How to survive and/or thrive as an open scientist

Tal Yarkoni
University of Texas at Austin

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

Daryl J. Bem
Cornell University

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

Open access, freely available online

Essay

Why Most Published Research Findings Are False

John P. A. Ioannidis

Summary

There is increasing concern that most current published research findings are false. The probability that a research claim is true may depend on study power and bias, the number of other studies on the same question, and, importantly, the ratio of true to no relationships among the relationships probed in each scientific field. In this framework, a research finding is less likely to be true when the studies conducted in a field are smaller; when effect sizes are smaller; when there is a greater number and lesser preselection of tested relationships; where there is greater flexibility in designs, definitions, outcomes, and analytical modes; when there is greater financial and other interest and prejudice; and when more teams are involved in a scientific field in chase of statistical significance. Simulations show that for most study designs and settings, it is more likely for a research claim to be false than true. Moreover, for many current scientific fields, claimed research findings may often be simply accurate measures of the

factors that influence this problem and some corollaries thereof.

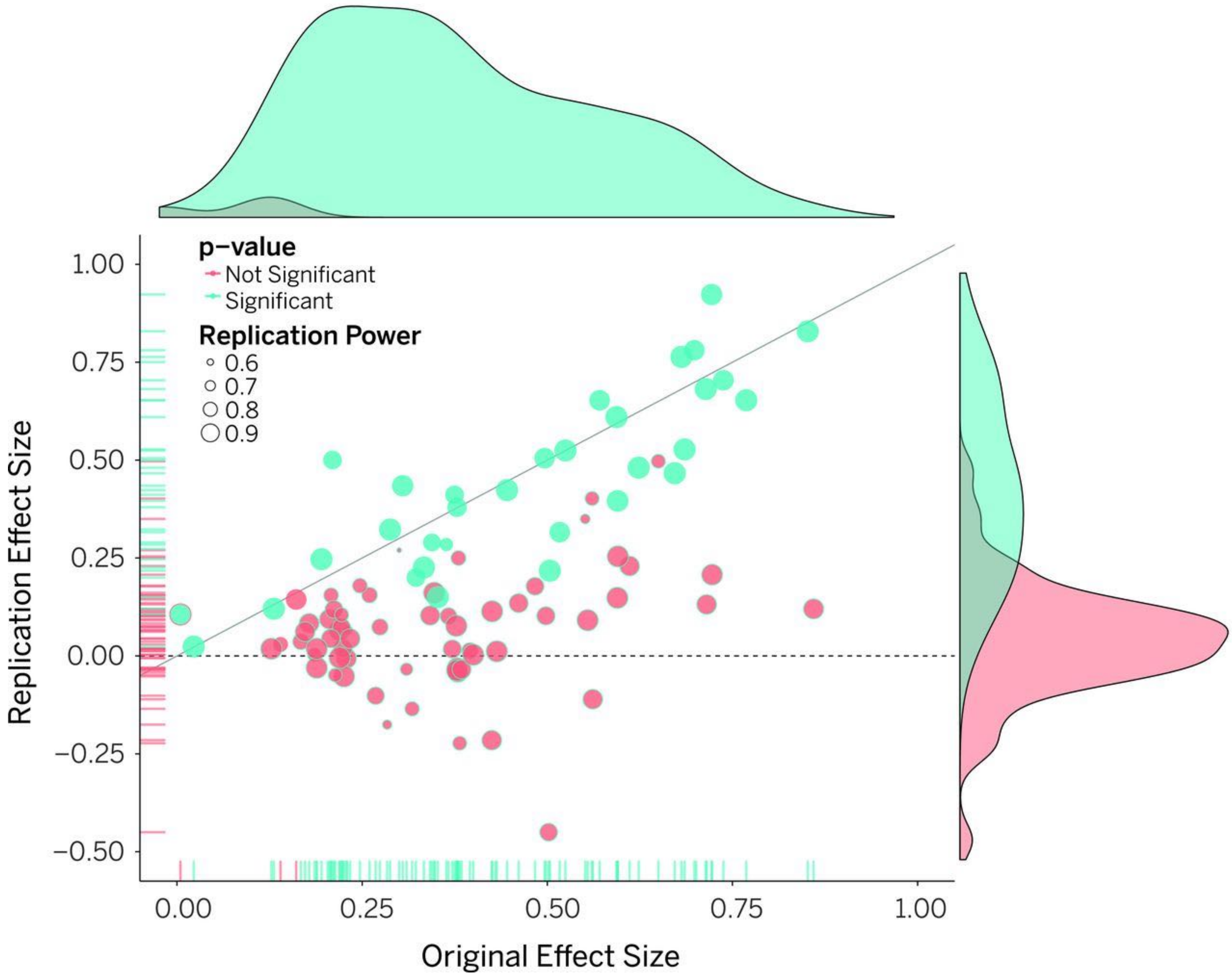
Modeling the Framework for False Positive Findings

Several methodologists have pointed out [9–11] that the high rate of nonreplication (lack of confirmation) of research discoveries is a consequence of the convenient, yet ill-founded strategy of claiming conclusive research findings solely on the basis of a single study assessed by formal statistical significance, typically for a *p*-value less than 0.05. Research is not most appropriately represented and summarized by *p*-values, but, unfortunately, there is a widespread notion that medical research articles

It can be proven that most claimed research findings are false.

should be interpreted based only on *p*-values. Research findings are defined here as any relationship reaching

is characteristic of the field and can vary a lot depending on whether the field targets highly likely relationships or searches for only one or a few true relationships among thousands and millions of hypotheses that may be postulated. Let us also consider, for computational simplicity, circumscribed fields where either there is only one true relationship (among many that can be hypothesized) or the power is similar to find any of the several existing true relationships. The pre-study probability of a relationship being true is $R/(R+1)$. The probability of a study finding a true relationship reflects the power $1-\beta$ (one minus the Type II error rate). The probability of claiming a relationship when none truly exists reflects the Type I error rate, α. Assuming that c relationships are being probed in the field, the expected values of the 2×2 table are given in Table 1. After a research finding has been claimed based on achieving formal statistical significance, the post-study probability that it is true is the positive predictive value, PPV.



Nosek et al. (2015)

How did we get here?

