htm

Link to Replicators' Package: https://osf.io/jv7kx/

Link to Original Authors' Response: https://osf.io/qxjnk/

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/138401/version/V1/view

B.1.94 Replication Report

Title Original Study: The Macroeconomics of Sticky Prices with Generalized Hazard Functions

doi: https://doi.org/10.1093/qje/qjab042, Quarterly Journal of Economics

Report's Abstract: We replicate the empirical results in Section 4 of Alvarez et al. (2022). First, we were able to reproduce the original authors' major empirical results, but only after editing the program for it to run on our computing platform. There are small discrepancies in the empirical estimates, e.g. bootstrapped standard errors, that involve the use of simulations. Second, we replicated Alvarez et al.'s results by adopting the data cleaning criteria used by their original data source (Cavallo 2018) to evaluate its robustness to data handling procedures. We found noticeable changes in the empirical results that can have important implications on the effects of monetary policy. To conclude, we propose using Docker container to promote research reproducibility, and more attention is needed on data handling to improve the robustness of empirical research.

Link to Full Report: https://github.com/atyho/Ottawa-Replication-Games-2023/blob/main/Ho_Huynh_Rea_Replication_Report.pdf

Link to Replicators' Package: https://github.com/atyho/Ottawa-Replication-Games-2023/

Link to Original Authors' Response:

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/IBM0IE

B.1.95 Replication Report

Title Original Study: The Morning After: Cabinet Instability and the Purging of Ministers after Failed Coup Attempts in Autocracies

doi: https://doi.org/10.1086/716952, Journal of Politics

Report's Abstract: We replicate the analysis provided in Bokobza et al. (2022). They identify a causal effect of failed coup attempts on cabinet minister removals in autocracies on both the country and individual minister level and show that higher-ranking ministers and those holding strategic positions are more likely to be purged than more loyal and veteran ministers using fixed effects panel models. We focus on computational reproducibility and robustness replicability. In addition to reproducing the original results using Stata and R, we replicate analyses using random effects panel models and ordered beta regression models, reproduced analyses performed in R using different packages, replaced the main independent variable, clustered standard errors on a different level, and added independent variables related to coup-proofing. We find that the original findings were reproducible and robust.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/45.htm

Link to Replicators' Package: https://doi.org/10.7910/DVN/21HZCC

Link to Original Authors' Response: https://osf.io/sm526/

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/GCDJ25

B.1.96 Replication Report

Title Original Study: The Origin of the State: Land Productivity or Appropriability?

doi: https://doi.org/10.1086/718372, Journal of Political Economy

Report's Abstract: This is a replication of Mayshar et al. (2022) (MPP).The article posits that the state (defined as societal hierarchy such as tax-levying elites) originated from cultivation of appropriable cereal grains, contrary to the conventional theory that the state originated from increased land productivity following the adoption of agriculture. The article uses multiple datasets to demonstrate a causal effect of cereal cultivation on hierarchy (Claim 1) without finding a similar effect for land productivity (Claim 2), and that societies based on roots or tubers display levels of hierarchy similar to nonfarming societies (Claim 3). The results of our replication in brief are: 1. The data and code provided by MMP closely reproduce the main results presented in their Table 1 (see our Table 1). 2. Concurrently testing the cereal cultivation and land productivity claims leads to slightly less statistical significance, on average, than the published article (Table 2).3. Removing the inherited 1-5 scale of the dependent variable (hierarchy) finds that cereal production is not as effective at moving across all levels of hierarchy compared to the more general claim (Table 3 and 4).4. Using the same procedures with an aim to confirm the conventional hypothesis (land productivity leads to increased hierarchy conditional on cereal growth) offers statistically significant evidence both for and against Claims 1 and 2 and against Claim 3 (Table 6). 5. The statistical significance of Claim 1 is sensitive to the removal of the top 3% of observations outliers (Table 7). 6. Correction of mis-classified 'none or none specified' crop societies alters the interpretation of coefficients behind Claim 3. Societies that rely more on agriculture among farming societies experience more complex hierarchies, irrespective of being primarily cereal producing or tubers growing (Table 8 and 9). (...)

