

SOC542 Statistical Methods in Sociology II

Causal inference

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Course updates

- ▶ Presentations on 5/5
 - ▶ 10 minutes to present project
 - ▶ Introduction
 - ▶ Data
 - ▶ Methodology
 - ▶ Main results
 - ▶ Robustness checks
 - ▶ Conclusions
 - ▶ 5 minutes for Q&A

Course updates

- ▶ Presentations on 5/5
 - ▶ Make your slides in Google Slides
 - ▶ Copy and paste into main deck when finished (link will be shared via email)

Plan

- ▶ Introduction to causal inference
- ▶ Methods
 - ▶ Matching and weighting using propensity scores*
 - ▶ Instrumental variables*
 - ▶ Regression discontinuity
 - ▶ Difference-in-differences

Causal inference and regression

- ▶ Causal inference entails making causal claims about relationships between variables
 - ▶ X causes Y
- ▶ So far, we have generally refrained from making causal claims
 - ▶ We have focused on estimating relationships between variables
 - ▶ Correlations, associations

Causal inference and regression

Causal inference in the social sciences

- ▶ Causal inference is a central goal of much social science research, but varying emphasis across disciplines
 - ▶ Primary goal in much economic research
 - ▶ Preferred in quantitative political science
 - ▶ Growing interest in sociology, harder to publish descriptive work
 - ▶ Psychology primarily an experimental science already

Causal inference and regression

Potential outcomes

- ▶ D_i is a binary variable denoting whether a unit is treated.
- ▶ For each unit, we have two **potential outcomes**:
 - ▶ Outcome if $D_i = 1$ (treated): Y_i^1
 - ▶ Outcome if $D_i = 0$ (untreated): Y_i^0
- ▶ The outcome for a given unit can be expressed as:
 - ▶ $Y_i = Y_i^1 D_i + Y_i^0 (1 - D_i)$

Causal inference and regression

The fundamental problem of causal inference

- ▶ Only one treatment status and outcome is observed for each unit.
 - ▶ The difference in potential outcomes for a unit, $Y_i^1 - Y_i^0$, is unobservable
 - ▶ The hypothetical outcome if treatment status differed is known as a **counterfactual**.

Causal inference and regression

When regression is causal

- ▶ In an experiment, we can randomly assign subjects to treatment and control conditions and compare the outcomes across subjects.
- ▶ Assuming a binary treatment, D we could estimate the following regression:

$$y_i = \hat{\beta}_0 + \hat{\beta}_1 D_i + u_i$$

- ▶ Here $\hat{\beta}_1$ will provide an estimate of the average treatment effect (ATE).

Causal inference and regression

Observational data

- ▶ Many social science questions cannot be tested via experiments.
- ▶ But we can conduct pseudo-experiments where observed variables are considered as treatments.
 - ▶ e.g. What is effect of college degree on earnings?
- ▶ Assignment to treatment is not controlled by researcher or randomized.
 - ▶ **Selection bias:** Subjects may select into treatment.

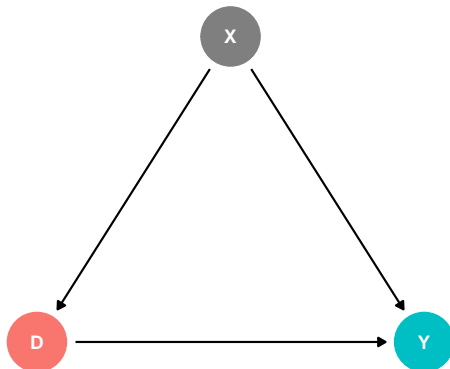
Causal inference and regression

Selection on observables

- ▶ We can account for selection process and make a causal estimate using observational data if the selection process is observable.
- ▶ **Ignorability**
 - ▶ The treatment assignment is independent of the potential outcomes, conditional on observed covariates.
 - ▶ All possible confounders are observed.
- ▶ After conditioning on X , the treatment assignment is as good as random.

Causal inference and regression

Selection on observables



Causal inference and regression

Selection on observables

- ▶ Confounding (omitted variables bias) is nearly always a risk
 - ▶ Few cases where we can expect to measure *all* unobserved confounders
- ▶ Causal inference techniques can be used to make causal claims by enabling us to approximate randomized experiments
 - ▶ But each approach entails many assumptions and there is no silver bullet when working with observational data

Causal inference and regression

Causal inferences from observational data

- ▶ There are several approaches to making causal inference using observational data that extend regression methods
- ▶ This class will focus on four common ones:
 - ▶ Propensity score matching and weighting
 - ▶ Instrumental variables
 - ▶ Regression discontinuity
 - ▶ Difference-in-difference

Propensity score matching and weighting

- ▶ Intuition:
 - ▶ Find treated and untreated units with similar covariates then compare outcomes.
- ▶ Addresses selection by comparing similar observations
 - ▶ Assumes selection on observables

