Eleven years of student replication projects provide evidence on the correlates of replicability in psychology

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Abstract

A cumulative science requires foundational empirical results that are robust enough to support theories and future experiments. Prominent failed replications of well-known published findings cast doubt on the psychological literature, and large scale replication projects suggested that the overall replication rate in psychology might be between 40-60%. Large-scale replications are arduous, and only around 170 replications have been conducted as part of a large-scale replication project. This dearth of data points limits estimations of replicability and analyses of potential correlates of replicability. Using a new sample of 176 replications conducted as part of a course, we estimate replicability of these studies that were of interest to graduate students. We look at statistical and experimental-design features of studies as potential predictors of replication. We find that within-study designs and large original effect sizes positively correlate with replicability.

1 Introduction

A cumulative science requires foundational empirical results that are robust enough to support theories and future experiments. If scientists attempt to build cumulative science on top of results that are unreliable and cannot be achieved consistently, they waste resources looking for results where there may be none. This is frustrating for individual scientists, but also damaging to the enterprise of science overall.

Scientists, policymakers, and the public often treat publication in a scientific journal as a signal that a result is (likely to be) true. This trust in published findings has led scientists to build on findings without verifying them first, but failures were often explained away and rarely published, leaving the original results uncontested in the literature, while failures to achieve the same outcomes accumulated in private.

During the replication crisis, publicized failures to replicate published findings called into question the tacit assumption that most of the literature was robust and would replicate. Certain prominent findings failed to replicate in multi-site replication attempts (ex. terror management theory, Klein et al. 2022, ego-depletion, Hagger et al. 2016), and a large-scale replication project pegged the replication rate for findings in top-tier psychology journals at around 40% (Open Science Consortium 2015).

How replicable is psychology as a whole? Few large scale replications have been conducted in psychology, due to the resources required to run large numbers of replication studies. We are aware of three. RP:P sampled roughly 100 studies from articles published in three top psychology journals in 2008 and found a overall replication rate of around 40% (Open Science Consortium 2015). ManyLabs investigated heterogeneity using short target studies that compared between two conditions each, but these studies are not representative of the psychology literature as a whole. Across Many Labs 1-3, 29 of 51 target effects (57%) replicated (Klein et al. 2014, Ebersole et al. 2016, Klein et al. 2018). Camerer et al. (2018) replicated all 21 behavioral social science studies from Nature and Science from 2010-2015 that were feasible; the replication rate was around 60%. These roughly 170 replications of psychology studies are the primary empirical results on which discussions of replication rates in psychology are based.

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A replication rate around 50% runs counter to the prior assumption that most findings in the literature were robust enough to support cumulative science, and it raises the question: What makes the replication rates in psychology so low?

Theoretical approaches to understanding the replication rate have modeled how flexible research practices, selective reporting, and a bias toward positive results could lead to a high rate of (non-replicable) false positives (CITE). However, while scientists admit to engaging in these practices, we don't yet know what proportion of non-replications are driven by these factors or how to identify these studies in the literature.

Experimental approaches attempt to intervene on the low replication rate, by changing certain factors that may affect whether studies replicate. Protzko et al. (2020) showed, across 16 studies, that better methodological practices, such as transparency, large sample sizes, and confirmatory tests, led to replication rates that matched theoretical expectations based on the effect sizes and sample sizes, with replication effect sizes comparable with the original. Many Labs 5 examined how adding expert advice to a replication process might increase the replication rate, and found that it mainly did not for the 10 studies they looked at (Ebersole et al. 2020). These types of experiments are valuable for testing potential causes of non-replication, but they don't scale well due to expense. Additionally, not all potential influences on non-replication are experimentally manipulable.

Correlational approaches looking for predictors of replicability are the most popular. Prediction markets and elicitations have established that people can predict what studies will replicate above chance (Dreber et al. 2015, Camerer et al. 2018, Forsell et al. 2019, Hoogeveen et al. 2019), but have not identified concrete predictors that differentiate replications from non-replications. Using machine learning approaches, Altmejd et al. (2019) found that sample size, effect size, and simple effects were predictive of replication. Open Science Consortium (2015) looked at simple correlates of replicability in the RP:P sample and found that studies in cognitive psychology (as opposed to social psychology) and studies with larger effect sizes and smaller p-values were more likely to replicate. Correlational approaches depend on data from replications, generally drawing heavily from the same small set of data points. In particular, the RP:P dataset itself is much discussed and reanalyzed (Anderson et al. 2016, Etz & Vandekerckhove 2016, Gilbert et al. 2016, Patil et al. 2016) to the point that much of what we think we know about replicability may be overfit to the 100 studies included in in RP:P.