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/82.htm

Link to Replicators' Package: https://osf.io/ekzdg/

Original Authors' Response: Comments taken into account in the report.

Original Authors' Package: https://www.journals.uchicago.edu/doi/suppl/10.1086/718372/suppl_file/2018030data.zip

B.1.97 Replication Report

Title Original Study: The Power of Hydroelectric Dams: Historical Evidence from the United States over the Twentieth Century

doi: https://doi.org/10.1093/ej/ueac059, Economic Journal

Report's Abstract: Successful computational reproducibility. No coding errors uncovered.

Original Authors' Package: https://zenodo.org/records/7019816

B.1.98 Replication Report

Title Original Study: The Relative Efficiency of Skilled Labor across Countries: Measurement and Interpretation

doi: https://doi.org/10.1257/aer.20191852, American Economic Review

Report's Abstract: Rossi (2022) examines the relative efficiency of skilled workers across countries. He finds the elasticity of skill efficiency with respect to GDP per worker is 1.4 and that the relative human capital accounts for only about 9 percent. We reproduce the paper's main findings and test the sensitivity of the results to (1) alternative samples and (2) additional controls for determining wages. We find the results remain robust to these alternative specifications, and the estimated values of the key elasticities remain nearly unchanged.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/59.htm

Link to Replicators' Package: https://osf.io/fge7z/

Original Author's Response: "Thanks for replicating the paper. I don't have any comments to add to the report."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/146041/version/V1/view

B.1.99 Replication Report

Title Original Study: The Side Effects of Immunity: Malaria and African Slavery in the United States

doi: https://doi.org/10.1257/app.20190372, American Economic Journal: Applied Economics

Report's Abstract: Esposito (2022) documents the role of malaria in the diffusion of African slavery in the US. She finds that the introduction of malaria triggered a demand for malaria-resistant labour, which led to a massive expansion of African enslaved workers in more malaria-infested areas. Further results document that, among African slaves, more malaria-resistant individuals commanded significantly higher prices. We reproduce the paper's main findings, uncovering only one minor coding error that has no effect on the study's main results. We then test the robustness of the results to (1) varying the set of control variables used in various analyses; (2) conducting permutation tests; and (3) conducting event studies that account for time-varying controls. We generally find that the author's results are robust to all of these alternative specifications, though there are some sets of controls that cause estimates to become small and statistically insignificant.

Link to Full Report: https://osf.io/728ud/

Link to Replicators' Package: https://osf.io/728ud/

Original Authors' Response: Original author provided feedback. No final response on the updated version.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/120483/version/V1/view

B.1.100 Replication Report

Title Original Study: The Wheels of Change: Technology Adoption, Millwrights and the Persistence in Britain'S Industrialisation

doi: https://doi.org/10.1093/ej/ueab102, Economic Journal

Report's Abstract: Mokyr et al. (2022) estimate the effects of early technology adoption on industrialization. Authors argue that human capital was the main determinant of the location of the industry in the first decades of the Industrial Revolution. They document that the location of mills in the eleventh century (reported in the Doomsday Book) has a positive and statistically significant impact on the number of wrights in the early eighteenth century. We confirm the computational reproducibility of the paper. The estimates are not sensitive to outliers, which are common in the data. The results are also robust to changes in the control variables. The results remain robust if we adjust the estimated p-values for the low number of clusters, and if we include county fixed effects. We conduct a placebo experiment with a present-day outcome (the Brexit referendum) to check if the results are picking up on a more general demographic and economic correlation pattern; the experiment shows no spurious correlations.

Link to Full Report: https://osf.io/gdne3/

Link to Replicators' Package: https://osf.io/tws8n/

Original Authors' Response: No response.