- We just did our jobs the way we were trained to
- Conventional research practices and incentives make the observed consequences almost inevitable

General Article

ASSOCIATION FOR
PSYCHOLOGICAL SCIENCE

Psychological Science
22(11) 1359–1366

DOI: 10.1177/0956797611417632
http://pss.sagepub.com
SAGE

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

Joseph P. Simmons[1], Leif D. Nelson[2], and Uri Simonsohn[1]
[1]The Wharton School, University of Pennsylvania, and [2]Haas School of Business, University of California, Berkeley

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 ($\leq .05$), 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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Research Article

ASSOCIATION FOR PSYCHOLOGICAL SCIENCE

Psychological Science
23(5) 524–532

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Measuring the Prevalence of Questionable Research Practices With Incentives for Truth Telling

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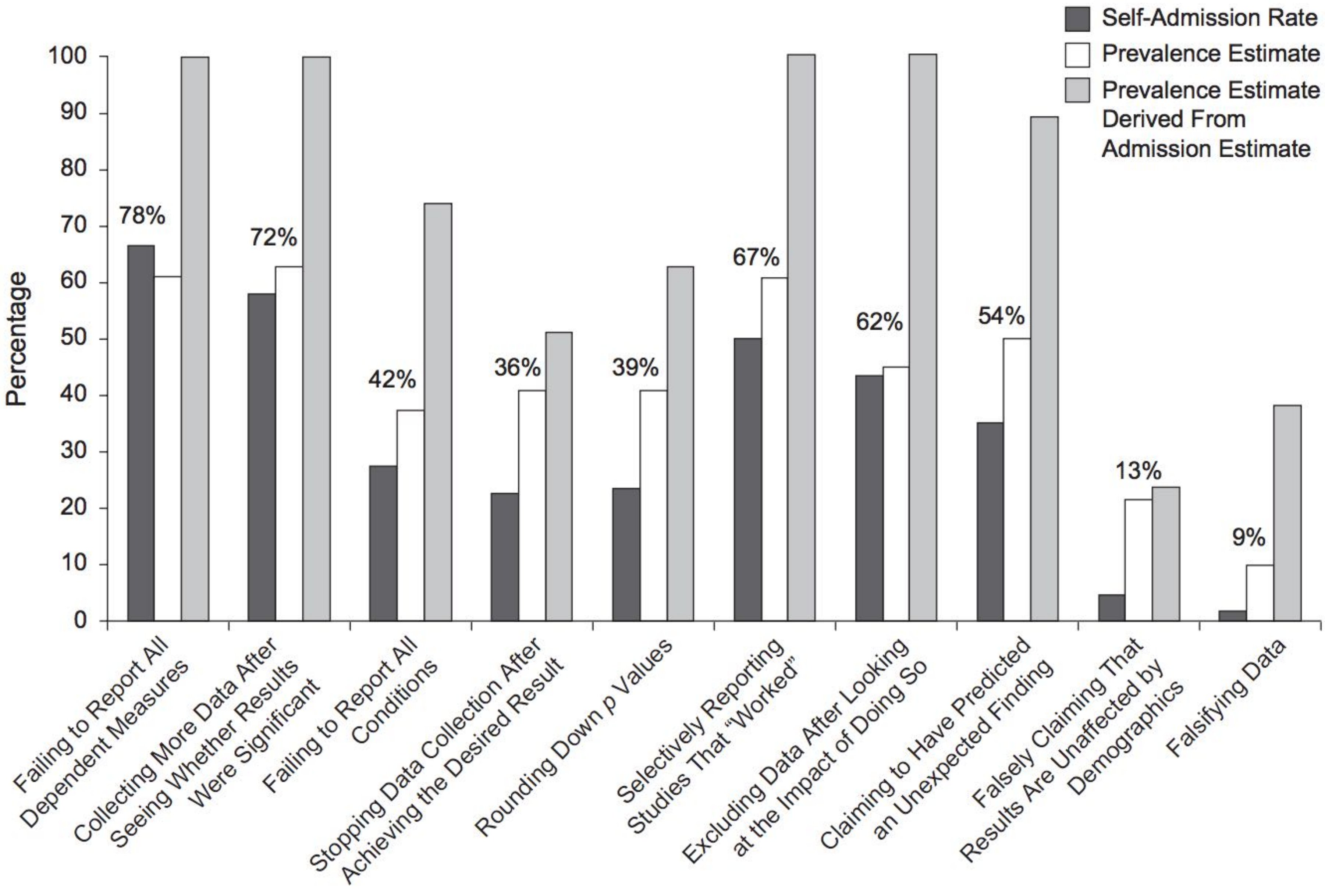
[1]Marketing Unit, Harvard Business School; [2]Department of Social & Decision Sciences, Carnegie Mellon University; and [3]Sloan School of Management and Departments of Economics and Brain & Cognitive Sciences, Massachusetts Institute of Technology

Abstract

Cases of clear scientific misconduct have received significant media attention recently, but less flagrantly questionable research practices may be more prevalent and, ultimately, more damaging to the academic enterprise. Using an anonymous elicitation format supplemented by incentives for honest reporting, we surveyed over 2,000 psychologists about their involvement in questionable research practices. The impact of truth-telling incentives on self-admissions of questionable research practices was positive, and this impact was greater for practices that respondents judged to be less defensible. Combining three different estimation methods, we found that the percentage of respondents who have engaged in questionable practices was surprisingly high. This finding suggests that some questionable practices may constitute the prevailing research norm.

Keywords

professional standards, judgment, disclosure, methodology


Self-Admission Rate
Prevalence Estimate
Prevalence Estimate Derived From Admission Estimate
Percentage
100
90
80
70
60
50
40
30
20
10
0
78%
72%
42%
36%
39%
67%
62%
54%
13%
9%
Failing to Report All Dependent Measures
Collecting More Data After Seeing Whether Results Were Significant
Failing to Report All Conditions
Stopping Data Collection After Achieving the Desired Result
Rounding Down p Values
Selectively Reporting Studies That "Worked"
Excluding Data After Looking at the Impact of Doing So
Claiming to Have Predicted an Unexpected Finding
Falsely Claiming That Results Are Unaffected by Demographics
Falsifying Data

The view from inside

“We initially submitted it with more studies, some of which had weaker results. The editor said to delete those. He wanted the paper shorter so as not to use up a lot of journal space with mediocre results. It worked: the resulting paper is shorter and stronger. … The studies deleted at the editor’s request are not the only story. I am pretty sure there were other studies that did not work. Let us suppose that our hypotheses were correct and that our research was impeccable. Then several of our studies would have failed, simply given the realities of low power and random fluctuations. Is anyone surprised that those studies were not included in the draft we submitted for publication? **If we had included them, certainly the editor and reviewers would have criticized them and formed a more negative impression of the paper**.”

—Roy Baumeister (quoted on https://replicationindex.wordpress.com/2014/12/01/roy-baumeisters-r-index/)

It’s not just psychology

- “Okay,” you say. “Sure. But those are *psychologists*. What do they know. Some of us here are *neuroscientists!* We do *real* science!”
- The problem is almost certainly *much* worse in imaging
- But we’ve been much less introspective about it

Some issues

- Samples are typically small, so overfitting is easy
- The brain is a big place—lots of statistical comparisons
- Our software is complex, hence buggy
- Most datasets support many different analyses
- Preprocessing pipelines are extremely flexible

ANALYSIS

Power failure: why small sample size undermines the reliability of neuroscience

Katherine S. Button[1,2], John P. A. Ioannidis[3], Claire Mokrysz[1], Brian A. Nosek[4], Jonathan Flint[5], Emma S. J. Robinson[6] and Marcus R. Munafò[1]

Abstract | A study with low statistical power has a reduced chance of detecting a true effect, but it is less well appreciated that low power also reduces the likelihood that a statistically significant result reflects a true effect. Here, we show that the average statistical power of studies in the neurosciences is very low. The consequences of this include overestimates of effect size and low reproducibility of results. There are also ethical dimensions to this problem, as unreliable research is inefficient and wasteful. Improving reproducibility in neuroscience is a key priority and requires attention to well-established but often ignored methodological principles.

