# Choosing and planning ethical evaluations

## Session 13

PMAP 8521: Program evaluation
Andrew Young School of Policy Studies

# Plan for today

Types of evaluations

Model- and design-based inference

Ethics and open science

# Types of evaluations

# Types of evaluation

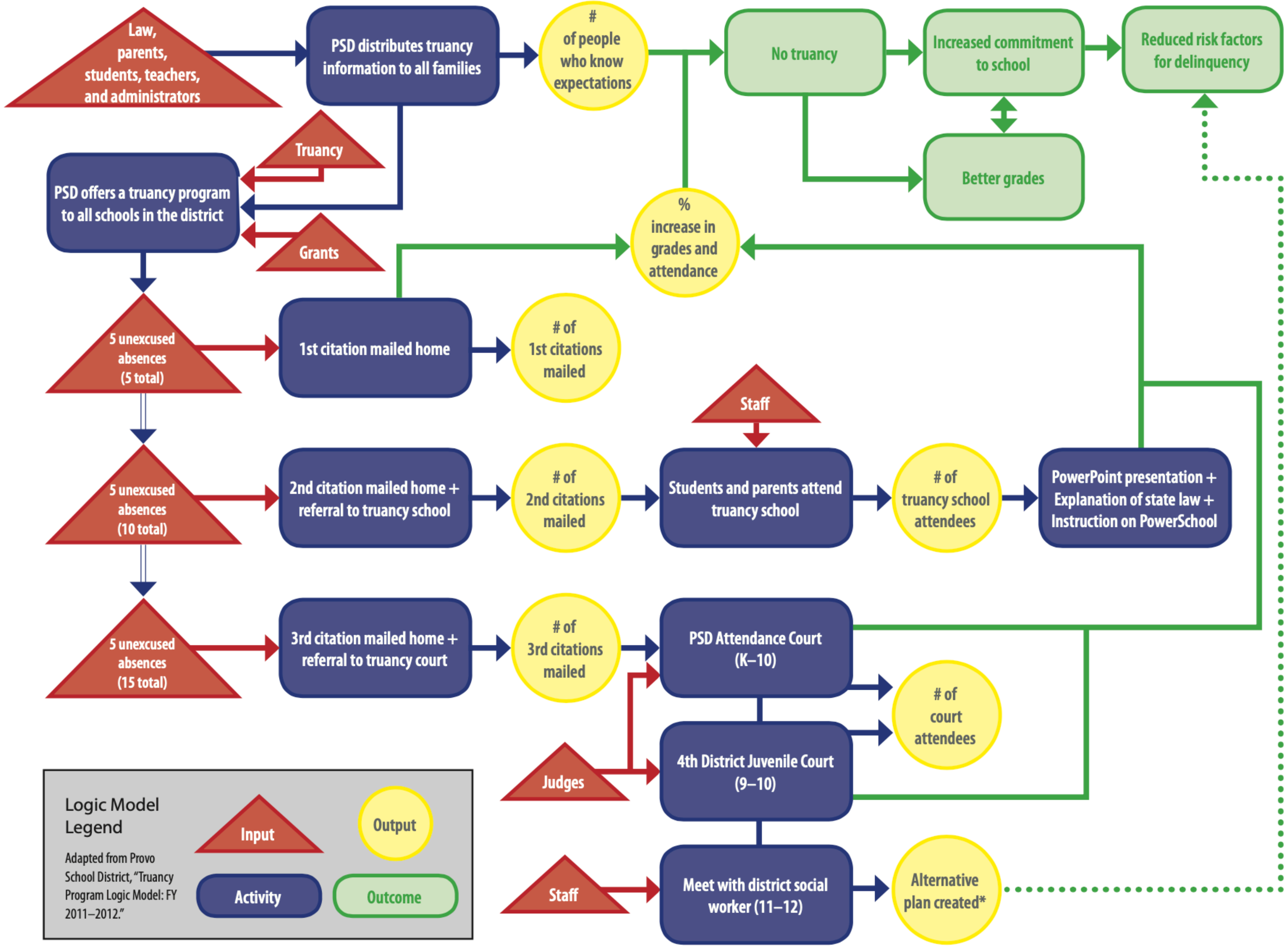
In this class we've focused on one type of evaluation

## Impact evaluation

Checking to see if the program causes outcomes

There are lots of others!