Many of the discussions and reanalyses have raised questions about what the right metrics for measuring replicability are and what standards are reasonable to expect. Some of this contention is based around notions that failed replications are referenda on the truth of the original finding.

Prior approaches to replicability have focused on interpreting results in terms of a potentially problematic estimand: the probability of a finding in the literature being somehow truly replicable. Critics have pointed out that "true" replicability may not be possible to estimate outside of a specific sample (Van Bavel et al. 2016) or even time period (Ramscar et al. n.d.).

Further, the methods for estimating this quantity have been theoretically problematic. Sampling schemes for prior work typically do not reflect an entirely random sample from the literature; instead they sample from specific journals where results may be of more interest and adjust the sample for feasibility concerns. These are reasonable sampling choices, but they undermine the claim that the estimand is the level of "truth" in the literature as a whole. Sampling truly at random from the literature may not even be desirable, as arguably a literature will succeed if useful discoveries come out of it, not if random findings are true (Wilson et al. 2020). The importance of a study being replicable is not uniformly distributed across the literature.

In contrast to prior approaches, we have explicitly pursued a different estimand: the probability that a researcher, on selecting a finding of interest from the literature, can successfully achieve a result satisfactorily close enough to the original that they can build on it in their own work, with all the necessary compromises to the methods and sample of the original that may be required by the constraints of the situation.

Our methodology is reflective of this framing of replicability as being the probability of a studying being able to support cumulative science. Rather than sampling at random from some parts of the literature; our sample of studies is selected based on what studies students were interested in and wanting to replicate, with some filtering for feasibility; this sampling reflects how scientists choose what studies to build on: those that are interesting and relatively doable given methodological and budgetary constraints.

We use a subjective replication score as our primary metric. Whether one feels confident in the results of

a study given a replication is not always dependent on only one outcome measure (ex. interaction term) and particularly not dependent on only one statistical comparison between the two studies (ex. replication is p<.05 same direction as original). This metric avoids bright line distinctions and accommodates the range of outcome measures in our diverse set of studies.

All our replications were conducted under short time scales and relatively small budgets, which mimics the constraints many scientists are under when starting new projects. From the point of view of cumulative science, researchers want to know if they can get paradigms to work under their real-world constraints. The same issue that may cause a study not to replicate under constrained circumstances (despite hypothetically replicating under more favorable circumstances such as expert administration or larger budgets) will also plague attempts to build off those studies in constrained circumstances. Thus, we believe it is relevant to estimate replicability under the limitations of real world resource and expertise constraints.

Overall, we take a functional approach to assessing replicability, by framing both our methods and interpretation around the idea of whether work can be repeated or built on by an early-career scientist.

Our contribution is a new dataset of 176 replications of experimental studies from the social sciences, primarily psychology. These replications were conducted by students in graduate-level experimental methods class between 2011 and 2022 as individual course projects. We investigated statistical and experimental-design predictors of replicability in this dataset and found that within-subjects designs and studies with large standardized effect sizes were positively correlated with replication success.