Despiking

Despiking using AFNI	No despiking

Slice-timing correction

Slice-timing correction	

Spatial no

Normalization of functional images to the SPM EPI template	Normalizatic anatomical i the SPM T1

Spatial s

Smoothing with kernel of 4 mm FWHM	Smoothing v of 8 mm FW

Normalization-modeling order

Normalize before modeling	Model before normalization

High-pass filtering

High-pass filtering using a cutoff of 128 s	No high-pass filtering

Temporal autocorrelation correction

AR(1) modelin	
Runs concatenat before model est	
Hemodynamic response function	Fini tim ver
Six regressors[2]	Twelve regres

	Uncorrected single-voxel threshold	Corrected single-voxel threshold	Cluster size threshold
Monte Carlo @ $p < 0.01$	$p < 0.01$	n/a	Determined by simulation
Monte Carlo @ $p < 0.001$	$p < 0.001$	n/a	Determined by simulation
Monte Carlo @ $p < 0.0001$	$p < 0.0001$	n/a	Determined by simulation
False discovery rate	n/a	$p < 0.05$	n/a
Gaussian random field theory	n/a	$p < 0.05$	n/a

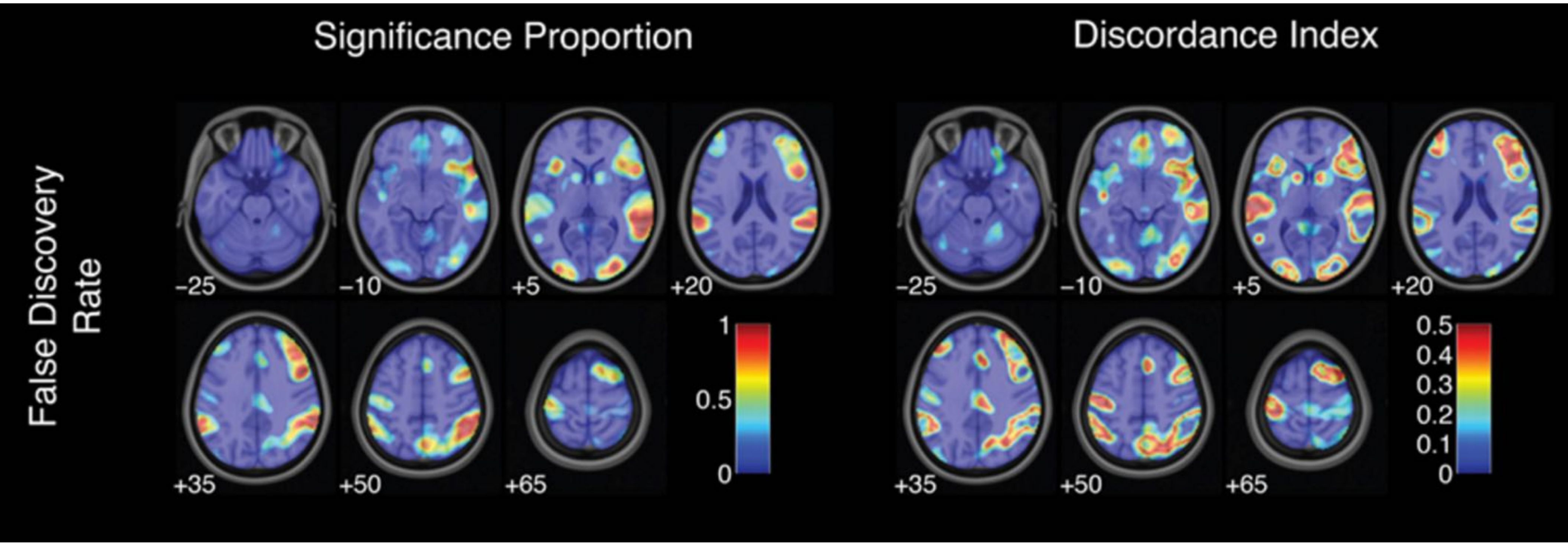
[1] *Eight basis functions.*

[2] *Raw motion parameters.*

[3] *Raw and time-shifted motion parameters.*

[4] *Raw, time-shifted, squared, and time-shifted squared motion parameters.*

Carp (2012)



Significance Proportion
Discordance Index
False Discovery Rate
-25
-10
+5
+20
+35
+50
+65
1
0.5
0
-25
-10
+5
+20
+35
+50
+65
0.5
0.4
0.3
0.2
0.1
0

Carp (2012)

CAN PARAMETRIC STATISTICAL METHODS BE TRUSTED FOR FMRI BASED GROUP STUDIES?

Anders Eklund [a,b,c], *Thomas Nichols* [d], *Hans Knutsson* [a,c]

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Linköping University, Linköping, Sweden
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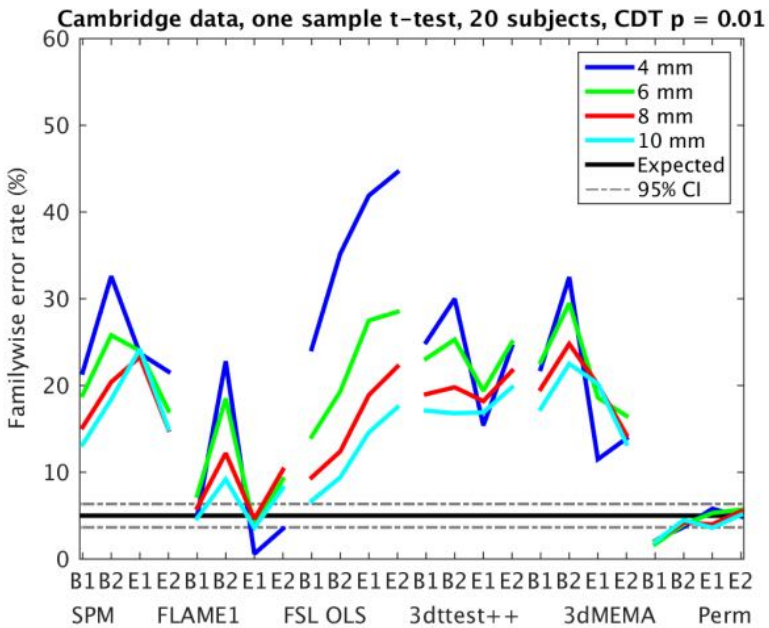
Linköping University, Linköping, Sweden
[c]Center for Medical Image Science and Visualization (CMIV),
Linköping University, Linköping, Sweden
[d]Department of Statistics, University of Warwick, Coventry, United Kingdom