Original Authors' Package: https://zenodo.org/records/5734954

B.1.101 Replication Report

Title Original Study: Understanding Ethnolinguistic Differences: The Roles of Geography and Trade

doi: https://doi.org/10.1093/ej/ueab065, Economic Journal

Report's Abstract: Dickens (2022) studies the role of trade on long-run inter-ethnic linguistic differences. He establishes that neighboring ethnolinguistic groups have smaller (lexicostatistical) linguistic distances when there is a larger agricultural productivity variation between them. Specifically, he establishes that pre-1500 land productivity variation (CSI SD) and its change due to Columbian Exchange in the post-1500 (CSI SD CHANGE) era decrease linguistic distances between groups. In what can be considered his main specification, which includes geographical controls, spatial controls, and language family fixed effects (Table 1 column 5), he estimates that a one standard deviation increase in the change in land productivity variation (post-1500) decreases linguistic distances by 0.11 standard deviations (p-value ¡ 0.01) and a one standard deviation increase in land productivity variation (pre-1500) decreases linguistic distances by 0.06 standard deviations (p-value = 0.12). We conduct a direct replication of the paper by (i) reconstructing the main independent variables using the same original sources and following the procedures explained in the original study, (ii) using an updated version of the linguistic map (Ethnologue v17 instead of v16), and (iii) constructing alternative measures of inter-ethnic potential gains from trade. Our results basically confirm the sign, magnitude, and statistical significance of the point estimates in the original study.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/62.htm

Link to Replicators' Package: https://osf.io/k3p7g/

Link to Original Authors' Response: https://econpapers.repec.org/paper/zbwi4rdps/63.htm

Original Authors' Package: https://oup.silverchair-cdn.com/oup/backfile/Content_public/Journal/ej/132/643/10.1093_ej_ueab065/2/ueab065_replication_files.zip?Expires=1704996457&Signature=vd4j4Sgew70tKc9uWbPKOHlAGfK48X1HRxhQJgweFXIbFcEKdILQEyfh8FdhERkzWILgGurP0VSMDLETGC9VaG3CgKIpAwM3q~ZOQcpkS8-aL7wWR5uGOeUe6tspavXQZO03ZSfMJIZdZoagJHeuKK-rbftOfNFQRVC7N6Bdry184zWa8RQy4xKSncJRgJmUBVCGZJBdA6KGfQx6o4S0lXMy7GOy8rBGQKRZEvC9qre1LYXXUx71ozqVClckTI6DmE0qpkhE9Xu20s-g-7LUxlY9pd8GuRzsWT4kSBqbznx7lys2iaMB2eej~30pZkHgMWs2XkTJYP1YQUbxijWN-A__&Key-Pair-Id=APKAIE5G5CRDK6RD3PGA

B.1.102 Replication Report

Title Original Study: Vulnerability and Clientelism

doi: https://doi.org/10.1257/aer.20190565, American Economic Review

Report's Abstract: The paper estimates the effect that changes in household vulnerability have on citizens' participation in clientelist relationships. The authors exploit two sources of variation in household vulnerability: rainfall shocks, and a randomized intervention that provided cisterns in drought-prone areas. We reproduce all the findings presented in the four main results tables presented in the paper. The results of our robustness replication show that the results in the original paper are robust to variations in the rainfall period used as a baseline to assess changes in household vulnerability, and to exclusions that eliminate individuals in the sample who may have been substituted with others at different survey points. However, some of the original results that explain the underlying mechanisms are sensitive to how "clientelist relationships" are defined. When more frequent interactions with politicians are used as the defining characteristic of households in clientelist relationships, we find that the original results suggesting clientelism as a significant mechanism are no longer statistically significant at any standard significance level. We note, however, that the authors, in a reply to questions we sent them after the Replication Games, convincingly show that their results are robust to changing the definition of the clientelist marker.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/83.htm

Link to Replicators' Package: https://osf.io/q2tw6/

Link to Original Authors' Response: https://econpapers.repec.org/paper/zbwi4rdps/84.htm