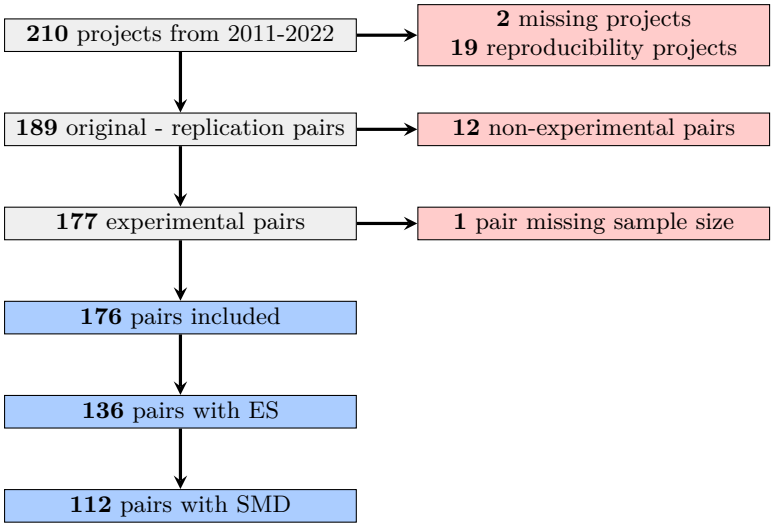

Figure 1: Which studies were excluded for what reasons, and how many original-replication pairs are left.

2 Results

PSYCH 251 is Stanford Psychology's graduate-level experimental methods class taught by MCF. During the 10 week class, students replicated a published finding. They individually re-implemented the study, wrote analysis code, pre-registered their study, collected data using an online platform, and wrote up a structured replication report. Students were free to choose studies related to their research interests, with the default recommendation being an article from a recent year of Psychological Science. The resultant sample of studies is not a random sample from the literature but is representative of studies that are of interest to and doable by first year graduate students.

The sample of replicated studies reflect the variability of the literature, including studies from different subfields, using different experimental methods and statistical outcomes. We leveraged the naturally occurring variability in this sample of replications to examine how different demographic, experimental design, and statistical properties predict replication success.

Many different measures can be used to define replication success of an individual statistical result (Simonsohn 2015, Gelman 2018/ed, Mathur & VanderWeele 2020). Because we operationalized replicability

as whether a study could be built upon, we used a subjective rating of replication success as our primary outcome measure. This measure was applicable across the diverse range of statistical measures and reporting practices present in the sample. It, unlike statistical measures of replication, could easily accommodate studies where there were multiple important outcome measures that together defined the pattern of interest. A holistic measure of replication success had been coded for each project when it was turned in at the end of the class. For reliability, VB independently code the replication success from the replication reports; discrepancies were resolved by discussion between MCF and VB (26% of cases).

As a complement, we also used two statistical measure of replication on the subset of the data where they were computable (138 cases, see Figure 1). We measured p-original, the p-value on the null hypothesis that the original and replication statistics are from the same distribution, as a continuous variable, and we also determined whether the replication statistic fell within the prediction interval of the original statistic (Errington et al. 2021). These both measure how similar the outcome estimates from the two sets of data are.

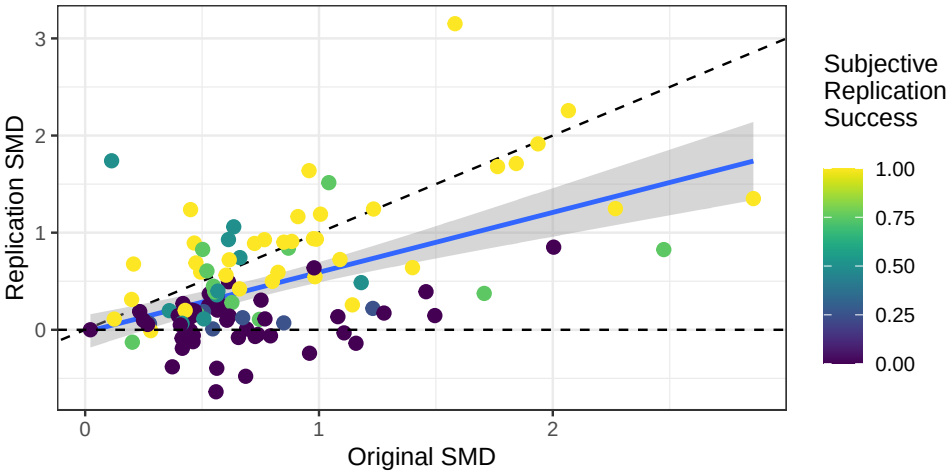

Figure 2: Relationship between SMD of the original study, SMD of the replication study, and subjective replication success rating, for those studies where SMD was applicable.