ABSTRACT

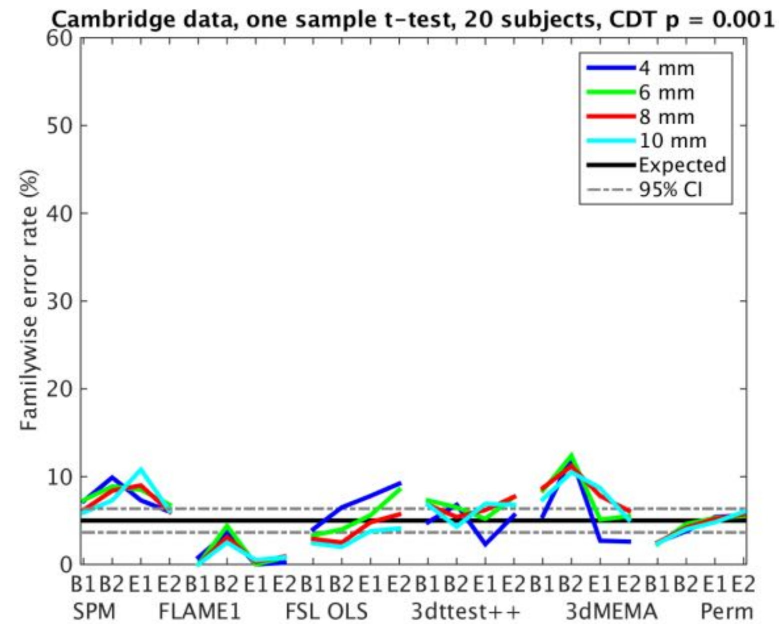
The most widely used task fMRI analyses use parametric methods that depend on a variety of assumptions. While individual aspects of these fMRI models have been evaluated, they have not been evaluated in a comprehensive manner with empirical data. In this work, a total of 2 million random task fMRI group analyses have been performed using resting state fMRI data, to compute empirical familywise error rates for the software packages SPM, FSL and AFNI, as well as a standard non-parametric permutation method. While there is some variation, for a nominal familywise error rate of 5% the parametric statistical methods are shown to be conservative for voxel-wise inference and invalid for cluster-wise inference; in particular, cluster size inference with a cluster defining threshold of p = 0.01 generates familywise error rates up to 60%. We conduct a number of follow up analyses and investigations that suggest the cause of the invalid cluster inferences is spatial auto correlation functions that do not follow the assumed Gaussian shape. By comparison, the non-parametric permutation test, which is based on a small

title or abstract). The first fMRI experiments consisted of simple motor tasks, while more recent examples involve resting state fMRI to study (dynamic) brain connectivity [3, 4]. Despite the popularity of fMRI as a tool for studying brain function, the statistical methods used have rarely been validated using real data, likely due to the high cost of fMRI data collection. Validations have instead mainly been performed using simulated data [5], but it is obviously very hard to simulate the complex spatiotemporal noise that arises from a living human subject in an MR scanner.

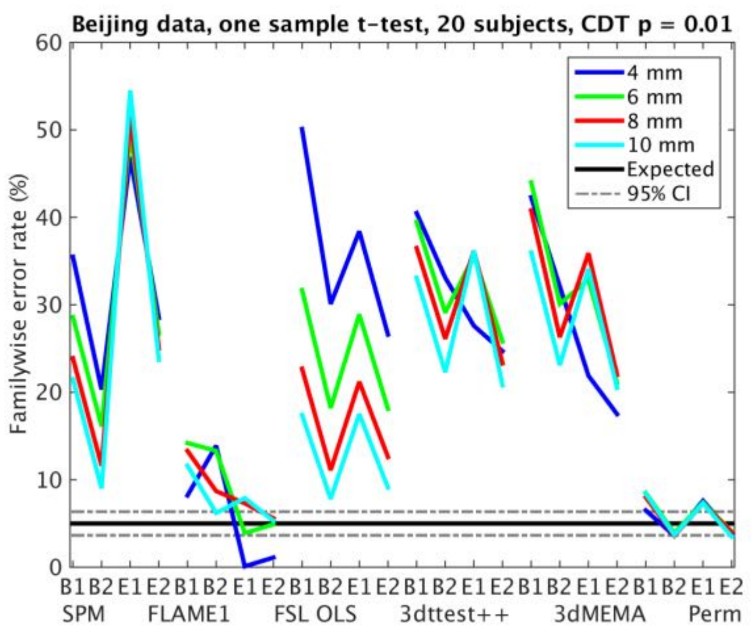
Through the introduction of international data sharing initiatives in the neuroimaging field [6, 7, 8, 9, 10, 11, 12], it has become possible to evaluate the statistical methods using real data. Scarpazza et al. [13] for example used freely available anatomical images from 396 healthy controls [6] to investigate the validity of parametric statistical methods for voxel based morphometry [14], when comparing a single subject to a group. Silver et al. [15] instead used image and genotype data from 181 subjects in the Alzheimer's disease neuroimaging initiative (ADNI) [10, 11]. The data were used to evaluate



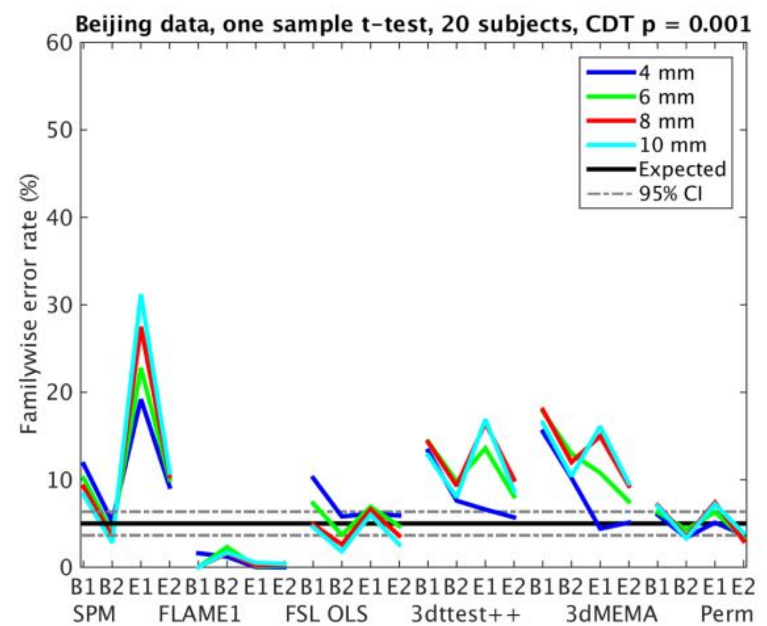
(a)



(b)



(c)



(d)