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/173341/version/V1/view

B.1.103 Replication Report

Title Original Study: Wage Cyclicality and Labor Market Sorting

doi: https://doi.org/10.1257/aeri.20210161, American Economic Review: Insights

Report's Abstract: Figueiredo (2022) examines wage cyclicality across the skill mismatch distribution finding large differences. Some key results include finding that wages are acyclical in good labor market matches but procyclical in poor matches. Using the public replication material provided by the authors, we were able to exactly duplicate the results of the study. Further, using several further robustness checks, such as subtracting (potentially correlated) covariates in the regressions, using different standard errors (rather than clustered ones), or different time periods of the data left the key results largely unchanged with some minor caveats.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/78.htm

Link to Replicators' Package: https://osf.io/a8hcg/

Original Authors' Response: "I have read the report and I do not wish to write a reply.

Congratulations on this initiative – it is great!"

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/150581/version/V1/view

B.1.104 Replication Report

Title Original Study: War, Socialism, and the Rise of Fascism: an Empirical Exploration

doi: https://doi.org/10.1093/qje/qjac001, Quarterly Journal of Economics

Report's Abstract: In this report, we present the results from a replication of Acemoglu et al. (2022). The authors suggest that the emergence of the 'Red Scare' in the aftermath of World War I led to a rise of fascism in Italy in the early 1920s. Their approach uses the war casualties as an instrument for the rise in socialist voting. We performed a series of replication strategies, including pre-trend controls, applying an alternative instrument and modifying the first-stage specification, as well as investigating the long-run political alignment. Based on our findings, the original authors' results are replicable under a variety of alternative specifications.

Link to Full Report: https://osf.io/a672c/

Link to Replicators' Package: https://osf.io/a672c/

Link to Original Authors' Response: No response.

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/CLJTSC

B.1.105 Replication Report

Title Original Study: What Makes Anticorruption Punishment Popular? Individual-Level Evidence from China

doi: https://doi.org/10.1086/715252, Journal of Politics

Report's Abstract: t also indirect effects through affecting evaluations of competence and morality. Conducting a conjoint study in China where respondents were asked to choose between two potential local officials, Tsai et al. found that 26% of the total effect of these officials punishing corrupt subordinates was estimated to come through indirect effects that go through evaluations of morality and competence. Using their code, I reproduced their original findings, and did not find any notable coding errors while doing so. Then, taking advantage of the fact that Tsai et al. included several additional covariates beyond punishment in their experiment, I engaged in an extension of the original model, using the same method, to examine whether economic performance characteristics have indirect effects on evaluation through competence and morality as well. I found results that suggest that economic performance does have an indirect effect on preferences through competence and morality. I then tested the robustness of Tsai et al.'s original heterogeneous sensitivity tests by varying cut points on two demographic variables and found that their findings of a lack of heterogeneous sensitivity remain robust to different cut-points. In all, my efforts suggest that Tsai et al.'s methods are valid and their findings robust.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/7.htm

Link to Replicators' Package: https://osf.io/czs6j/

Original Authors' Response: "We appreciate your efforts, both in replicating our paper and in doing so systematically for other studies in leading political science and economic journals. Your contribution is valuable to the entire academic community and to us especially.

We also appreciate your sharing replication reports with the original authors prior to dissemination and are glad to see from the replication report that our results and methods appear to be both valid and robust. Although a longer follow-up may not be necessary, we do wish to convey our gratitude to the replicator(s) and to the editorial team."

Original Authors' Package: https://dataverse.harvard.edu/dataset.xhtml;jsessionid=34454d461ad29192edc557995095?persistentId=doi%3A10.7910%2FDVN%2FXTRWKG&version=&q=&fileTypeGroupFacet=&fileAccess=Public&fileSortField=date