Each type focuses on a specific part of a logic model




* Because 11th and 12th graders who receive 3rd citations are generally unable to graduate from high school, district social workers no longer attempt to increase their commitment to school. As such, any outcomes that occur as a result of the alternative plans made for these students (work study programs, career development assistance, etc.) are only tangentially related to the outcomes of the truancy program itself. The system for creating alternative plans is an entirely separate program with its own logic model, goals, and outcomes.

# Needs assessment

**Formative evaluation / needs assessment**

**Is the program needed?**
**What inputs and activities does it need?**
**What outcomes does it need to cause?**

**Use interviews, surveys, focus groups with target population**

**Do *before* starting the program or when considering changes**

# Process evaluation and monitoring

## Process evaluation / program monitoring

Are inputs going to the right places?
Are the activities working correctly?
Are activities producing right levels of outputs?

**Use monitoring systems, benchmarks, regular reports from within the program itself**

**Do *during* the program**

# Process evaluation and monitoring

# Outcome evaluation

## Outcome evaluation

**Are activities and outputs leading to *initial* outcomes?**
**(basically a short-term impact evaluation)**

**Use surveys, interviews, etc. with target population**

**Do *during* the program**

# Cost-benefit analysis

## Economic evaluation / cost-benefit analysis

**Is the program worth it?**
**Do the benefits of helping the target population outweigh the costs of running the program?**

**Monetize all program costs and benefits, apply a discount factor, convert all costs to net present value, subtract NPV of costs from NPV of benefits**

**Do *during* or at the end of the program**

# Cost-benefit analysis

Table 2
Net Lifetime Benefits of Various Backup Systems
On a Per Vehicle Basis ($2006)

| 3% discount rate | 50 % Driver Factor | 80% Driver Factor |
|---|---|---|
| **Ultrasonic** | | |
| At low speeds, 10 % are backing up crashes | -$82.73 | -$75.34 |
| At low speeds, 25 % are backing up crashes | -$64.26 | -$45.78 |
| **Camera** | | |
| At low speeds, 10 % are backing up crashes | -$375.21 | -$365.20 |
| At low speeds, 25 % are backing up crashes | -$350.19 | -$325.16 |
| **Both** | | |
| At low speeds, 10 % are backing up crashes | -$468.57 | -$457.54 |
| At low speeds, 25 % are backing up crashes | -$441.00 | -$413.43 |

| 7% discount rate | 50 % Driver Factor | 80% Driver Factor |
|---|---|---|
| **Ultrasonic** | | |
| At low speeds, 10 % are backing up crashes | -$74.23 | -$68.35 |
| At low speeds, 25 % are backing up crashes | -$59.53 | -$44.83 |
| **Camera** | | |
| At low speeds, 10 % are backing up crashes | -$365.11 | -$357.14 |
| At low speeds, 25 % are backing up crashes | -$345.19 | -$325.28 |
| **Both** | | |
| At low speeds, 10 % backing up | -$447.80 | -$439.02 |
| At low speeds, 25 % backing up | -$425.86 | -$403.92 |