Eklund, Nichols, & Knutsson (2015)

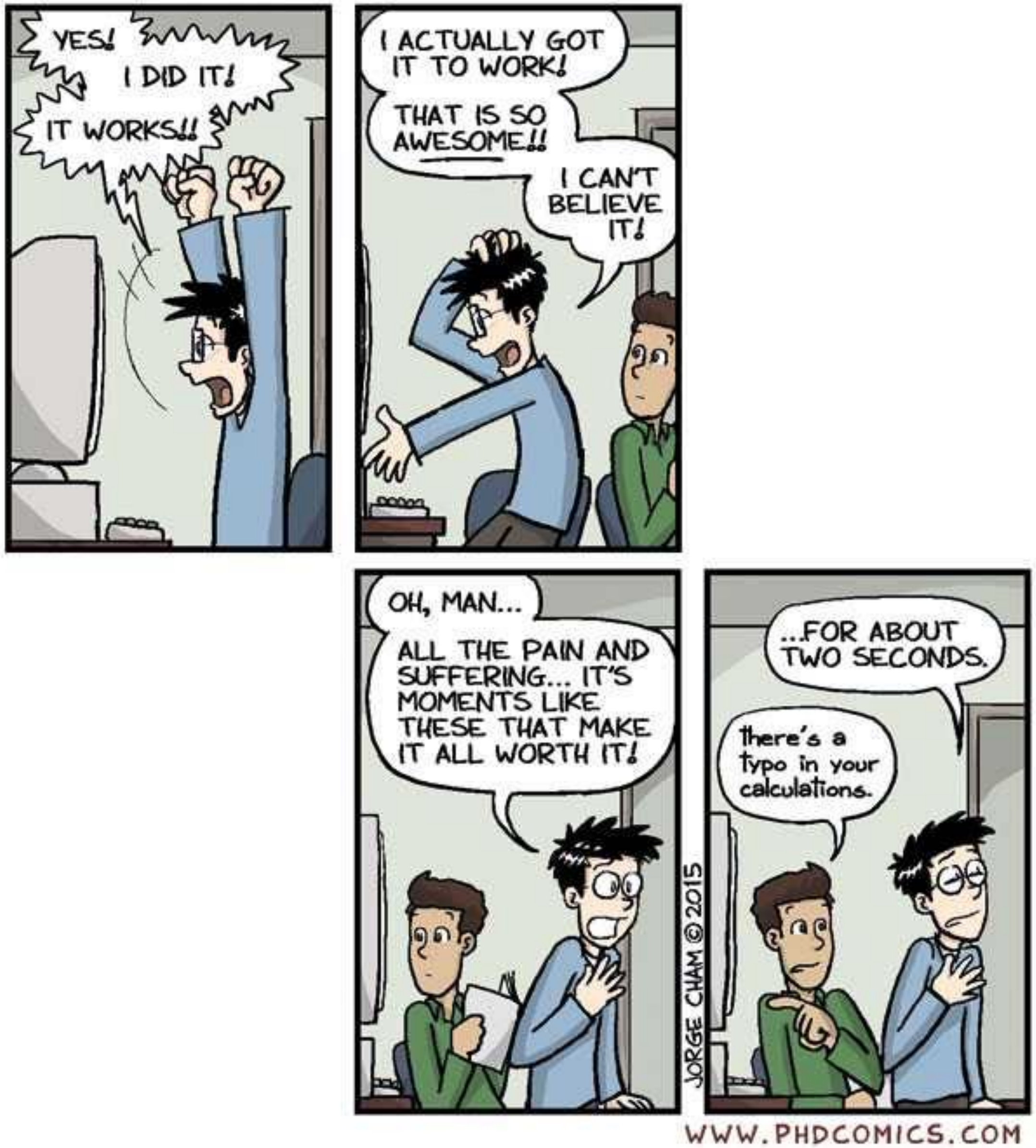
A way forward

- Most of these problems are due in some way to a lack of transparency and openness
- Thought experiment: what would others think of your findings if they saw *everything* you did?

Martyrs need not apply

- Open science is sometimes viewed as a noble but risky cause
    - What good is an open scientist who loses the ability to do science?
- Good news: you can now do open science for completely selfish reasons!
- 6 things you can start doing today

1. Be reproducible



1. Be reproducible

- Your results aren't useful unless they can't be reproduced
- Actively work towards reproducibility:
    - Maintain consistent project structure
    - Document everything
    - Automate as much of your work as possible
    - Maintain everything under version control (e.g., GitHub)
- E.g., Hanke et al. (2015)
    - https://github.com/psychoinformatics-de/paper-f1000_pandora_data
- E.g. Waskom et al. (2014)
    - http://nbviewer.ipython.org/github/WagnerLabPapers/Waskom_JNeurosci_2014/blob/master/Behavioral_and_Decoding_Analyses.ipynb

Benefits of reproducibility

- Sends a signal you believe in your work
- Other people use, extend, and trust your findings more
- Being organized saves time in the long run
- Write fewer apologies and long explanations

Objections

- "People will find my mistakes."
  - If they do, you should thank them for it
  - People are surprisingly understanding
- "It's a lot of work."
  - Yes, but so is everything else we do
  - Usually *saves* time in the long run
- "I don't know how to do that."
  - A legitimate concern—we need to modernize our training

2. Replicate everything

- In theory, replication is a critical part of science
- In practice, direct replication attempts are very rare
  - ~1% of studies (Makel et al., 2012)
- Replicate your own effects early and often
- Selfish benefits:
  - Less likely to publish false findings
  - More impressive results