2.1 Overall replication rate

Across the 176 studies, the overall subjective replication rate was 49%. 45% (62/138) of the studies had replication outcomes within the prediction interval of the original outcome. The median p_original value was 0.03. Figure 2 shows the relationship between original standardized effect size, replication effect size, and subjective replication score. Roughly speaking, there's a cluster of studies that replicate with similar effect sizes to the original and another cluster that fail to replicate with replication effect sizes near zero. On average, there is a diminution of effect sizes from original to replication.

2.2 Single predictors

Properties of both the original study and the replication can influence whether or not the replication is a success. We chose a set of predictor variables from the correlational results of RP:P and our own intuitions about experimental factors that might impact replication success as well as some covariates related to how close the replication was. A full description of these features is given in Methods.

Many predictors individually correlate with subjective replication success (Table 1). Predictors of higher replicability included within-subjects designs, higher numbers of trials, and the original study having open data. Predictors of lower replicability included single vignetted studies, social psychology studies, and original-replication pairs where the original study was in-person and replication switched to online.

Distributions of study outcomes across some of these properties are shown in Figure 3. Both social and cognitive psychology studies were well represented, and the cognitive psychology studies replicated at 2.45 times the rate of social psychology studies. Within and between subjects designs were both common,

Table 1: The correlation of individual predictors with subjective replication outcomes. For subfield, cognitive psychology is treated as the baseline condition. See Methods for how these variables were coded.

Predictors	r	p
Within subjects	0.333	0.000
Log trials	0.182	0.015
Open data	0.150	0.047
Non psych	0.080	0.294
Other psych	0.075	0.322
Publication year	0.064	0.399
Open materials	0.002	0.979
Stanford	-0.027	0.725
Log rep/orig sample	-0.047	0.536
Log original sample size	-0.108	0.155
Switch to online	-0.158	0.037
Social	-0.246	0.001
Single vignette	-0.267	0.000

and within-subjects designs replicated 3.35 times as much. Similarly, studies with multiple vignettes replicated 2.62 times more than single vignetted studies. However, there were strong correlations among these experimental features and between these experimental features and subfield. Studies with open data, which almost always also had open materials, tended to replicate more than studies without open data, although this may be linked to temporal trends.

Nearly all replications studies were conducted online, but original studies were split between using in-person and online recruitment. Replications that switched to online were less likely to replicate than those that had the same modality as the original (generally both online, in a few cases both in-person). While online studies in general show comparable results to studies conducted in person (Crump et al. 2013), switching the modality does decrease the closeness of the replication, and some studies done in person may not have been well-adapted (ex. inductions may be weaker or attention checks inadequate to the new sample).