# Impact evaluation

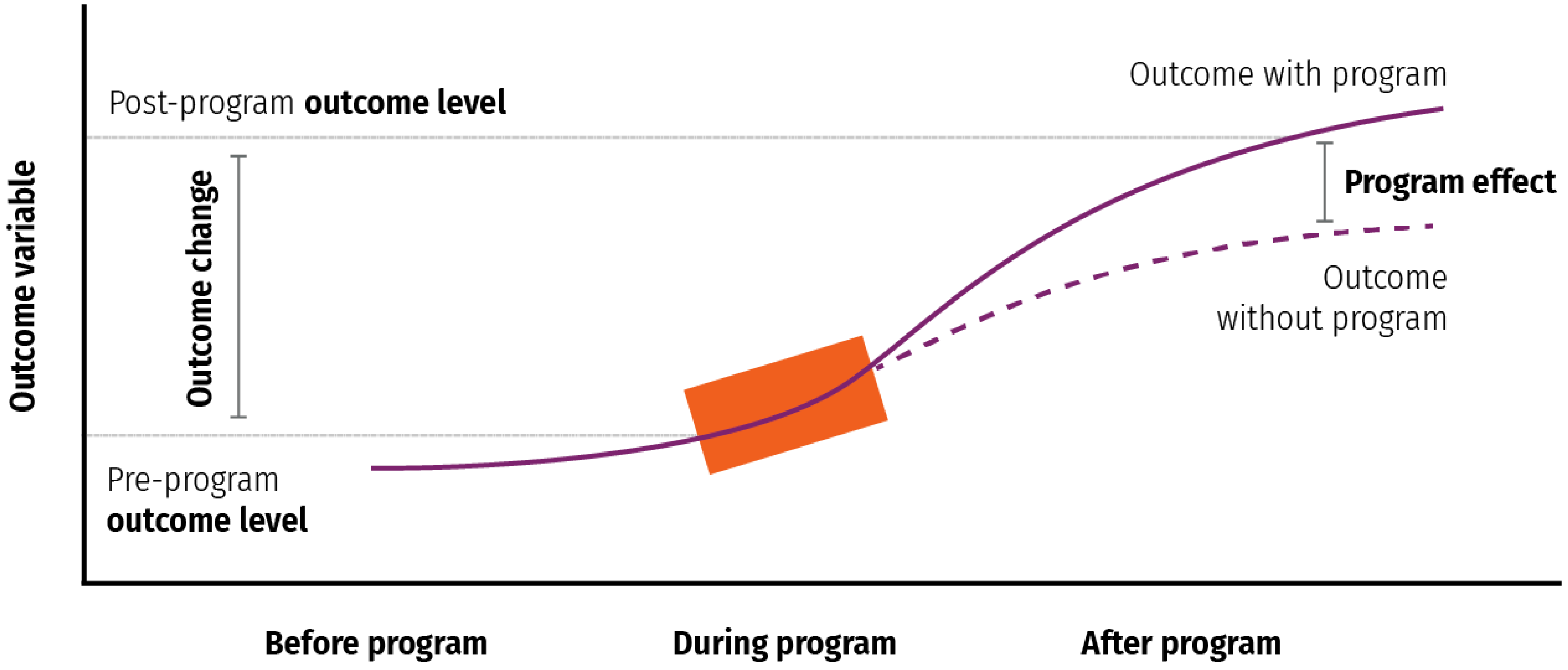
## Impact evaluation

**Does the program cause lasting change?**
**(What we did this semester)**

**Use causal inference tools**

**Do *during* or at the end of the program**

# Impact evaluation



# Types of evaluation

Needs assessment

Process evaluation and monitoring

Outcome evaluation

Cost-benefit analysis

Impact evaluation

**You can take entire classes for just one type!**

# Model- and design-based inference

# Choosing a method

We just learned a *ton* of different methods for causal inference!