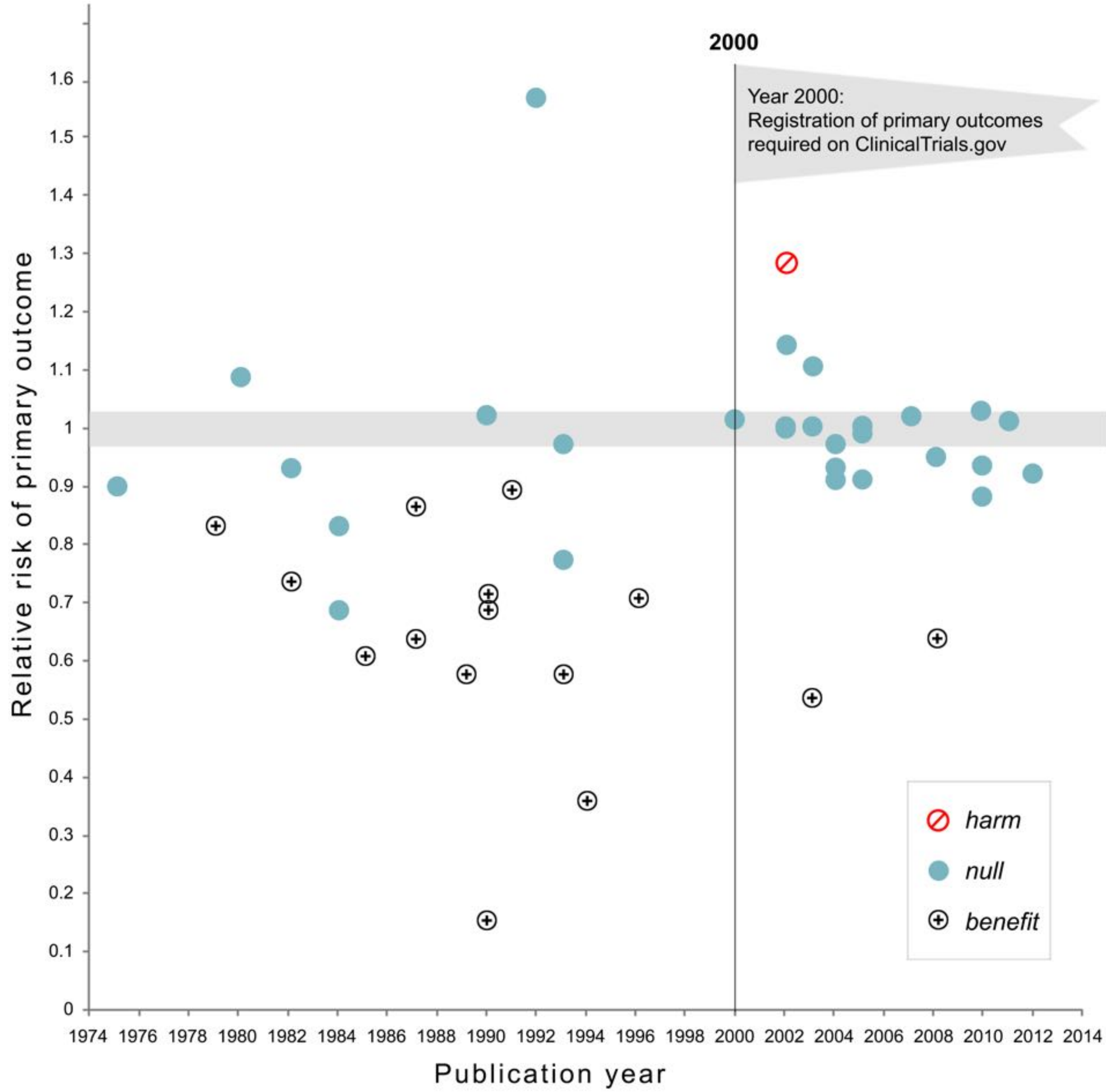
Objections

- “Replication is a lot of work.” (notice a theme?)
  - So is collecting data, writing papers, etc.—but you do all *that*, right?
- “I have no incentive to replicate my own work.”
  - This might be true… for now. But the incentives are changing…

3. Preregister your studies

- HARKing (Hypothesizing After Results are Known) is a major source of the problems we saw earlier (Kerr, 1998)
- One solution is to publicly preregister one's hypotheses and design (Chambers et al., 2014; Nosek et al., 2015)
- Doesn't matter where—as long as there's a timestamp
  - GitHub, OSF.io, etc.
- Selfish benefits:
  - Prevents self-fooling
  - Sends signal that your work can be trusted
  - May actually make it *easier* to publish good work (registered reports)

Think you don't need preregistration?



Kaplan & Irvin (2015)

Objections

- “If I preregister, I can’t explore my data!”
  - Nonsense. You can—and *should* explore your data. You just can’t pretend that exploration is confirmation
- “There’s no incentive—I can only lose!”
  - Registered reports are a pretty good incentive to preregister
  - A successful preregistered study is a tremendous signal of credibility

Registered Reports: A new article format from Cortex

Cortex is pleased to announce the launch of a new innovation in scientific publishing called the Registered Report. Different to established publishing models, Registered Reports divide the review process into two stages. Experimental methods and proposed analyses are pre-registered and reviewed before data are collected. Then, if peer reviews are favourable, authors' articles are accepted in principle. This guarantees publication of their future results providing that they adhere precisely to their registered protocol. Once their experiment is complete, authors then resubmit their full manuscript for final consideration.

Metaphoric structuring: understanding time through spatial metaphors

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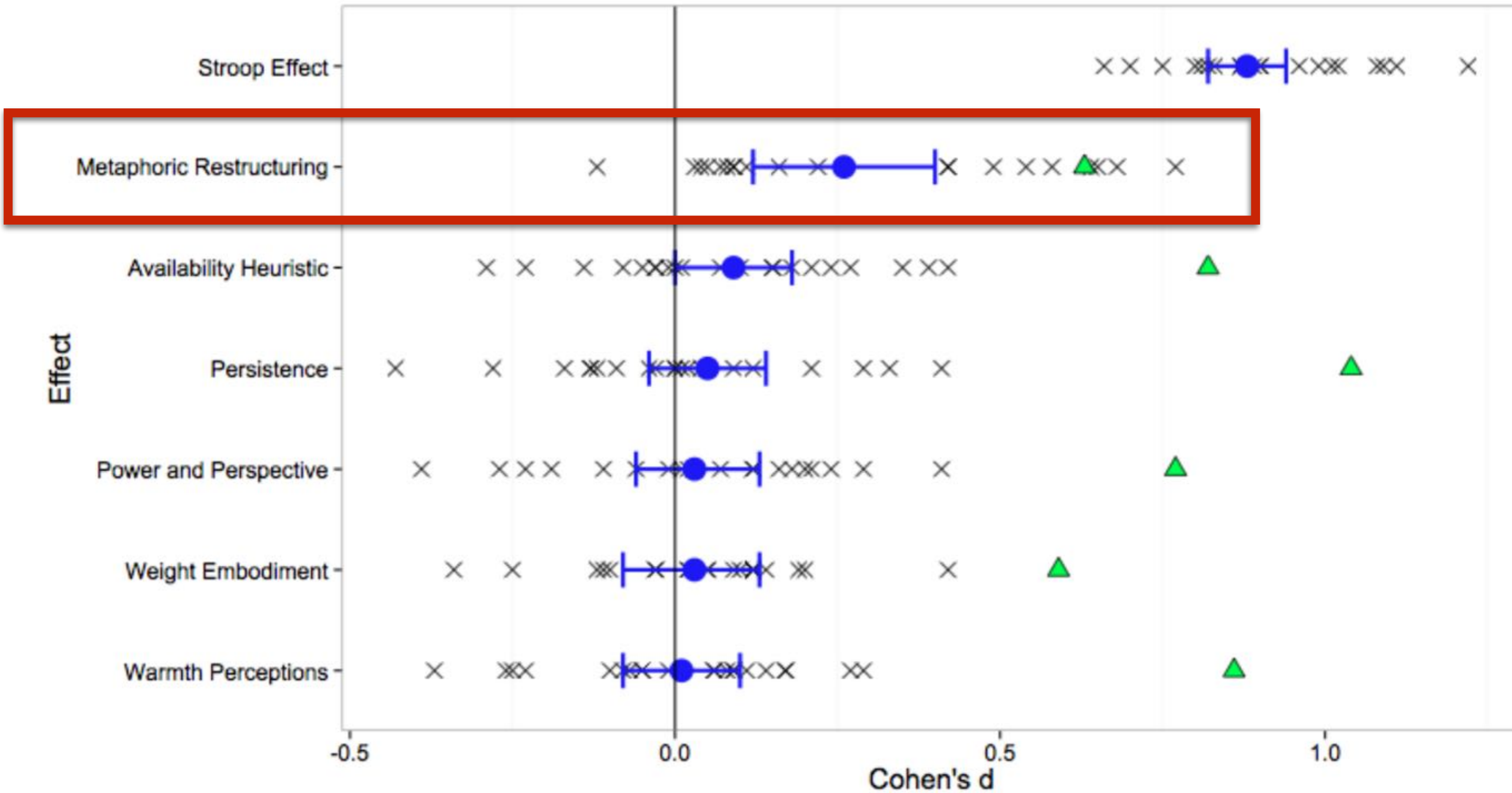
Received 17 December 1998; received in revised form 22 September 1999; accepted 23 October 1999

