Replicability & Open Science


The replication crisis

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Feeling the Future: Experimental Evidence for Anomalous Retroactive Influences on Cognition and Affect

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The term *psi* denotes anomalous processes of information or energy transfer that are currently unexplained in terms of known physical or biological mechanisms. Two variants of psi are *precognition* (conscious cognitive awareness) and *premonition* (affective apprehension) of a future event that could not otherwise be anticipated through any known inferential process. Precognition and premonition are themselves special cases of a more general phenomenon: the anomalous retroactive influence of some future event on an individual's current responses, whether those responses are conscious or nonconscious, cognitive or affective. This article reports 9 experiments, involving more than 1,000 participants, that test for retroactive influence by "time-reversing" well-established psychological effects so that the individual's responses are obtained before the putatively causal stimulus events occur. Data are presented for 4 time-reversed effects: precognitive approach to erotic stimuli and precognitive avoidance of negative stimuli; retroactive priming; retroactive habituation; and retroactive facilitation of recall. The mean effect size (*d*) in psi performance across all 9 experiments was 0.22, and all but one of the experiments yielded statistically significant results. The individual-difference variable of stimulus seeking, a component of extraversion, was significantly correlated with psi performance in 5 of the experiments, with participants who scored above the midpoint on a scale of stimulus seeking achieving a mean effect size of 0.43. Skepticism about psi, issues of replication, and theories of psi are also discussed.

Keywords: psi, parapsychology, ESP, precognition, retrocausation

The term *psi* denotes anomalous processes of information or energy transfer that are currently unexplained in terms of known physical or biological mechanisms. The term is purely descriptive; it neither implies that such phenomena are paranormal nor connotes anything about their underlying mechanisms. Alleged psi phenomena include *telepathy*, the apparent transfer of information from one person to another without the mediation of any known channel of sensory communication; *clairvoyance* (sometimes called *remote viewing*), the apparent perception of objects or

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Psi is a controversial subject, and most academic psychologists

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Why Psychologists Must Change the Way They Analyze Their Data: The Case of Psi: Comment on Bem (2011)

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Does psi exist? D. J. Bem (2011) conducted 9 studies with over 1,000 participants in an attempt to demonstrate that future events retroactively affect people's responses. Here we discuss several limitations of Bem's experiments on psi; in particular, we show that the data analysis was partly exploratory and that one-sided *p* values may overstate the statistical evidence against the null hypothesis. We reanalyze Bem's data with a default Bayesian *t* test and show that the evidence for psi is weak to nonexistent. We argue that in order to convince a skeptical audience of a controversial claim, one needs to conduct strictly confirmatory studies and analyze the results with statistical tests that are conservative rather than liberal. We conclude that Bem's *p* values do not indicate evidence in favor of precognition; instead, they indicate that experimental psychologists need to change the way they conduct their experiments and analyze their data.

Keywords: confirmatory experiments, Bayesian hypothesis test, ESP

Bem (2011) presented nine experiments that test for the presence of psi.[1] The experiments were designed to assess the hypothesis that future events affect people's thinking and people's behavior in the past (henceforth, precognition). As indicated by Bem, precognition—if it exists—is an anomalous phenomenon, because it conflicts with what we know to be true about the world (e.g., weather forecasting agencies do not employ clairvoyants, casinos make profits). In addition, psi has no clear grounding in known biological or physical mechanisms.[2]

Despite the lack of a plausible mechanistic account of precognition, Bem (2011) was able to reject the null hypothesis of no precognition in eight out of nine experiments. For instance, in Bem's first experiment 100 participants had to guess the future position of pictures on a computer screen, left or right. And indeed,

We think that the answer to this question is negative and that the take-home message of Bem's (2011) research is in fact of a completely different nature. One of the discussants of the Utts review paper made the insightful remark that "parapsychology is worth serious study . . . If it is wrong [i.e., psi does not exist], it offers a truly alarming massive case study of how statistics can mislead and be misused" (Diaconis, 1991, p. 386). And this, we suggest, is precisely what Bem's research really shows. Instead of revising our beliefs regarding psi, Bem's research should instead cause us to revise our beliefs on methodology: The field of psychology currently uses methodological and statistical strategies that are too weak, too malleable, and offer far too many opportunities for researchers to befuddle themselves and their peers.