DAGs

Matching

Inverse probability weighting

Randomized controlled trials

Difference-in-differences

Regression discontinuity

Instrumental variables

**How do you know which one to use and when?**

# Identification strategies

**The goal of *all* these methods is to isolate (or identify) the arrow between treatment → outcome**

## Model-based identification

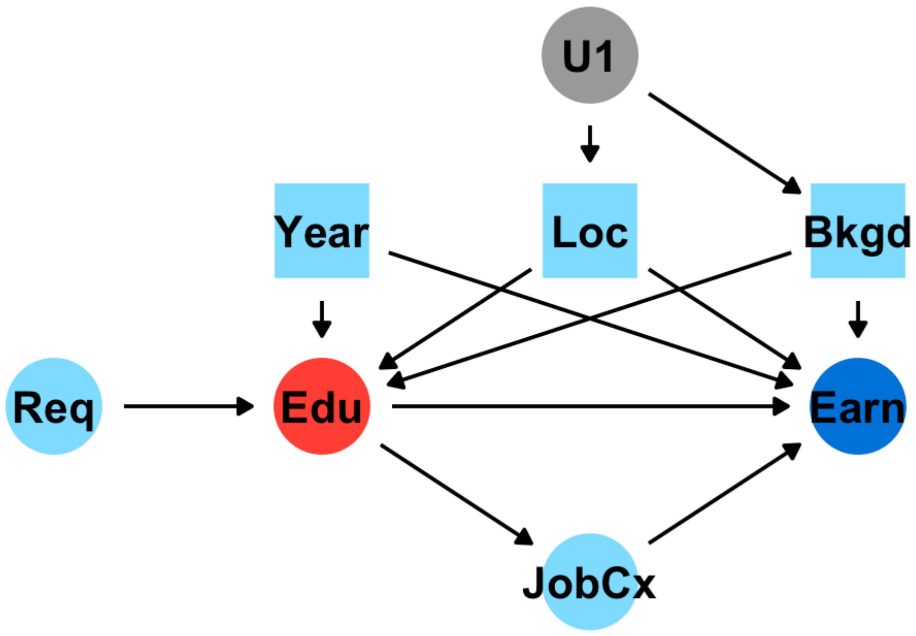
- DAGs
- Matching
- Inverse probability weighting

## Design-based identification

- Randomized controlled trials
- Difference-in-differences
- Regression discontinuity
- Instrumental variables

# Model-based identification

## Use a DAG and *do*-calculus to isolate arrow




**Core assumption: selection on observables**

Everything that needs to be adjusted is measurable; no unobserved confounding

**Big assumption!**

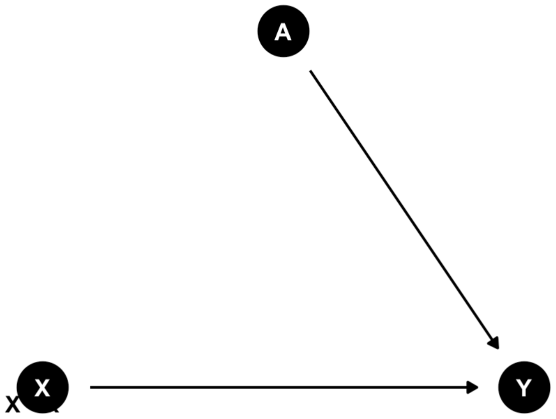
This is why lots of people don't like DAG-based adjustment