Abstract

The present paper evaluates the claim that abstract conceptual domains are structured through metaphorical mappings from domains grounded directly in experience. In particular, the paper asks whether the abstract domain of time gets its relational structure from the more concrete domain of space. Relational similarities between space and time are outlined along with several explanations of how these similarities may have arisen. Three experiments designed to distinguish between these explanations are described. The results indicate that (1) the domains of space and time do share conceptual structure, (2) spatial relational information is just as useful for thinking about time as temporal information, and (3) with frequent use, mappings between space and time come to be stored in the domain of time and so thinking about time does not necessarily require access to spatial schemas. These findings provide some of the first empirical evidence for Metaphoric Structuring. It appears that abstract domains such as time are indeed shaped by metaphorical mappings from more concrete and experiential domains such as space.

Keywords: Metaphoric structuring; Understanding time; Spatial metaphors



Ebersole et al. (2015)

4. Publish openly

- Nearly all science is publicly funded, but the vast majority of humanity can't access the results
- Publishing in Open Access (OA) journals remedies this
- Selfish benefits (cf. https://dx.doi.org/10.6084/m9.figshare.1619902.v6)
    - Openly accessible articles have a citation advantage (Eysenbach, 2006; Hajjem, Harnad, & Gingras, 2006)
    - Often faster
    - You retain full copyright

Objections

- "There aren't any good OA journals in my field."
  - Sure there are—if "good" means anything other than "IF > 20"
- "It's too expensive to publish in OA journals."
  - No it isn't: fee waivers, PeerJ.com, etc.
- Don't let the perfect be the enemy of the good

Preprint everything

- Deposit your manuscripts online early on
- Selfish benefits:
  - Build citation advantage
  - Establish precedence
  - Receive feedback
  - Pad CV (“working papers”)
  - Indulge your physics envy

Choosing prediction over explanation in psychology: Lessons from machine learning

19.02.2016, 22:23 (GMT) by Tal Yarkoni, Jacob Westfall

Abstract: Psychology has historically been concerned, first and foremost, with explaining the causal mechanisms that give rise to behavior. Randomized, tightly controlled experiments are enshrined as the gold standard of psychological research, and there are endless investigations of the various mediating and moderating variables that govern various behaviors. We argue that psychology's near-exclusive emphasis on explaining the causes of behavior has led much of the field to be populated by research programs that provide intricate theories of psychological mechanism, but that have little (or unknown) ability to actually predict future behaviors with any appreciable accuracy. We propose that principles and techniques from the field of machine learning can help psychology become a more predictive science. We review some of the fundamental concepts and tools of machine learning and point out examples where these concepts have been used to conduct interesting and important psychological research that focuses on predictive research questions. We suggest that an increased focus on prediction, rather than explanation, can ultimately lead us to greater understanding of behavior.

FUNDING

R01MH096906

CATEGORIES

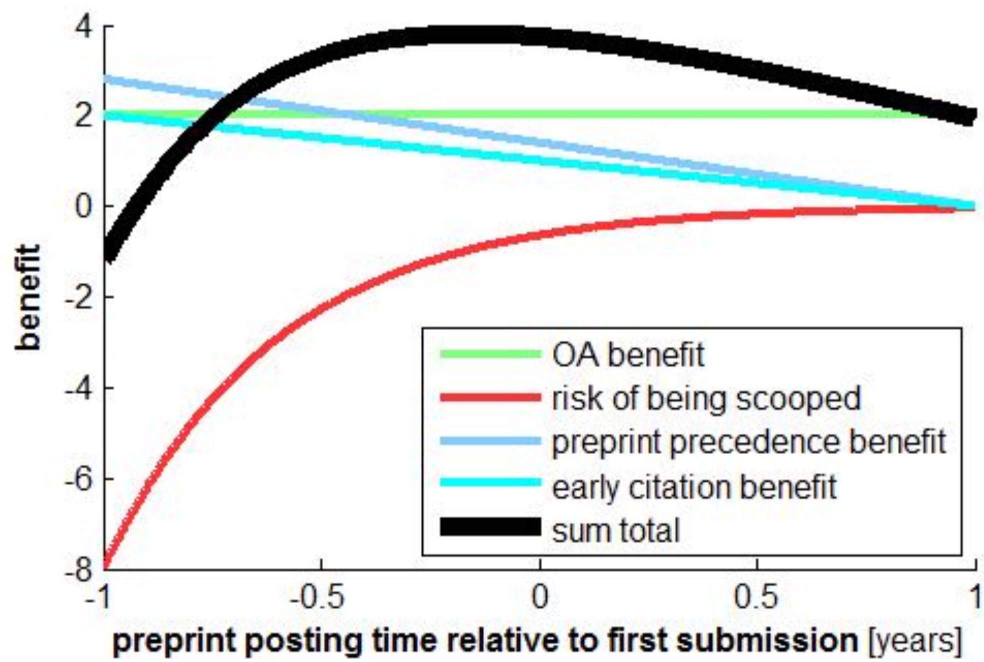
- Psychological Methodology, Design and Analysis

TAGS

explanation · prediction · machine learning · cross-validation · overfitting · Big Data · regularization

Objections

- “Journals will no longer be interested.”
  - No evidence that this is true; most explicitly allow it
  - They may even benefit!
- “I could be scooped!”
  - Possible, but unlikely—and timing is important



http://nikokriegeskorte.org/2016/03/13/the-selfish-scientists-guide-to-preprint-posting/

5.

