ExpEcon Methods:
Ethical & Unethical Research Practices

ECON 8877
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Replication Crises & Data Colada

- Replication crisis in psychology & social science: mid-2010s
  - Concerns had been floating around since the 1960s...
  - Social Psych hit especially hard
- Replication projects re-running existing experiments
  - Nosek et al. (2015): Only 36% of results replicated!!
    - Social psych: 25%
    - Cognitive psych: 50%
  - Camerer et al. (2016): Experimental economics papers
    - 11 of 18 (61%) replicated
- Data Colada blog identified systemic problems
  - Co-authored by data sleuths, notably Uri Simonsohn
  - Identified outright fraud by several famous economists

Dishonesty in Research

- There are two widely recognized types of research-driven publication bias “dishonesty”
- Selection Problems: The “file drawer effect”
    - Studies with nonsignificant effects have lower publication rates
- Inflation Bias: “p-hacking” or “selective reporting”
    - Strategic reporting of favorable specifications/results

Do these only come from maliciously fraudulent researchers? NO!

File Drawer Bias

- Assuming the Null is true, if 100 studies are performed, 5 of them should yield statistically significant results
- If only these 5 are sent in for publication, then the community may believe that these are indicative of the true effect, while in fact they are not
- Many researchers have huge budgets, and can carry out many studies, and put the ones that do not produce significant results in the file drawer
- How to correct?
    1. Replication by self or others.
    2. Requiring robustness checks.
    3. Incentives to publish null results and replication studies (JESA)

P-Hacking

“P-Hacking”: unethical techniques to try to get a significant result

“False-Positive Psychology: Undisclosed Flexibility in Data Collection and Analysis Allows Presenting Anything as Significant” by Simmons, Nelson, and Simonsohn

- Researchers have a lot of flexibility in their analyses:
    A. Choosing the best dependent variables/outcome measures
    B. Adding to the sample size if p-value is “close”
    C. Adding/removing covariates (gender, IQ, etc.)
    D. Discarding “outliers” or even treatments ex-post

They simulate some of these “tricks” for a hypothetical study:

Table 1. Likelihood of Obtaining a False-Positive Result

Researcher degrees of freedom	Significance level		
	$p < .1$	$p < .05$	$p < .01$
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%

Illustration: Combining Pilots With Data

A simulation:

1. Run a pilot with n_p subjects
    - Generate n_p observations of X_i^p and Y_i^p from $N(0, 1)$
    - Run a t-test on X^p vs Y^p
    - $\approx$ 5% will (wrongly) reject H_0

2a. If pilot fails to reject, stop the project! It's a dud

2b. If pilot rejects, run full sample with n_s subjects
    - Generate n_s observations of X_i^s and Y_i^s from $N(0, 1)$
    - Two options:

Ethical:	Analyze new samples only: X^s vs. Y^s. Throw away the pilot.
Unethical:	Analyze combined samples: (X^p, X^s) vs. (Y^p, Y^s)

Repeat this 10,000 times.
How bad will it be?

Simulation Results

Simulation #:	1	2	3
Pilot n_p:	100	100	500
Sample n_s:	100	500	100
# Pilots:	10,000	10,000	10,000
% Pilots that Reject:	0.0483	0.0511	0.0463
# Continued Studies:	483	511	463
% Reject (New Data Only):	0.056	0.053	0.048
% Reject (Combined Data):	0.354	0.160	0.631

You're selectively picking only pilots with false positives!

What about checking your data?

Entirely hypothetical question:

- Suppose you're a nervous young researcher
- Maybe your experiment software has a bug!!
- So, you run 50 subjects on Prolific to make sure it works
- If it looks okay, you run 300 more

Is this a problem? (discuss)

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Is this a problem? (discuss)
No, as long as either

1. your stop/go decision doesn't depend on the statistical test result (just on "data quality"), or
2. you throw away the first 50 subjects

Unethical Sequential Sampling

More generally:
Consider the following (unethical) sampling algorithm:
Parameters: Sample sizes: n, n_a. Thresholds: $\bar{p} > 0.05$, $\bar{n} > n$

1. Collect n initial observations each of X and Y
   - Suppose the null is true. e.g. $X, Y \sim N(0, 1)$
2. Run test. If $p < 0.05$, stop. You win! H_0 is rejected! Publish!
   - If $p > \bar{p}$, give up. It's hopeless. You lose. File drawer.
3. Otherwise, add another n_a subjects to each treatment
   - If $n + n_a > \bar{n}$, you ran out of money, so you lose. File drawer.
   - Otherwise, repeat with $n = n + n_a$ and try again!