# Design-based identification

## Use a special situation to isolate arrow

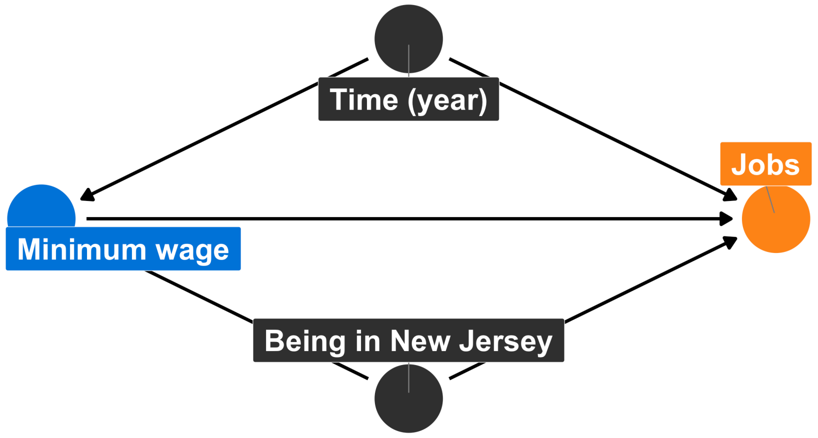
### RCTs

Use randomization
to remove confounding




### Difference-in-differences

Use before/after & treatment/control
differences to remove confounding

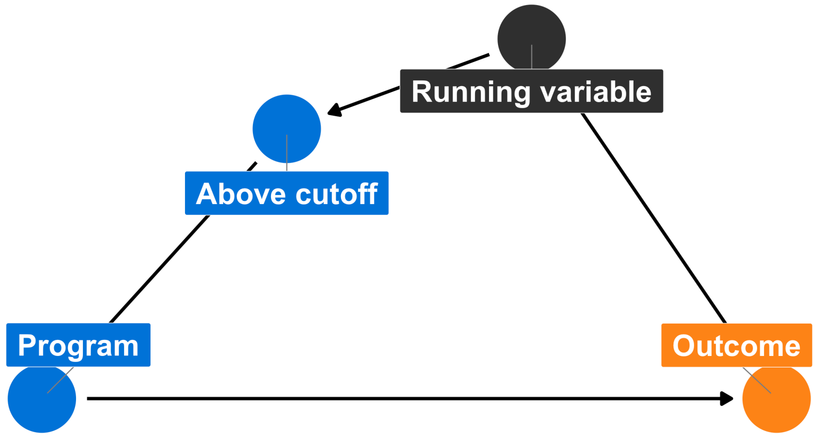


# Design-based identification

**Use a special situation to isolate arrow**

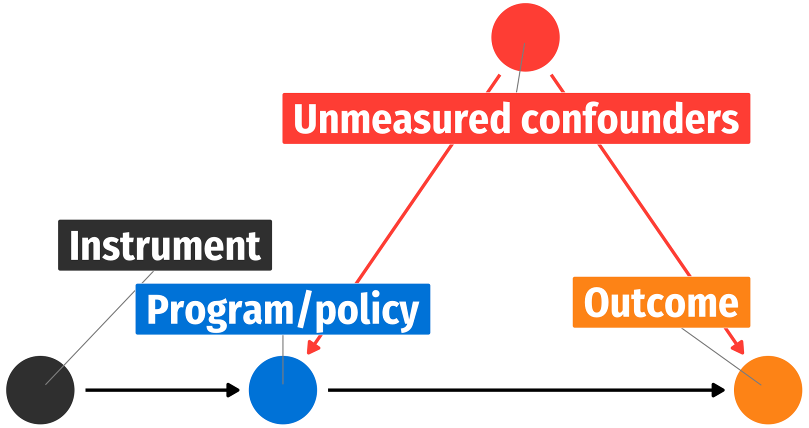
## Regression discontinuity

Use cutoff
to remove confounding




## Instrumental variables

Use instrument
to remove confounding



# Which kind is better?

## Model-based advantages

You don't need to wait for
a special circumstance to emerge!

Use existing datasets

## Model-based disadvantages

The DAG has to be super correct

You can't adjust your way
out of unobserved confounding

## Design-based advantages

Unobserved confounding
is less of a problem!

## Design-based disadvantages

You need a specific situation

You need randomization,
treatment/control+before/after,
some arbitrary cutoff,
or some obscure instrument

# Controlling for stuff

**It's *super* tempting to throw a bunch of control variables in a model**

**This is likely what you did in past stats classes!**

**It's *super* tempting to interpret each of those coefficients**

**Don't!**

Table 7: The effect of anti-NGO legislation on the proportion of US aid channeled through *US-based and international* NGOs in the following year ($H_3$), full models. Each cell contains the parameter's posterior median, the 95% credible interval, and the probability that the parameter is greater than one (in italics)