KEEP
CALM
AND
SHARE
STUFF

Share your stuff

- Give away the products of your labor
    - Data, results, code, etc.
- Selfish benefits:
    - More citations (Piwowar, Day, & Fridsma, 2007; Piwowar & Vision, 2013)
    - Greater visibility and influence
    - Sends positive signal to others
    - Less work required to respond to requests
    - No guilt for blatant violation of contractual agreements

- [ ] I have read the Instructions to Authors contained on the Journal of Applied Psychology's website (www.apa.org/pubs/journals/apl).
- [ ] I have included a cover letter in the cover letter text box that contains the names and contact information of all authors. Inclusion of such information indicates that these individuals have agreed to be an author.
- [ ] I attest that this manuscript is not being considered by another journal nor has it been published elsewhere ***and*** that the data on which the manuscript is based are original.
- [ ] Or: If the manuscript has been previously published in conference proceedings I have explained this in my cover letter.
- [ ] I attest that the data on which this manuscript is based are original data and have not been previously published. I further attest that the data were not simultaneously collected from the same sample along with other data (e.g., data that are parsed, collected at different time points etc.) that have been published or that I intend to subsequently submit for publication.
- [ ] Or: Multiple uses of data collected from the same sample (e.g., data that overlap, data that are parsed or collected at different time points, etc.) have the potential to constitute "duplicate publication." If applicable, I have provided information in my cover letter about related manuscripts that have been, are, or will be submitted for consideration to the same or another journal. I have also edited the manuscript to be transparent with respect to prior, current, and / or any additional uses of the data set. In order to maintain masked review, I have incorporated a table to provide data transparency, following the sample format provided.
- [ ] I verify that if this manuscript describes a study wherein humans were participants, the treatment of those participants was in accordance with established ethical guidelines and appropriate institutional approval has been obtained.
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PLoS one

Willingness to Share Research Data Is Related to the Strength of the Evidence and the Quality of Reporting of Statistical Results

Jelte M. Wicherts*, Marjan Bakker, Dylan Molenaar

Psychology Department, Faculty of Social and Behavioral Sciences, University of Amsterdam, Amsterdam, The Netherlands

Abstract

Background: The widespread reluctance to share published research data is often hypothesized to be due to the authors' fear that reanalysis may expose errors in their work or may produce conclusions that contradict their own. However, these hypotheses have not previously been studied systematically.

Methods and Findings: We related the reluctance to share research data for reanalysis to 1148 statistically significant results reported in 49 papers published in two major psychology journals. We found the reluctance to share data to be associated with weaker evidence (against the null hypothesis of no effect) and a higher prevalence of apparent errors in the reporting of statistical results. The unwillingness to share data was particularly clear when reporting errors had a bearing on statistical significance.

Conclusions: Our findings on the basis of psychological papers suggest that statistical results are particularly hard to verify when reanalysis is more likely to lead to contrasting conclusions. This highlights the importance of establishing mandatory data archiving policies.

Citation: Wicherts JM, Bakker M, Molenaar D (2011) Willingness to Share Research Data Is Related to the Strength of the Evidence and the Quality of Reporting of Statistical Results. PLoS ONE 6(11): e26828. doi:10.1371/journal.pone.0026828

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Competing Interests: The authors have declared that no competing interests exist.

* E-mail: j.m.wicherts@uva.nl

Objections

- “It’s a lot of effort.”
  - So is collecting data and writing Methods sections
- “I could get scooped!”
  - You should be so lucky!
  - Odds are much better that someone will cite you and thank you
- “There are privacy/confidentiality issues.”
  - Sure—not *every* dataset can be shared

6. Use social media

- A lot of good science happens on Twitter, Facebook, blogs, and other social media channels
- Learn from others
- Discuss with others
- Promote your work

Where my blog traffic comes from

	Source	Acquisition			Behavior		
		Sessions	% New Sessions	New Users	Bounce Rate	Pages / Session	Avg. Session Duration
		40,033 % of Total: 41.75% (95,883)	78.18% Avg for View: 79.68% (-1.88%)	31,298 % of Total: 40.97% (76,401)	84.88% Avg for View: 83.79% (1.30%)	1.22 Avg for View: 1.22 (0.05%)	00:01:02 Avg for View: 00:01:04 (-2.64%)
1.	news.ycombinator.com	**13,501** (33.72%)	90.15%	12,171 (38.89%)	88.59%	1.09	00:00:44
2.	t.co	**6,857** (17.13%)	70.06%	4,804 (15.35%)	81.68%	1.22	00:01:12
3.	facebook.com	**2,842** (7.10%)	59.11%	1,680 (5.37%)	82.58%	1.20	00:01:14
4.	m.facebook.com	**1,777** (4.44%)	83.12%	1,477 (4.72%)	86.38%	1.17	00:01:03
5.	rank-checker.online	**1,434** (3.58%)	77.89%	1,117 (3.57%)	96.30%	1.74	00:00:31
6.	talyarkoni.org	**1,152** (2.88%)	0.17%	2 (0.01%)	58.59%	1.72	00:02:33
7.	reddit.com	**867** (2.17%)	88.58%	768 (2.45%)	86.04%	1.14	00:01:01
8.	quant-econ.net	**615** (1.54%)	87.48%	538 (1.72%)	69.76%	1.57	00:02:44
9.	neurosynth.org	**473** (1.18%)	75.48%	357 (1.14%)	73.57%	1.38	00:00:49
10.	4webmasters.org	**421** (1.05%)	90.74%	382 (1.22%)	98.81%	1.01	00:00:05

Micah Allen

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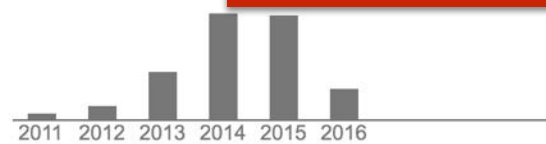

Title 1–11	Cited by	Year
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Anterior insula coordinates hierarchical processing of tactile mismatch responses		2016

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- Chonggang Wang


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FOLLOWING
3,500
FOLLOWERS
17.9K
LIKES
6,930
LISTS
15
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Micah Allen @neuroconscience · Mar 21
Got a null finding? Support publishing them? Let the world know - tweet your null findings at #BringOutYerNulls!

Objections

- “It takes a lot of time to build up a reputation.”
  - Yes, just like it does with publishing
- “I don’t have the time.”
  - O RLY
- “I might offend someone important.”
  - Do you turn down colloquium invitations for the same reason?
- “It’s not considered a real contribution.”
  - Most of the benefits don’t require explicit acknowledgment; you reap them whether or not people realize it

Do the overall benefits outweigh the costs?

- In any individual case, it’s impossible to say
- The variance is clearly high
- But on average, practicing open science now seems like a net positive to one’s career
- Remember: it’s not all or nothing!
    - Some things are easier than others to do
    - Do whatever works for you

And supposing they don’t…

- What if you do all of this, and still don’t get a job?
  - Sadly, that’s the most likely outcome no matter what you do
- A good question to ask yourself is: “do I want to spend my career doing research I don’t really believe in?”
  - It won’t get easier to make a change later