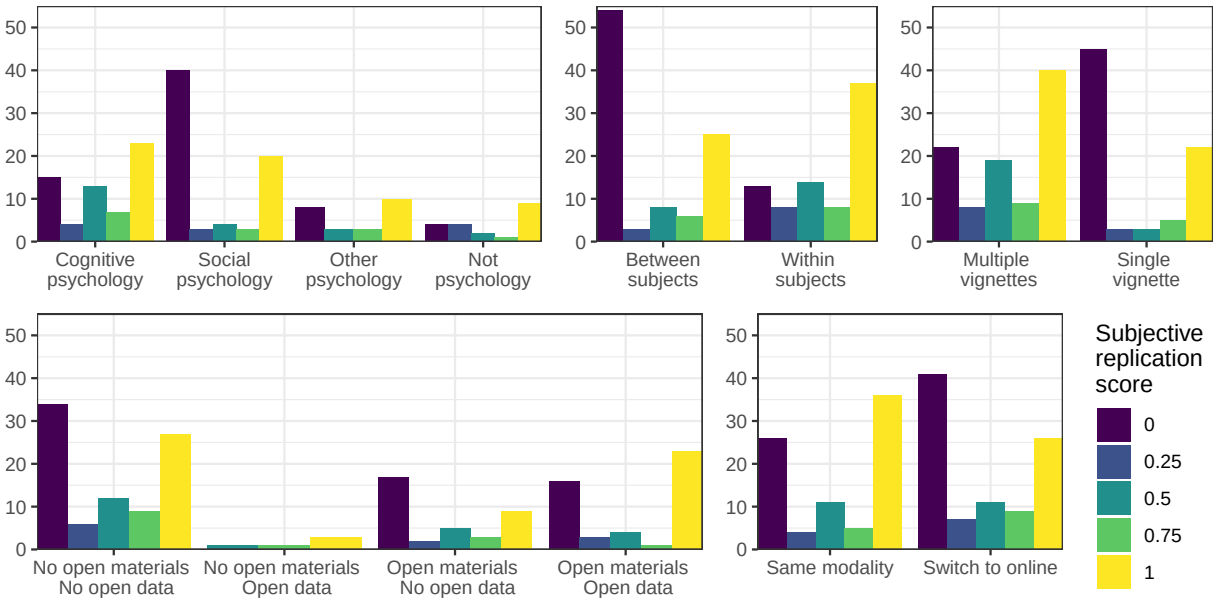

Figure 3: Distribution of subjective replication scores within categories. Bar heights are counts of studies.

2.3 Regression model

While a number of predictors show individual correlations with the subjective replication score, many of the predictors also correlate with one another. In order to determine which predictors were the strongest, we ran a series of pre-registered regularized regression models (see Methods for details; see Supplement for all estimates from all models). We ran models both using all the data, but without statistical predictors (that were uncodable for some studies) and models including statistical predictors, but limiting to the subset of data where all predictors were available. The coefficient estimates from two models predicting the subjective replication scores are shown in Figure 4. Due to a large number of predictors coupled with a small and noisy dataset, even with strong regularization, there is much uncertainty around the coefficients. The general directions of coefficients are consistent with the effects of the predictors in isolation.

Within-subjects designs stand out as the strongest indicator of replicability in the model without statistical predictors (0.55, CrI= [-0.01, 2]). When statistical predictors are added to the model, within-subjects designs remain predictive (0.64, CrI= [-0.03, 2.38]). Standardized effect size is another strong predictor of subjective replication score (0.59, CrI= [0.31, 2.58]). Both effects are robust to a sensitivity analysis including only studies with close replications and matching statistical tests (within-subjects 0.87, CrI= [-0.01, 3.34]; effect size 0.97, CrI= [0.76, 4.59]).

We also ran models predicting our secondary outcome measures: whether the replication effect was within the prediction interval as the original effect and what the p-original was between the replication and original. Both these models had even more uncertain estimates. While the credible intervals were wide, the general patterns of predictor direction and relative strength were similar to the subjective replication models. The strongest predictors were still within-subjects designs (0.71, CrI= [-0.12, 2.38]) and studies with larger effect sizes (0.3, CrI= [-0.31, 0.89]).