| | (1) | (2) | (3) |
|---|---|---|---|
| **Fixed part (odds ratios)** | | | |
| Total legal barriers$_{\text{within}}$ | 0.95<br>(0.83, 1.08); *0.20* | | |
| Total legal barriers$_{\text{between}}$ | 1.02<br>(0.89, 1.16); *0.60* | | |
| Barriers to advocacy$_{\text{within}}$ | | 1.04<br>(0.53, 1.99); *0.54* | |
| Barriers to advocacy$_{\text{between}}$ | | 0.96<br>(0.59, 1.54); *0.44* | |
| Barriers to entry$_{\text{within}}$ | | 1.36<br>(0.98, 1.90); *0.97* | |
| Barriers to entry$_{\text{between}}$ | | 1.07<br>(0.84, 1.35); *0.71* | |
| Barriers to funding$_{\text{within}}$ | | 0.71<br>(0.52, 0.97); *0.01* | |
| Barriers to funding$_{\text{between}}$ | | 0.99<br>(0.76, 1.30); *0.48* | |
| Civil society reg. env. (CSRE)$_{\text{within}}$ | | | 1.11<br>(0.95, 1.30); *0.89* |
| Civil society reg. env. (CSRE)$_{\text{between}}$ | | | 1.03<br>(0.89, 1.19); *0.66* |
| Polity IV (0–10)$_{\text{within}}$ | 1.04<br>(0.93, 1.18); *0.75* | 1.04<br>(0.93, 1.18); *0.74* | 1.00<br>(0.87, 1.14); *0.52* |
| Polity IV (0–10)$_{\text{between}}$ | 0.98<br>(0.91, 1.06); *0.32* | 0.98<br>(0.90, 1.06); *0.30* | 0.95<br>(0.84, 1.08); *0.23* |
| GDP per capita (log)$_{\text{within}}$ | 0.29<br>(0.17, 0.48); *0.00* | 0.28<br>(0.16, 0.47); *0.00* | 0.28<br>(0.16, 0.46); *0.00* |
| GDP per capita (log)$_{\text{between}}$ | 0.72<br>(0.62, 0.85); *0.00* | 0.72<br>(0.62, 0.85); *0.00* | 0.73<br>(0.62, 0.85); *0.00* |
| Trade as % of GDP$_{\text{within}}$ | 1.00<br>(0.99, 1.00); *0.15* | 1.00<br>(0.99, 1.00); *0.14* | 1.00<br>(0.99, 1.00); *0.17* |
| Trade as % of GDP$_{\text{between}}$ | 1.00<br>(0.99, 1.00); *0.36* | 1.00<br>(0.99, 1.00); *0.39* | 1.00<br>(0.99, 1.00); *0.36* |
| Corruption$_{\text{within}}$ | 1.13<br>(0.96, 1.31); *0.93* | 1.12<br>(0.94, 1.31); *0.91* | 1.16<br>(0.97, 1.36); *0.95* |
| Corruption$_{\text{between}}$ | 1.30<br>(1.19, 1.42); *1.00* | 1.29<br>(1.18, 1.42); *1.00* | 1.30<br>(1.18, 1.42); *1.00* |
| Proportion of aid to foreign NGOs in present year (logit) | 1.39<br>(1.33, 1.45); *1.00* | 1.38<br>(1.32, 1.45); *1.00* | 1.39<br>(1.33, 1.45); *1.00* |

# Controlling for stuff

**When focusing on isolating the treatment → outcome arrow, arrows between/from other nodes are less meaningful**

**You also don't pick up their full effects!**

**"[E]ven valid controls are often correlated with other unobserved factors, which renders their marginal effects uninterpretable from a causal inference perspective"**

**(Hünermund and Louw 2020, p. 2)**

# Controlling for stuff

| Method | Controls | Minimum model |
|---|---|---|
| Matching/IPW | Use for matching, propensity scores | `outcome ~ treatment, matched_data`<br>`outcome ~ treatment, weights` |
| RCTs | Not really necessary | `outcome ~ treatment` |
| Diff-in-diff | Not really necessary, use if DAG says to | `outcome ~ treatment + after + treatment*after` |
| RDD | Not really necessary | `outcome ~ running_var + cutoff` |
| IV | Not really necessary, use if DAG says to | `treatment_hat ~ instrument`<br>`outcome ~ treatment_hat` |

# Guidelines

## Your choice of method depends on the situation + the available data

**Table 11.1 Relationship between a Program's Operational Rules and Impact Evaluation Methods**

| | | **Excess demand for program (limited resources)** | | **No excess demand for program (fully resourced)** | |
|---|---|---|---|---|---|
| | **Eligibility criteria** | (1)<br>Continuous eligibility ranking and cutoff | (2)<br>No continuous eligibility ranking and cutoff | (3)<br>Continuous eligibility ranking and cutoff | (4)<br>No continuous eligibility ranking and cutoff |
| **Timing of Implementation** | (A)<br>Phased implementation over time | **Cell A1**<br>Randomized assignment (chapter 4)<br>RDD (chapter 6) | **Cell A2**<br>Randomized assignment (chapter 4)<br>Instrumental variables (randomized promotion) (chapter 5)<br>DD (chapter 7)<br>DD with matching (chapter 8) | **Cell A3**<br>Randomized assignment to phases (chapter 4)<br>RDD (chapter 6) | **Cell A4**<br>Randomized assignment to phases (chapter 4)<br>Instrumental variables (randomized promotion to early take-up) (chapter 5)<br>DD (chapter 7)<br>DD with matching (chapter 8) |
| | (B)<br>Immediate implementation | **Cell B1**<br>Randomized assignment (chapter 4)<br>RDD (chapter 6) | **Cell B2**<br>Randomized assignment (chapter 4)<br>Instrumental variables (randomized promotion) (chapter 5)<br>DD (chapter 7)<br>DD with matching (chapter 8) | **Cell B3**<br>RDD (chapter 6) | **Cell B4**<br>If less than full take-up:<br>Instrumental variables (randomized promotion) (chapter 5)<br>DD (chapter 7)<br>DD with matching (chapter 8) |