RESEARCH ARTICLE

PSYCHOLOGY

Estimating the reproducibility of psychological science

Open Science Collaboration*†

Reproducibility is a defining feature of science, but the extent to which it characterizes current research is unknown. We conducted replications of 100 experimental and correlational studies published in three psychology journals using high-powered designs and original materials when available. Replication effects were half the magnitude of original effects, representing a substantial decline. Ninety-seven percent of original studies had statistically significant results. Thirty-six percent of replications had statistically significant results; 47% of original effect sizes were in the 95% confidence interval of the replication effect size; 39% of effects were subjectively rated to have replicated the original result; and if no bias in original results is assumed, combining original and replication results left 68% with statistically significant effects. Correlational tests suggest that replication success was better predicted by the strength of original evidence than by characteristics of the original and replication teams.

Reproducibility is a core principle of scientific progress (*1*–*6*). Scientific claims should not gain credence because of the status or authority of their originator but by the results are false and therefore irreproducible (*9*). Some empirical evidence supports this analysis. In cell biology, two industrial laboratories reported success replicating the original results of

facilitated each step of the process and maintained the protocol and project resources. Replication materials and data were required to be archived publicly in order to maximize transparency, accountability, and reproducibility of the project (https://osf.io/ezcuj).

In total, 100 replications were completed by 270 contributing authors. There were many different research designs and analysis strategies in the original research. Through consultation with original authors, obtaining original materials, and internal review, replications maintained high fidelity to the original designs. Analyses converted results to a common effect size metric [correlation coefficient (r)] with confidence intervals (CIs). The units of analysis for inferences about reproducibility were the original and replication study effect sizes. The resulting open data set provides an initial estimate of the reproducibility of psychology and correlational data to support development of hypotheses about the causes of reproducibility.

Sampling frame and study selection

We constructed a sampling frame and selection process to minimize selection biases and maximize generalizability of the accumulated evidence. Simultaneously, to maintain high quality, within this sampling frame we matched individual replication projects with teams that had

Believe it or not: how much can we rely on published data on potential drug targets?

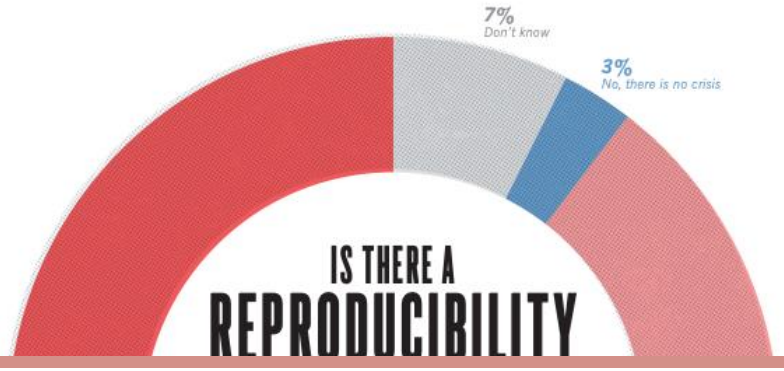
Florian Prinz, Thomas Schlange and Khusru Asadullah

A recent report by Arrowsmith noted that the success rates for new development projects in Phase II trials have fallen from 28% to 18% in recent years, with insufficient efficacy being the most frequent reason for failure (Phase II failures: 2008–2010. *Nature Rev. Drug Discov.* **10**, 328–329 (2011))[1]. This indicates the limitations of the predictivity of disease models and also that the validity of the targets being investigated is frequently questionable, which is a crucial issue to address if success rates in clinical trials are to be improved.

Candidate drug targets in industry are derived from various sources, including in-house target identification campaigns, in-licensing and public sourcing, in particular based on reports published in the literature and presented at conferences. During the transfer of projects from an academic to a company setting, the focus changes from 'interesting' to 'feasible/marketable', and the financial costs of pursuing a full-blown drug discovery and development programme for a particular target could ultimately be hundreds of millions of Euros. Even in the earlier stages, investments in activities such as high-throughput screening programmes are substantial, and thus the validity of published data on potential targets is crucial for companies when deciding to start novel projects.

To mitigate some of the risks of such investments ultimately being wasted, most pharmaceutical companies run in-house target validation programmes. However, validation projects that were started in our company based on exciting published data have often resulted in disillusionment when key data could not be reproduced. Talking to scientists, both in academia and in industry, there seems to be a general impression that many results that are published are hard to reproduce. However, there is an imbalance between this apparently widespread impression and its public recognition (for example, see REFS 2,3), and the surprisingly few scientific publications dealing with this topic. Indeed, to our knowledge, so far there has been no published in-depth, systematic analysis that compares reproduced results with published results for wet-lab experiments related to target identification and validation.