How bad can it be?

Unethical Sequential Sampling

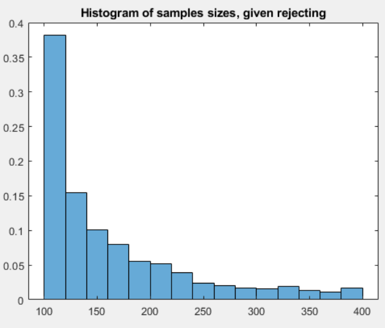
Rejection frequencies, varying give-up $\bar{p}$

Simulation #:	1	2	3
Initial n:	100	100	100
Added n_a:	10	20	20
Max $\bar{n}$:	200	200	400
$\bar{p} = 0.10$	0.0665	0.0666	0.0649
$\bar{p} = 0.15$	0.0785	0.0779	0.0744
$\bar{p} = 0.20$	0.0914	0.0843	0.0907
$\bar{p} = 0.30$	0.1011	0.0991	0.1023
$\bar{p} = 0.50$	0.1172	0.1117	0.1378

1. False positives clearly increasing in $\bar{p}$ (give-up threshold)
2. Increasing $n_a \Rightarrow$ fewer tries $\Rightarrow$ fewer false positives
3. But increasing n_a and $\bar{n}$ together (1 vs. 3) $\Rightarrow$ depends on $\bar{p}$?

Unethical Sequential Sampling

How much do you spend? ($n = 100$, $n_a = 20$, $\bar{n} = 400$, $\bar{p} = 0.50$)

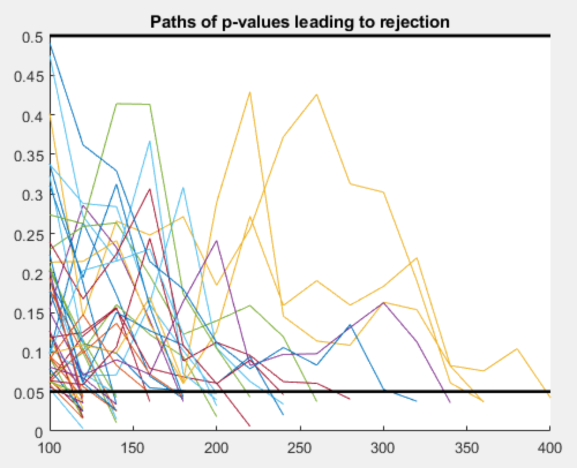


Avg: 154 subjects. Median: 120 subjects

Quit because $p > 0.50$: 83%
Quit because $n > 400$: 3.6%

Unethical Sequential Sampling

Paths of p-values that led to rejection:
$(n = 100, n_a = 20, \bar{n} = 400, \bar{p} = 0.50)$



Ethical Sequential Sampling

- There are ethical sequential sampling procedures...
- Wald's Sequential Probability Ratio Test
  - Requires 2 specific, parameterized hypotheses
  - Ex: H_0: $N(0,1)$ vs H_1: $N(1,1)$
  - Let $p(x_i|0)$ and $p(x_i|1)$ be likelihoods of x_i under each
  - Likelihood ratio of H_1 for data vector $x = (x_1, \ldots, x_n)$:

$$\frac{p(x_1|1)\, p(x_2|1) \;\cdots\; p(x_n|1)}{p(x_1|0)\, p(x_2|0) \;\cdots\; p(x_n|0)} \to \sum_i log\left(\frac{p(x_i|1)}{p(x_i|0)}\right)$$

  - Under H_0, compare to test error ratio:

$$\frac{p(x_1|1)\, p(x_2|1) \;\cdots\; p(x_n|1)}{p(x_1|0)\, p(x_2|0) \;\cdots\; p(x_n|0)} = \frac{\beta}{1-\alpha} \to \log\left(\frac{\beta}{1-\alpha}\right)$$

  - Collect data *sequentially*, monitoring the total log-likelihood ratio
  - If it falls below $a = \log(\beta/(1-\alpha))$, accept H_0
  - If it rises above $b = \log((1-\beta)/\alpha)$, accept H_1
- $\exists$ a sequential test for a single hypothesis?