*Note:* DD = difference-in-differences; RDD = regression discontinuity design.

Table 11.1 from *Impact Evaluation in Practice*, p. 191

# Ethics and open science

# Ethics of evaluating programs

Social programs are designed to help people

In order to evaluate them, you need some people to **not use the program**

Control groups are essential for causal inference!

"Groups should not be excluded from an intervention that is known to be beneficial solely for the purpose of an evaluation"
(*Impact Evaluation in Practice*, p. 233)

# Ethical control groups

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*Note:* DD = difference-in-differences; RDD = regression discontinuity design.

Table 11.1 from *Impact Evaluation in Practice*, p. 191

# Ethical evaluation practices

## Follow IRB guidelines

Respect for persons | Beneficence | Justice

## Make sure participants give informed consent

## Maintain privacy

Any published data needs to be be de-identified

# Ethical open science practices

## Preregistration

Prevents file drawer problem + p-hacking

## Preanalysis plan

Prevents p-hacking, data mining, multiple hypothesis testing

## Replication

Ensures that others can find same results with your data

## Documentation

Ensures that others know what you're measuring

**Table 13.1 Ensuring Reliable and Credible Information for Policy through Open Science**

| Research issue | Policy implications | Prevention and mitigation solutions through open science |
|---|---|---|
| *Publication bias.* Only positive results are published. Evaluations showing limited or no impacts are not widely disseminated. | Policy decisions are based on a distorted body of knowledge. Policy makers have little information on what *doesn't* work and continue to try out/adopt policies that have no impact. | Trial registries |
| *Data mining.* Data are sliced and diced until a positive regression result appears, or the hypothesis is retrofitted to the results. | Policy decisions to adopt interventions may be based on unwarranted positive estimates of impacts. | Preanalysis plans |
| *Multiple hypothesis testing, subgroup analysis.* Researchers slice and dice the data until they find a positive result for some group. In particular, (1) multiple testing leads to a conclusion that some impacts exist when they do not, or (2) only the impacts that are significant are reported. | Policy decisions to adopt interventions may be based on unwarranted positive estimates of impacts. | Preanalysis plans and specialized statistical adjustment techniques such as index tests, family-wise error rate, and false discovery rate control[a] |
| *Lack of replication.* Results cannot be replicated because the research protocol, data, and analysis methods are not sufficiently documented. | Policy may be based on manipulated (positive or negative) results, as results may be due to mistakes in calculations. | Data documentation and registration, including project protocols, organizing codes, publication of codes, and publication of data |
| Mistakes and manipulations may go undetected. | Results between different studies cannot be compared. | |
| Researchers are not interested in replicating studies, and journals are not interested in "me-too" results. | Validity of results in another context cannot be tested. | Changes in journal policies and funding policies to require data documentation and encourage replication |
| Interventions cannot be replicated because the intervention protocol is not sufficiently documented. | Policy makers may be unable to replicate the intervention in a different context. | |

a. For a basic introduction to the multiple comparisons problem and potential statistical corrections, please see https://en.wikipedia.org/wiki/Multiple_comparisons_problem.

Table 13.1 from *Impact Evaluation in Practice*, p. 238

# Synthetic data

It feels weird to say that making fake data helps with good open science practices!

**But it does!**

Make your pre-analysis plan based on simulated data

Do whatever statistical shenanigans you want with the fake data