Early research in the pharmaceutical industry, with a dedicated budget and scientists who mainly work on target validation to increase the confidence in a project, provides a unique opportunity to generate a broad data set on the reproducibility of published data. To substantiate our incidental observations that published reports are frequently not reproducible with quantitative data, we performed an analysis of our early (target identification and validation) in-house projects in our strategic research fields of oncology, women's health and cardiovascular diseases that were performed over the past 4 years (FIG. 1a). We distributed a questionnaire to all involved scientists from target discovery, and queried names, main relevant published data (including citations), in-house data obtained and their relationship to the published data, the impact of the results obtained for the outcome of the projects, and the models



Data on how much of the scientific literature is reproducible are rare and generally bleak. The best-known analyses, from psychology[1] and cancer biology[2], found rates of around 40% and 10%, respectively. Our survey respondents were more optimistic: 73% said that they think that at least half of the papers in their field can be trusted, with physicists and chemists generally showing the most confidence.

More than 70% of researchers have tried and failed to reproduce another scientist's experiments, and more than half have failed to reproduce their own experiments. Those are some of the telling figures that emerged from *Nature*'s survey of 1,576 researchers who took a brief online questionnaire on reproducibility in research.

The data reveal sometimes-contradictory attitudes towards reproducibility. Although 52% of those surveyed agree that there is a significant 'crisis' of reproducibility, less than 31% think that failure to reproduce published results means that the result is probably wrong, and most say that they still trust the published literature.

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The results capture a confusing snapshot of attitudes around these issues, says Arturo Casadevall, a microbiologist at the Johns Hopkins Bloomberg School of Public Health in Baltimore, Maryland. "At the current time there is no consensus on what reproducibility is or should be." But just recognizing that is a step forward, he says. "The next step may be identifying what is the problem and to get a consensus."

Failing to reproduce results is a rite of passage, says Marcus Munafo, a biological psychologist at the University of Bristol, UK, who has a long-standing interest in scientific reproducibility. When he was a student, he says, "I tried to replicate what looked simple from the literature, and wasn't able to. Then I had a crisis of confidence, and then I learned that my experience wasn't uncommon."

The challenge is not to eliminate problems with reproducibility in published work. Being at the cutting edge of science means that sometimes results will not be robust, says Munafo. "We want to be discovering new things but not generating too many false leads."