Table 2. Simple Solution to the Problem of False-Positive Publications

Requirements for authors

1. Authors must decide the rule for terminating data collection before data collection begins and report this rule in the article.
2. Authors must collect at least 20 observations per cell or else provide a compelling cost-of-data-collection justification.
3. Authors must list all variables collected in a study.
4. Authors must report all experimental conditions, including failed manipulations.
5. If observations are eliminated, authors must also report what the statistical results are if those observations are included.
6. If an analysis includes a covariate, authors must report the statistical results of the analysis without the covariate.

Guidelines for reviewers

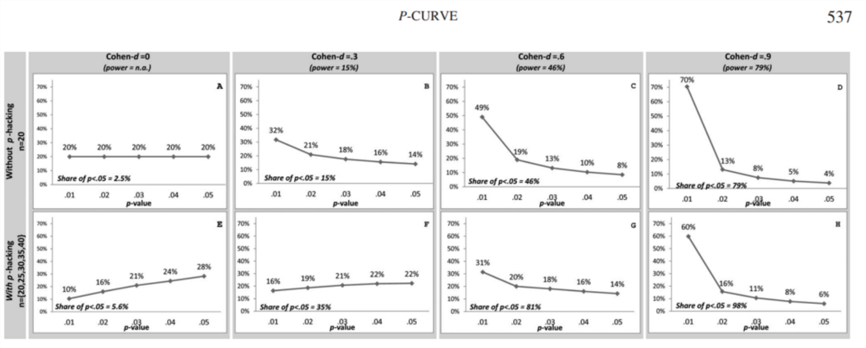
1. Reviewers should ensure that authors follow the requirements.
2. Reviewers should be more tolerant of imperfections in results.
3. Reviewers should require authors to demonstrate that their results do not hinge on arbitrary analytic decisions.
4. If justifications of data collection or analysis are not compelling, reviewers should require the authors to conduct an exact replication.

Evidence for P-hacking

How to identify P-hacking?

- "P-Curve: A Key to the File-Drawer" by Simonsohn, Nelson, and Simmons
- Look at the distribution of p-values in a literature
- What should the distribution look like below 0.05??
    - Red flag: lots of values just below 0.50
        - That shouldn't happen naturally!

P-Curve under certain distributions



Look for “uphill” or “flattened” curves

A demonstration

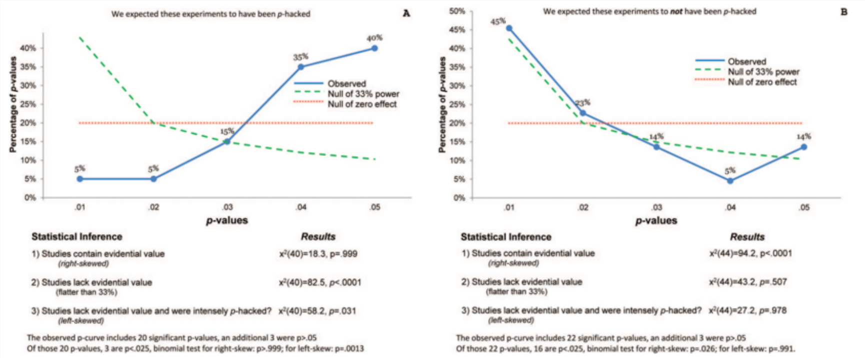


Figure 3. P-curves for *Journal of Personality and Social Psychology (JPSP)* studies suspected to have been *p*-hacked (A) and not *p*-hacked (B). Graphs depict *p*-curves observed in two separate sets of 20 studies. The first set (A) consists of 20 *JPSP* studies that only report statistical results from an experiment with random assignment, controlling for a covariate; we suspected this indicated *p*-hacking. The second set (B) consists of 20 *JPSP* studies reported in articles whose full text does not include keywords that we suspected could indicate *p*-hacking (e.g., *exclude*, *covariate*).