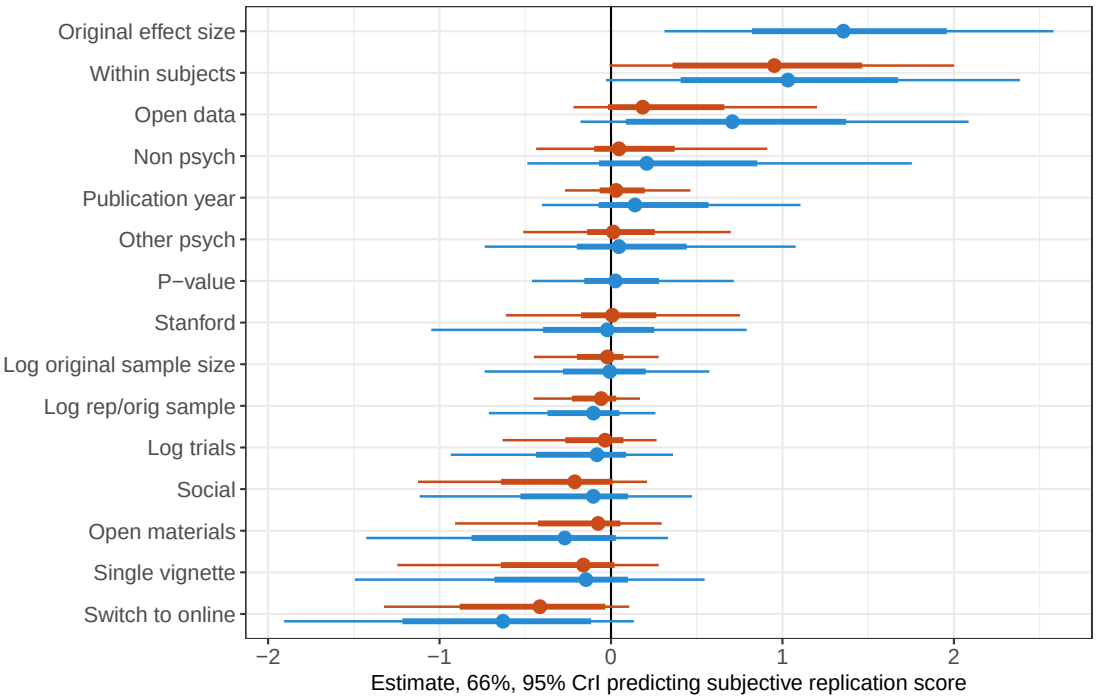

Figure 4: Coefficients and uncertaintly estimates from a model of all predictors (N=112, shown in blue) and non-statistical predictors (N=176, shown in red) predicting subjective replication scores as the dependent variable.

3 Discussion

Prior large scale replications of psychology findings have estimated the replication rate at somewhere around 50%. This rate feels low, raising the question of what is causing so many studies not to replicate.

However, limited numbers of replications and limited research into specific predictors of replication failure leave this an open question.

Here we took advantage of 11 years of graduate student replication projects to look at correlational predictors of replication in a previously-unused dataset.

In line with previous results, we found a `rround(pluck(sub_rep_rate,1))`% replication rate, with some studies showing effect sizes similar to the original and others much smaller. When we looked at individual correlates of replicability, within-subjects designs, work in the subfield of cognitive psychology, and the original and replication both using online samples stood out as the strongest correlates. As many of these predictors interrelate with one another, we ran regularized regressions with all the predictors at once. Due to our small sample, model estimates were uncertain, but within-subjects designs and large original effect sizes were the strongest predictors.

In our case, our estimand is about how well first-year graduate students will do at the replication, which takes into account the limited time, limited budget, and limited experience. We think this is an important estimand, as much of the work of psychology is done by trainees, in circumstances like these. Thus, if some studies have delicate results that require large samples and very exact methods to achieve, they may not replicate under normal resources.

Other replication projects have other estimands: Camerer et al. (2018) seems to ask something closer to how likely studies are to replicate when preformed by an expert with a large budget, and Ebersole et al. (2020) asks how likely certain studies are to replicate when performed with extensive feedback from the original authors.

We do not interpret our results as saying that all non-replications were false positives (presumably some would be replicable under different implementations and budgets and others would not). There are many possible reasons for the non-replications in this sample. In some cases, it seemed that the problem may have been with the replication: for instance, if there were too few participants, or if there were high levels of wrong answers on attention checks, or participants speeding through without attention checks. For these cases, there was a clear next attempt that a student could make if they wanted to get the replication to work. In other cases, there might have been a priori reasons to distrust the original study results, such as exclusion criteria that seemed to be post-hoc, or a three-way interaction effect on a small sample (CITE THAT THIS IS SKETCH). In yet other cases, it's unclear why the results failed to replicate.

[somehow transition here] Pedagogy is important for open science. It's one thing to require or incentivize scientists to use open science practices and conduct replicable and reproducible research, but using the right tools and workflows to do open science is something that has to be learned. Teaching it in the classroom addresses the knowing how point at the beginning and shows students how to have open science practices integrated in to their science at the beginning, before other habits can ossify. Doing replications give students the motivation to care about open science, as they see how much easier it is to implement the study with open materials versus the study where they have to make guesses about the study instructions from the methods section. In presenting work with classmates, students see that there is variation in how well studies replicate, with some replicating very cleanly and others not at all. This sort of first hand experience teaches that not everything they read in the literature may just work if done again.

Our results are limited by the number and quality of the studies we included. These studies are not necessarily representative of the studies of interest to psychologists as a whole. The replication studies were not designed from the start with this analysis in mind, so analysis is limited by the choices and reporting used at the time of replications. We reiterate that given our sample, we are able to estimate the correlates of studies replicating when the replications are conducted online, by graduate students with limited time and budget. This estimand is not the same as whether they would replicate when done by large labs with access to more money and experts. This estimand does not represent whether or not the original study was "true".