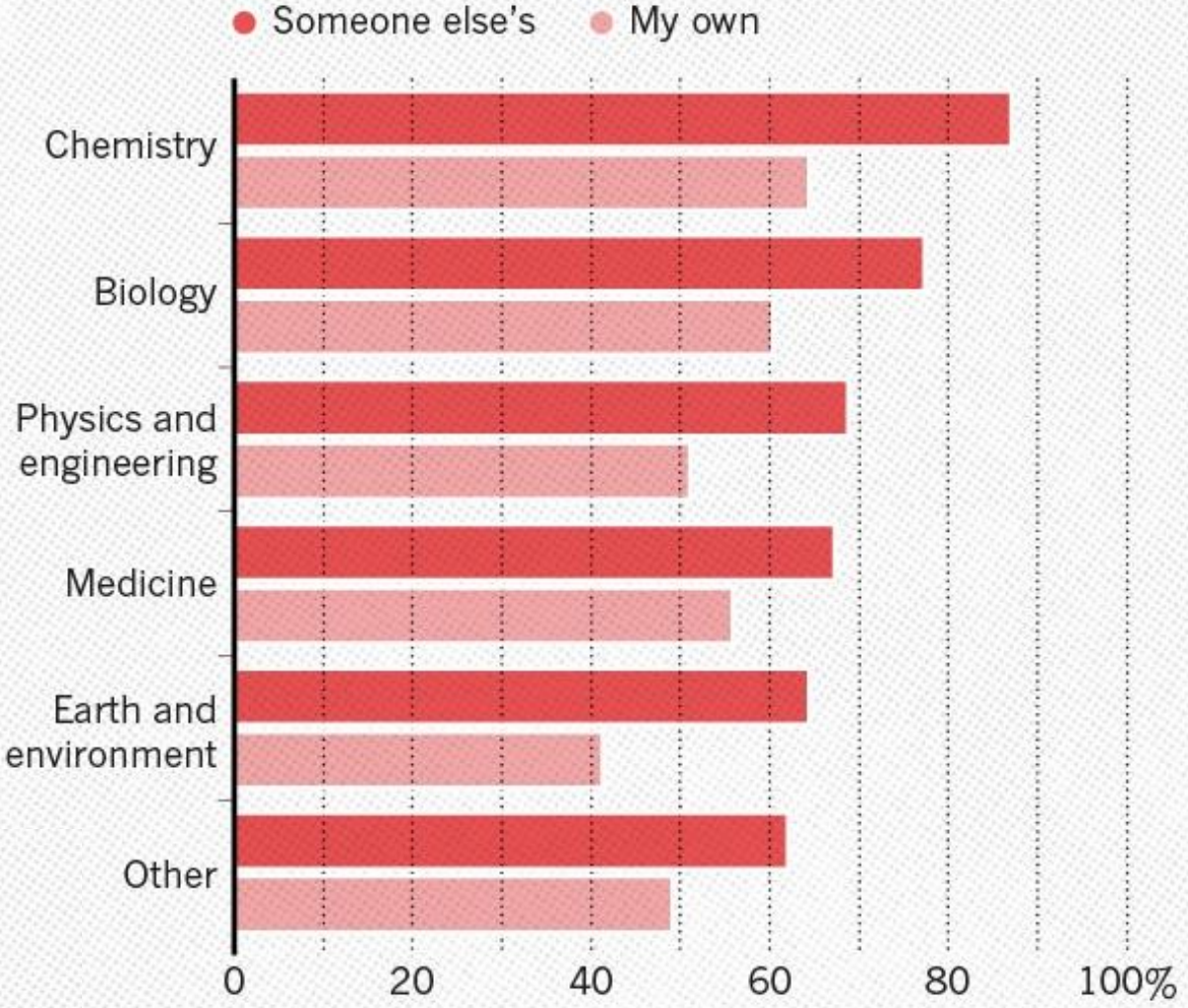
THE SCALE OF REPRODUCIBILITY

But sorting discoveries from false leads can be discomfiting. Although the vast majority of researchers in our survey had failed to reproduce an experiment, less than 20% of respondents said that they had ever been contacted by another researcher unable to reproduce their work (see 'A 'crisis' in numbers'). Our results are strikingly similar to another online survey of nearly 900 members of the American Society for Cell Biology (see go.nature.com/kbzs2b). That may be because such conversations are difficult. If experimenters reach out to the original researchers for help, they risk appearing incompetent or accusatory, or revealing too much about their own projects.

A minority of respondents reported ever having tried to publish

IS THERE A REPRODUCIBILITY CRISIS?

Most scientists have experienced failure to reproduce results.




p-hacking
Small *N*s
HARKing
Publication bias

p-Hacking

Study 2: musical contrast and chronological rejuvenation

Using the same method as in Study 1, we asked 20 University of Pennsylvania undergraduates to listen to either "When I'm Sixty-Four" by The Beatles or "Kalimba." Then, in an ostensibly unrelated task, they indicated their birth date (mm/dd/yyyy) and their father's age. We used father's age to control for variation in baseline age across participants.

An ANCOVA revealed the predicted effect: According to their birth dates, people were nearly a year-and-a-half younger after listening to "When I'm Sixty-Four" (adjusted $M = 20.1$ years) rather than to "Kalimba" (adjusted $M = 21.5$ years), $F(1, 17) = 4.92$, $p = .040$.

General Article

aps ASSOCIATION FOR PSYCHOLOGICAL SCIENCE

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SAGE

False-Positive Psychology: Undisclosed Flexibility in Data Collection and Analysis Allows Presenting Anything as Significant

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Abstract

In this article, we accomplish two things. First, we show that despite empirical psychologists' nominal endorsement of a low rate of false-positive findings (≤ .05), flexibility in data collection, analysis, and reporting dramatically increases actual false-positive rates. In many cases, a researcher is more likely to falsely find evidence that an effect exists than to correctly find evidence that it does not. We present computer simulations and a pair of actual experiments that demonstrate how unacceptably easy it is to accumulate (and report) statistically significant evidence for a false hypothesis. Second, we suggest a simple, low-cost, and straightforwardly effective disclosure-based solution to this problem. The solution involves six concrete requirements for authors and four guidelines for reviewers, all of which impose a minimal burden on the publication process.