Red flag: papers that add controls when treatment was random

Specification Curve Analysis

Simonsohn, Simmons, Lennon (2020)

- Report all results of all sensible specifications. Meaning:
  1. a sensible test of the research question,
  2. expected to be statistically valid, and
  3. not redundant with the other tests reported.
- Similar to applied micro's table of regressions

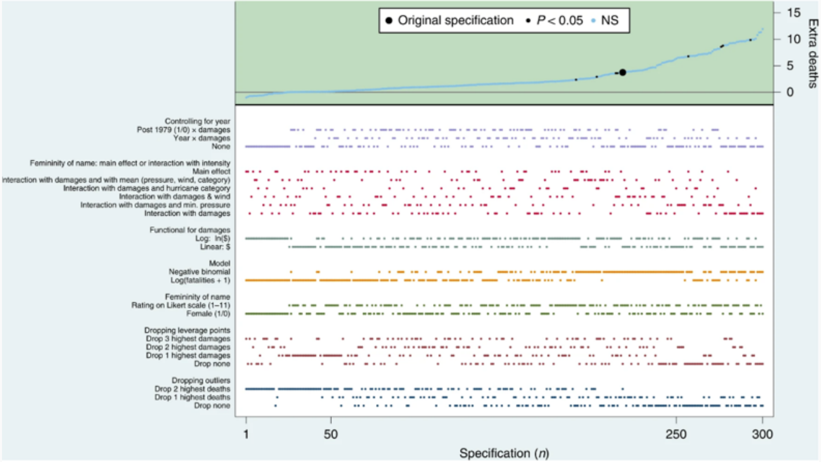
Specification Curve

- Your regression specification:

$$y = F(x; Z) + \epsilon$$

- Lots of degrees of freedom!
  - Different y (wealth, education...)
  - Different F (linear, polynomial...)
  - Different x (treatments, covariates...)
  - Different Z (gender, race, education...)
  - You can easily generate 100+ specifications

Example: "Hurricanes with female names cause more damage"



Each dot in the top panel (green area) depicts the marginal effect, estimated at sample means, of a hurricane having a female rather than male name; the dots vertically aligned below (white area) indicate the analytical decisions behind those estimates. A total of 1,728 specifications were estimated; to facilitate visual inspection, the figure depicts the 50 highest and lowest point estimates and a random subset of 200 additional ones, but the inferential statistics for specification curve analysis include all 1,728 specifications. NS, not significant.

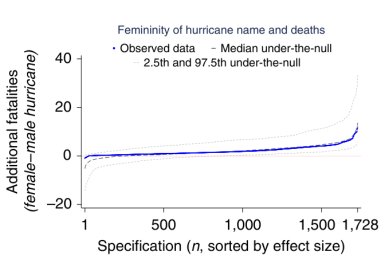
Top: Marginal effects of female name on extra deaths. Most are N.S.

Bottom: dots show specification choices for points on the line above

How To Analyze This?

Bootstrapping!

1. Reshuffle the hurricane names, but nothing else
2. Run the specification curve on the bootstrapped sample
3. Repeat many times, plot each curve



Median effect size (across specifications) using true names: 1.56.
% Bootstrapped medians $> 1.56 = 0.536 \leftarrow p$-value
(Can use "% significant specifications" instead of median effect size)

“Design Hacking”

“Design Hacking” (my term)
File-drawer bias can happen “within” a project as well:

- Try one design, throw it on Prolific, get a null result
- Tweak your design, keep trying, until finally you reject the null

Obviously problematic:

1. The design that works is likely to be a false positive
2. Even if it’s not, it’s clearly not robust

Solution (again): replications and robustness checks!!

Deception

Another ethical issue: Deception

- What counts?
  - Lying to subjects
  - Surprise treatments/questions?
  - Hiding information from subjects???
- Blatant deception unlikely to publish in Econ
  - Vernon credits Sidney Siegel for this norm
  - Really implemented by Plott and Smith, others
- Why? Loss of control of subjects' beliefs

Deception

Charness, Samek, and van Den Van (2022)

- Survey of experimental econ researchers
- What counts as deception?
- 788 of 1554 responded
- Also surveyed experiment participants

Scenario	Researcher text
S1: Subgroup re-match	In a multi-period experiment, the experimenter tells the participants that they will be randomly matched every period, but in fact the participants are only re-matched (for statistical purposes) within a subgroup of the participants
S2: Surprise re-start	Participants in an experiment are told that there will be 10 periods in the session, but are then told that there will be another 10 periods (a surprise re-start)
S3: Non-representative sample	The experimenter tells the participants the average value of the choices or beliefs of "a sample of the other participants", but doesn't mention that this is not a representative sample (and states other averages to other participants)
S4: Unexpected data use	The experimenter uses participant responses in a way that is not revealed to the participant: for example, (1) participants are incentivized to predict behavior of other people, but are not told that these predictions will be shown to others, or (2) participant data from one part of the experiment is used to sort participants into groups in another part of the experiment
S5: Confederates	The experimenter uses either confederates or computers that do not operate of their own volition, but instead behave as scripted by the experimenter. The experimenter does not tell subjects that confederates or computers are involved in the experiment
S6: Unknown/unpaid participation	The experimenter conducts a field experiment that encourages people to put forth (unpaid) effort or take action, but does not inform the participants that they are in an experiment
S7: Misinterpretation	The experimenter relies upon the assumption that participants will misinterpret the instructions [e.g., using the term "random" when the probabilities are actually 75% and 25% and when it is essential that they believe that this was truly random (i.e., 50%)]