TODO conclusion

4 Methods

Our pre-registration, code, and coded data are available at TODO OSF REPO.

4.1 Dataset

The dataset of replication projects comes from class projects conducted in PSYCH 251 (earlier called PSYCH 254) a graduate-level experimental methods class taught at Stanford by MCF from 2011 to 2022. This class is commonly taken by first year graduate students in psychology and related disciplines, and it has been a requirement of the Psychology PhD since around 2015. Each student chose a study to replicate, implemented the study, wrote analysis code, pre-registered their replication, ran the study, and turned in a structured final report including methods, analytic plan, changes from the original study, confirmatory and exploratory analyses, and discussion of outcomes. Students were encouraged to do experimental replications, but some students chose to replicate correlational outcomes or do computational reproducibility projects instead. We cannot include the full student reports for confidentiality reasons, but we include an example as well as the template given to students at TODO example and template.

Students were free to choose what study to replicate; the recommended path for students who did not have their own ideas was to pick an interesting study from a recent year of Psychological Science (this led to a high fraction of Psych Science articles in the replication sample, 80, 45.4545455% of studies).

We note that 4 (TODO check) of the replication projects were included in RP:P, and 10 of them were previously reported in Hawkins et al. (n.d.).

4.2 Coding procedure

We relied primarily on student reports to code the measured variables for the replications. We supplemented this with spreadsheets of information about projects from the time of the class and the original papers.

4.2.1 Measures of replication success

Our primary replication outcome was experimenter and instructor rated replication success. The subjective replication success was recorded by the teaching staff for the majority of class replications at the time they were conducted. Where the values were missing they were filled in by MCF on the basis of the reports. For all studies, replication success was independently coded by VB on the basis of the reports. Where VB's coding disagreed with the staff/MCF's code, the difference was resolved by discussion between VB and MCF (25.5681818% of studies). Subjective replication scores were coded on a [0, .25, .5, .75, 1] scale.

This subjective replication outcome was chosen because it already existed, could be applied to all projects (regardless of type and detail of statistical reporting), and did not rely solely on one statistical measure. As a complement, we also identified a "key" statistical test for each paper (see below for details), and if possible, computed p_original and prediction interval at this statistic, following Errington et al. (2021). p_original was a continuous measure of the p-value on the hypothesis that the original and replication samples come from the same distribution. Prediction interval was a binary measure of whether the replication outcome fell within the prediction interval of the original outcome measure.

4.2.2 Demographic properties

We coded the subfield of the original study as a 4 way factor: cognitive psychology, social psychology, other psychology, and non-psychology. For each paper, we coded its year of publication, whether it had open materials, whether it had open data, and whether it had been conducted using an online, crowd-sourced platform (i.e. MTurk or Prolific).

4.2.3 Experimental design properties

We coded experimental design on the basis of student reports, which often quoted from the original methods, and if that did not suffice, the original paper itself. To assess the role of repeated measures, we coded the number of trials seen per participant, including filler trials and trials in all conditions, but excluding training or practice trials.

We coded whether the manipulation in the study was instantiated in a single instance ("single vignette"). Studies with one induction or prime used per condition across participants were coded as having a single vignette. Studies with multiple instances of the manipulation (even if each participant only saw one) were coded as not being single vignette. While most studies with a single vignette only had one trial and vice versa, there were studies with a single induction and multiple test trials, and other studies with multiple scenarios instantiating the manipulation, but only one shown per participant.

We coded the number of subjects, post-exclusions. We coded whether a study had a between-subjects, within-subjects, or mixed design; for the analysis, mixed studies were counted as within-subjects designs. In the analysis, we used a log-scale for number of subjects and numbers of trials.

4.2.4 Properties of replication

We coded whether the replication was conducted on a crowd-sourced platform; this was the norm for the class projects, but a few were done in-person. For analysis, we coded this into a variable indicating if the recruitment platform changed between original and replication. This grouped the few in-person replications in with the studies that were originally online and stayed online in a "no change" condition, in contrast with the studies that were originally in-person with online replications.