Keywords

methodology, motivated reasoning, publication, disclosure

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Our job as scientists is to discover truths about the world. We generate hypotheses, collect data, and examine whether or not the data are consistent with those hypotheses. Although we aspire to always be accurate, errors are inevitable.

Perhaps the most costly error is a *false positive*, the incorrect rejection of a null hypothesis. First, once they appear in

Which control variables should be considered? Should specific measures be combined or transformed or both?

It is rare, and sometimes impractical, for researchers to make all these decisions beforehand. Rather, it is common (and accepted practice) for researchers to explore various ana-

Table 3. Study 2: Original Report (in Bolded Text) and the Requirement-Compliant Report (With Addition of Gray Text)

Using the same method as in Study 1, we asked ~~20~~ 34 **University of Pennsylvania undergraduates to listen** only **to either "When I'm Sixty-Four" by The Beatles or "Kalimba"** or "Hot Potato" by the Wiggles. We conducted our analyses after every session of approximately 10 participants; we did not decide in advance when to terminate data collection. **Then, in an ostensibly unrelated task, they indicated** only **their birth date (mm/dd/yyyy) and** how old they felt, how much they would enjoy eating at a diner, the square root of 100, their agreement with "computers are complicated machines," **their father's age**, their mother's age, whether they would take advantage of an early-bird special, their political orientation, which of four Canadian quarterbacks they believed won an award, how often they refer to the past as "the good old days," and their gender. **We used father's age to control for variation in baseline age across participants**.

An ANCOVA revealed the predicted effect: According to their birth dates, people were nearly a year-and-a-half younger after listening to "When I'm Sixty-Four" (adjusted *M* = 20.1 years) rather than to "Kalimba" (adjusted *M* = 21.5 years), *F*(1, 17) = 4.92, *p* = .040. Without controlling for father's age, the age difference was smaller and did not reach significance (*M*s = 20.3 and 21.2, respectively), *F*(1, 18) = 1.01, *p* = .33.

Table 1. Likelihood of Obtaining a False-Positive Result

	Significance level		
Researcher degrees of freedom	$p < .1$	$p < .05$	$p < .01$
Situation A: two dependent variables ($r = .50$)	17.8%	9.5%	2.2%
Situation B: addition of 10 more observations per cell	14.5%	7.7%	1.6%
Situation C: controlling for gender or interaction of gender with treatment	21.6%	11.7%	2.7%
Situation D: dropping (or not dropping) one of three conditions	23.2%	12.6%	2.8%
Combine Situations A and B	26.0%	14.4%	3.3%
Combine Situations A, B, and C	50.9%	30.9%	8.4%
Combine Situations A, B, C, and D	81.5%	60.7%	21.5%

HARKing

"[P]resenting a post hoc hypothesis (i.e., one based on or informed by one's results) in one's research report as if it were, in fact, an a priori hypothesis." (Kerr, 1998)

Open Science

Open Science in Psychology

Jo-Ann Tsang

Baylor University

Example of p-hacking

Different operationalizations of variables can lead to different results

- Predictor: party of President
- Criterion: GDP
- Result: no relationship

- Predictor: party of governor
- Criterion: GDP
- Result: Having more Republicans in office leads to worse economy

- Predictor: party of senator and representatives
- Criterion: Employment
- Result: Having more Republicans in office leads to better economy

https://fivethirtyeight.com/features/science-isnt-broken/#part2

How to Address?

Transparent Language

Preregistration

Registered Report

Transparent Language

- ☆ 21 word solution: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2160588
- ☆ Determine **sample size** in advance. Stopping rule.
- ☆ Data exclusions
- ☆ Report all manipulations
- ☆ Report all measures

Preregistration

- Sample preregistration: Divine Forgiveness
  - https://osf.io/zgrcn

- Lists hypotheses, variables, analysis plan
- Ideally before data are collected; can also be done after data collection but before analyses
- Mention in Methods section of paper

Registered Report

- Submit to journal the Intro & methods.
- Reviewers provide feedback
- Stage 1 in principle acceptance
- Then collect data, analyze, send in manuscript with Results and Discussion
- Stage 2 acceptance if you did what you said you'd do, regardless of statistical significance of results

Registered Report: Religious Privilege Example

- Ways this improved our research
  - Competing hypothesis framing
  - Required more pilot data to make sure our measures and manipulations were adequate
  - Importance of detailed analysis plan
  - Reviews were more helpful than adversarial