B.1.106 Replication Report

Title Original Study: When a Doctor Falls from the Sky: The Impact of Easing Doctor Supply Constraints on Mortality

doi: https://doi.org/10.1257/aer.20210701, American Economic Review

Report's Abstract: Okeke (2023) evaluates a policy experiment conducted in Nigeria, whereby communities were randomly allocated to receive a new doctor at the local public health center. The performance of these centers was compared to other sites which were allocated either a new midlevel health-care provider, or no additional staff. The study finds that communities assigned a new doctor were associated with a decrease in seven-day infant mortality, such a decrease was not observed in communities assigned a midlevel health-care provider. This suggests that it is the 'quality' of the additional doctor driving the effects rather than due to a quantity increase of an additional health worker. The size of the mortality reduction increased with increased exposure to the intervention. We first conduct a computational reproduction, rerunning the original code and data, finding that the results reported in the original study are reproducible. Second, we test the robustness of the results in several ways, by 1) adapting the existing controls to make the results robust to contamination bias, 2) altering and adding to the control variables included, 3) changing the specification or regression technique used, and 4) testing coding grouping and changing how service use was coded. These changes cause little change to the point estimates, although we find that the original paper's standard errors were overly conservative, and thus the statistical significance of some results was understated.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/53.htm

Link to Replicators' Package: https://github.com/e-mcmanus/Okeke23_Replication

Original Authors' Response: "Thank you for sharing the replication report (and please pass on my thanks to the replicators). There does not appear to be much for me to respond to. It is gratifying to see that the results have held up well to additional scrutiny."

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/181581/version/V1/view

B.1.107 Replication Report

Title Original Study: Who Chooses Commitment? Evidence and Welfare Implications

doi: https://doi.org/10.1093/restud/rdab056, Review of Econoimc Studies

Report's Abstract: We conduct a computational reproduction and a robustness replication of Carrera et al. (2022) by using the same dataset and similar procedures as specified in their paper (i.e., method and analysis). Instead of using STATA, we use R and code the results from scratch. We also replicate the MATLAB code used for simulations and test whether it produces reasonable results for different parameter values. We confirm all of the main results and do not find high sensitivity of the model to changes in parameters.

Link to Full Report: https://osf.io/752q9/

Link to Replicators' Package: https://osf.io/752q9/

Link to Original Authors' Response: The authors provided feedback which was taken into account.

Original Authors' Package: https://zenodo.org/records/5173081

B.1.108 Replication Report

Title Original Study: Who Sells During a Crash? Evidence from Tax Return Data on Daily Sales of Stock

doi: https://doi.org/10.1093/ej/ueab059, Economic Journal

Report's Abstract: Hoopes et al., (2021) analyze United States tax return data encompassing all individual taxable stock sales between 2008 and 2009, to investigate the individuals who increased their stock sales in response to market turbulence. Our findings reveal that such increases were notably prevalent among investors in the highest tiers of the income distribution, including the top 1% and 0.1%, as well as retirees and those at the uppermost levels of the dividend income distribution. We reproduce the paper's main findings and results are very similar.

Link to Full Report: https://osf.io/b6s9k/

Link to Replicators' Package: https://www.dropbox.com/scl/fo/c3ysdlenysq391mugzprm/h?rlkey=riooyohci7i5vwx475r13jaqq&dl=0

Original Authors' Response: The authors provided feedback which was taken into account.

Original Authors' Package: https://oup.silverchair-cdn.com/oup/backfile/Content_public/Journal/ej/132/641/10.1093_ej_ueab059/1/ueab059_replication_files.zip?Expires=1704997287&Signature=upyVQnDMB8sNLQdV8sEneBiOcslAUwcueEn5D9bSDy-XtIMI1GC8cuUSONoONguJ2exME~p4ap2V4vqFcH4UwnYece8Xqf84jorKGaCSxUu2GufwIYi9Io2JA3xqxW-gZ1chzZ8mt0FWEYqkrfSkAJM1kxuBWT3yRj6MPbG9ObHH~g9ynCpndkxbUHZYuX8Rgr57j6KWNBQ0WSyb3D9Y-0-o5TaQETTCL93hRMhCciipdP96qZq~0Ml9QgquGTVs7QK-FP4HD7JONWESzoFYTNterBQypZV1DbCP056qtgH12~77cC2aJycGL56wkQ2r0dgSD1bw1JvDIfPuRFipyA__&Key-Pair-Id=APKAIE5G5CRDK6RD3PGA