Scenario	Deceptive (1–7)	Negative (1–7)	Appropriate (1–7)	Useful (1–7)
Unexpected data use	3.18	2.94	5.19	4.96
	(0.07)	(0.07)	(0.06)	(0.06)
Subgroup re-match	3.20	3.01	5.00	4.64
	(0.07)	(0.08)	(0.07)	(0.07)
Unknown/unpaid participation	3.23	2.85	5.25	
	(0.08)	(0.07)	(0.07)	
Non-representative sample	3.76	3.42	4.76	4.40
	(0.07)	(0.07)	(0.07)	(0.07)
Surprise re-start	3.88	3.45	4.75	4.41
	(0.07)	(0.07)	(0.07)	(0.07)
Misinterpretation	4.78	4.58	3.70	
	(0.07)	(0.07)	(0.07)	
Confederates	5.33	4.79	3.88	4.07
	(0.07)	(0.07)	(0.07)	(0.07)
Total	3.91	3.58	4.65	4.50
	(0.03)	(0.03)	(0.03)	(0.03)

Mean ratings. Items are rated on a 7-point scale, ranging from 1 (“not at all”) to 7 (“extremely”).

Questions asked:

1. How **deceptive** is it? 2. Would you feel **negative** as a referee?
3. How **appropriate** is it if $\nexists$ alternative? 4. How **useful** is it?

My View

What do you think?

My View

My view:

- All that matters is whether subjects will believe the instructions next time they come to an experiment
  - This is a public good!
  - Ethical issues matter, but this conservative approach covers them
- My assumption: Likelihood that they care/notice is driven by likelihood that they regret their former actions
  - Example: Testing Gang-of-Four with a surprise restart
  - “Regret-inducing surprise”
  - “Regret-free” deception *might* be okay, but still risky!
- Isn’t it okay if they don’t find out?
  - How sure are you? What if they talk?
  - What if they read our papers?
- I think it’s rare that you *must* use deception

Experimenter Demand Effects

Smaller but pervasive issue: Experimenter Demand Effects

- Altering choices through framing/display
  - Example: Preference for Randomization
- Or, making it obvious what's the research question
  - Ex: Gender study, only ask about gender
- Directional effect may be unclear!
- Raises deeper questions about:
  1. What is a preference? Depends on framing?
  2. What does it mean to have "external validity"?
-

Can We Reduce Experimenter Demand Effects?

- Incentives: Vernon's "Dominance"
  - Camerer: larger stakes reduce noise
- Neutral framing/instructions
  - But isn't "neutral" just another frame??
- Reducing interaction with the experimenter
  - Read-alone instructions? Video?
- My view: every frame alters preferences.
  - There is no "neutral frame" or "true preference"
  - So just document the framing you used
  - Future researchers can test robustness

The de Quidt et al (2017) Method

de Quidt et al. (2017)
Example: effect of incentives on effort

1. Run original design as planned.
    - Control (0): no pay
    - Treatment (1): piece rate pay
    - Let the mean actions be $a^0(0)$ and $a^0(1)$
2. Run a new copy, but with a "strongly positive" demand
    - "You would be doing us a favor if you work hard"
    - Let mean actions be $a^+(0)$ and $a^+(1)$
3. Run a "strongly negative" demand experiment
    - "You would be doing us a favor if you are lazy"
    - Let mean actions be $a^-(0)$ and $a^-(1)$
4. Compare treatment effects
    - Original treatment effect: $a^0(1) - a^0(0)$
    - Lower bound on treatment effect: $a^-(1) - a^+(0)$

The de Quidt et al (2017) Method

Another usage:

- If $a^{+} \approx a^{0}$ or $a^{-} \approx a^{0}$ then no big deal!
- Usually prior expectation of direction ($+$ or $-$)