We coded the replication sample size (after exclusions). This was transformed to the predictor variable log ratio of replication to original sample size.

As a control variable, we included whether the original authors were faculty at Stanford at the time of the replication. This was to account for potential non-independence of these replications (ex. if replicating their advisor's work, students may have access to extra information about methods).

We made note of studies to exclude for sensitivity analyses, due to not quite aligned statistics, extremely small or unbalanced sample sizes, or a student choosing a key statistical measure that was not of central importance to the original study.

4.2.5 Determination and coding of key statistical measure

For each study pair, we used one key measure of interest for which we calculated the predictor variables of p-value and effect size and the statistical outcome measures p_original and prediction interval. If the student specified a single key measure of interest and this was a measure that was reported in both the original paper and replication, we used that measure. If a student specified multiple, equally important, key measures, we used the first one. When students were not explicit about a key measure, we used other parts of their report (including introduction and power analysis) to determine what effect and therefore what result they considered key. In a few cases, we went back to the original paper to find what effect was considered crucial by the original authors. When the measures reported by the student did not cleanly match their explicit or implicitly stated key measure, we picked the most important (or first) of the measures that were reported in both the original and replication. These decisions could be somewhat subjective but importantly they were made without reference to replication outcomes.

Whenever possible, we used per-condition means and standard deviations, or the test statistic of the key measure and its corresponding degrees of freedom (ex. T test, F test). We took the original statistic from the replication report if it quoted the relevant analysis or from the original paper if not. We took the replication statistics from the replication report.

We then calculated p values, ES, p_orig, and predInt. We choose to recalculate p values and effect sizes from the means or test statistic rather than use reported measures when possible because we thought this would be more reliable and transparent. The means and test statistics are more likely to have been outputted programmatically and copied directly into the text. In contrast, p-values are often reported as $<.001$ rather than as a point value, and effect size derivations may be error prone. By recording the raw statistics we used and using our available code to calculate other measures, we are transparent, as the test statistics can be searched for in the papers, and all processing is documented in code.

In some cases, p-values and or effect sizes were not calculable either due to insufficient reporting (ex. reporting a p-value but no other statistics from a test) or key measures where p-values and effect sizes did not apply (ex. PCA as measure of interest). Where studies reported beta estimates and standard

errors or proportions, standardized effects sizes are not an applicable measure, but we were still able to calculate p_original and prediction interval.

We separately coded whether the original and replication effects were in the same direction, based on raw means and graphs. This is more reliable than the statistics because F-tests don't include the direction of effect, and some students may have flipped the direction in coding for betas or t-tests. In the processed data, the direction of the effect of the replication was always coded consistently with the original study's coding, so a positive effect was in the same direction as the original and a negative effect in the opposite direction.

In regression analyses, we used SMD and log p-value as predictors.

4.3 Modelling

Due to the monotonic missingness of the data, we had more predictor variables and outcome variables for some original-replication pairs than for others. To take full advantage of the data, we ran a series of models, with some models having fewer predictors, but more data, and others having more predictors, but less data.

We ran a model predicting the subjective replication score on the basis of demographic and experimental predictors on the entire dataset. We ran two models predicting p_original and prediction interval from demographic and experimental predictors on the subset of data where we had p_original and prediction intervals. Then, on the smaller subset of the data where we had effect sizes and p-values, we re-ran these three models with those as additional predictor variables.

The subjective replication scores were coded on [0, .25, .5, .75, 1], and we ramapped these to 1-5 to run an ordinal regression predicting replication score. We ran logistic regressions predicting prediction interval and linear regressions predicting p_original.

All models used a horseshoe prior in brms. All models included random slopes for predictors nested within year the class occurred to control for variation between cohorts of students. We did not include any interaction terms in the models. All numeric predictor variables were z-scored after other transforms (e.g., logs) to ensure comparable regularization effects from the horseshoe prior.

As a secondary sensitivity analysis, we examined the subset of the data where the statistical tests had the same specification, the result was of primary importance in the original paper (i.e. not a manipulation check), and there were no big issues with the replication.

Results from these models not reported in the main paper are reported in the supplement.

Acknowledgements

Acknowledge people here. `{-}` useful to not number this section.

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