B.1.109 Replication Report

Title Original Study: Why Don't Firms Hire Young Workers During Recessions?

doi: https://doi.org/10.1093/ej/ueab096, Economic Journal

Report's Abstract: We gauge the replicability of the results of Forsythe (2022) studying the cyclicality of individuals' labor market transitions conditional on their experience. Using Current Population Survey (CPS) data and state-level variation in cyclical unemployment, this paper shows that the hiring probability of youths is more sensitive to business-cycle conditions than that of experienced individuals. We replicate the main results in this paper by reconstructing the dataset using data from the IPUMS-CPS database (Flood et al. (2020)) and recoding the paper's main regressions. We also conduct a robustness replicability analysis and show that the paper's main results are robust in terms of statistical significance to (i) extending the sample period from 1994-2014 to 1994-2019 and (ii) using MSA-level unemployment variation instead of state-level variation. However, these extensions reduce the magnitude of the main effects of interest. The paper's key conclusions are unaffected.

Link to Full Report: https://osf.io/3pqbt/

Link to Replicators' Package: https://github.com/jcrechet/replication_forsythe_2022_EJ

Link to Original Authors' Response: The author responded but did not provide a response.

Original Authors' Package: https://zenodo.org/records/5710784

B.1.110 Replication Report

Title Original Study: Yellow Vests, Pessimistic Beliefs, and Carbon Tax Aversion

doi: https://doi.org/10.1257/pol.20200092, American Economic Journal: Economic Policy

Report's Abstract: Douenne and Fabre (2022) implement a representative survey following the Yellow Vests movement in France that started in opposition to the carbon tax in 2018. They find that a majority of French citizens would oppose a carbon tax and dividend program with proceeds paid equally to each adult. The authors further find that respondents have pessimistic beliefs about several aspects of the policy. They then show how informational treatments cause respondents to update these beliefs, and they finally estimate the causal effect of these beliefs on support for the policy. In this note, we focus on the second section of this paper: the causal effects of feedback on beliefs. Based on elicited household characteristics, Douenne and Fabre (2022) estimate whether each household "wins" or "loses" from the carbon tax and dividend reform. They provide this binary (win vs. lose) information to households and subsequently ask households to evaluate whether they believe they would financially benefit from the policy. By exploiting the discontinuity in win vs. lose feedback, they assess the degree to which feedback affects subjective beliefs, finding that a household that is told it will "win" as a result of the reform increases its subjective belief that it will not lose by about 25 percentage points. The subset of households that is part of the Yellow Vests movement, however, revises its subjective belief of not losing upwards by only 10 percentage points after being told that it will "win" from the carbon tax reform. Conversely, households who initially support the tax increase this belief by 41 percentage points when told they will "win." In this note we replicate this second section of the paper-the causal effects of feedback on beliefs- using the processed data provided by the authors. We successfully replicate the average treatment effect, but we find that the heterogeneous treatment effects may be biased due to model misspecification. While our results support the conclusion that these estimated effects depend on a household's attitudes toward the policy, we find that the source of heterogeneity differs. Further, we note two changes to the analysis that we believe are appropriate (which do not affect the conclusions drawn): first, some (1.8%) of observations in the dataset appear to be misclassified-wrongly coded as if a household would "lose" when in fact they would "win"-and second, the main causal analysis is based on a regression discontinuity design, but does not include standard components of such a design (e.g., a RD plot, optimal selection of bandwidth, density analysis, placebo tests). We update the design to address both of these points. We find results that generally support the main conclusions of Douenne and Fabre (2022), but we urge caution when interpreting the heterogeneous treatment effects.

Link to Full Report: https://econpapers.repec.org/paper/zbwi4rdps/58.htm

Link to Replicators' Package: https://github.com/karemanyassin/Yellow-Vests-Pessimistic-Beliefs-and-Carbon-Tax-Aversion-2022-A-Comment

Original Authors' Response: Authors provided feedback which was taken into account. No response.

Original Authors' Package: https://www.openicpsr.org/openicpsr/project/128143/version/V1/view