Propensity score matching and weighting

Matching approaches

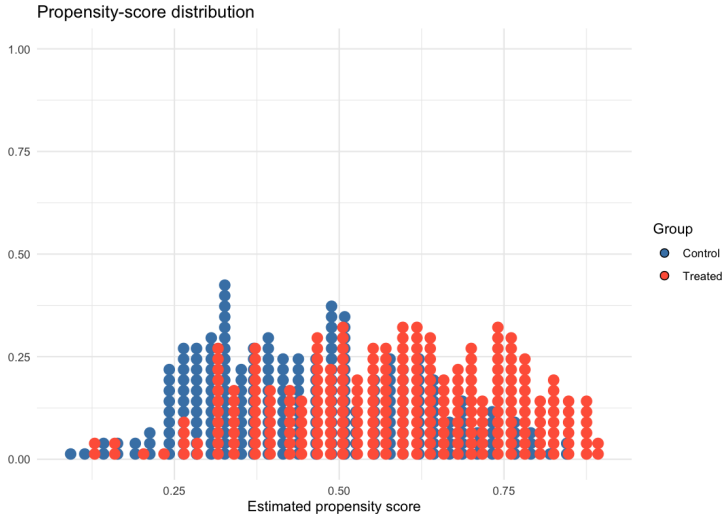
- ▶ Exact matching
 - ▶ Ideal case, but limited value in practice as requires extreme subsampling
- ▶ Partial or fuzzy matching
 - ▶ Match on a subset of covariates
 - ▶ Use a distance metric to find similar units
- ▶ Propensity score matching
 - ▶ Estimate a model to predict treatment, $\hat{p}_i = P(D_i = 1|X_i)$
 - ▶ Use \hat{p}_i to match units with similar scores
- ▶ Causal estimate obtained by comparing outcomes of matched units

Propensity score matching and weighting

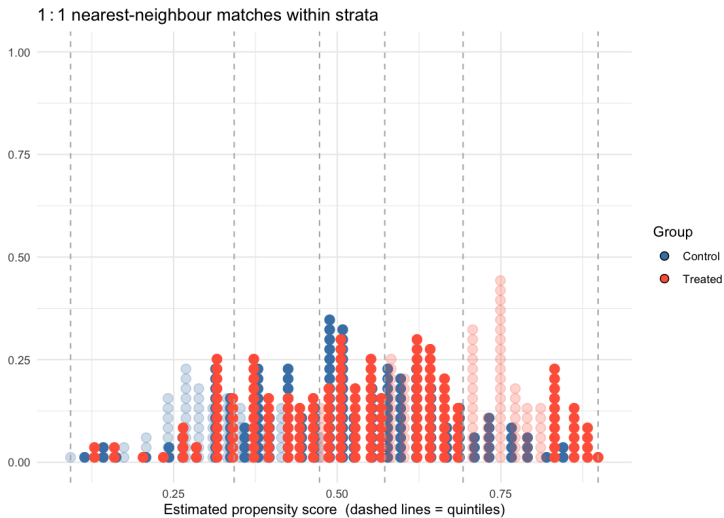
Estimating propensity scores

- ▶ Estimate the probability of treatment conditional on covariates, typically using logistic regression
 - ▶ Goal is to explain as much variance as possible to try to achieve balance
 - ▶ A large number of predictors and transformations recommended (Morgan and Todd 2008)
- ▶ Key assumptions:
 - ▶ Ignorability: Treatment assignment is independent of potential outcomes, conditional on X
 - ▶ Overlap: Each unit has a nonzero probability of receiving treatment and control

Simulating propensity scores



Matches within propensity score strata



Propensity score matching and weighting

Propensity score weighting

- ▶ Reweight observations to create a synthetic sample
 - ▶ Adjust regression estimates to account for propensity to select into treatment
- ▶ Check *balance* after weighting to ensure covariates are similar across treatment groups

Propensity score matching and weighting

Propensity score weighting

- ▶ Reweighting to measure the Average Treatment Effect on the Treated (ATT)
 - ▶ Treatment group is unchanged. The goal is to “turn the control group into a representative sample of the population-level treatment group” (Morgan and Todd 2008: 244).

$$d_i = 1 : w_{i,ATT} = 1$$

$$d_i = 0 : w_{i,ATT} = \frac{\hat{p}_i}{1 - \hat{p}_i}$$

Propensity score matching and weighting

Morgan and Todd (2008) procedure

Stage 1. Estimate baseline regression models:

Estimate a bivariate regression model



Estimate a multiple regression model by introducing adjustment variables

Stage 2. Model treatment selection/assignment and construct weights:

Estimate a model predicting membership in the treatment group from the adjustment variables used in the multiple regression model



Form weights as a function of the predicted probabilities of membership in the treatment group



Check the balance of the adjustment variables produced by the weights



If the adjustment variables remain unbalanced, respecify the model predicting treatment group membership



Stage 3. Estimate weighted regression models and develop a diagnosis:

Reestimate the initial regression models using the final weights



Compare alternative weighted regression estimates and accept a preliminary diagnosis if the estimates differ



Assess the stability of the preliminary diagnosis to alternative decisions about overlap and supplemental parametric adjustment

Propensity score matching and weighting

Example: Organizational participation and network expansion (Davidson and Sanyal 2017)

- ▶ Do women who participate in self-help groups (SHGs) have larger networks?
- ▶ Causal mechanism: SHGs provide social capital and resources
- ▶ Selection problem
 - ▶ Women who join SHGs may be systematically different from those who do not
- ▶ Solution
 - ▶ Propensity scores to estimate SHG participation
 - ▶ Use ATT weighting and matching estimators

Unadjusted regression results

| Variable | Model 1 | Model 2 | Model 3 |
|-------------------|---------------------|------------------------------|---------------------------------|
| | Bivariate | Bivariate with Fixed Effects | Multivariate with Fixed Effects |
| SHG participation | 1.601*** (0.040) | 1.785*** (-0.042) | 1.347*** (0.032) |

Balance table

| | Difference in mean | | % Bias reduction |
|-------------------------------|--------------------|----------|------------------|
| | Unmatched | Matched | |
| Nonkin indegree | 1.002*** | 0.571*** | 43.0 |
| Nonkin outdegree | 0.285*** | 0.233*** | 21.8 |
| Kin indegree | 0.361*** | 0.013 | 96.5 |
| Kin outdegree | 0.033 | 0.018 | 44.7 |
| Age | 2.27*** | -0.381 | 83.2 |
| Years of education | -1.047*** | 0.041 | 96.0 |
| Scheduled Caste | 0.036*** | -0.002 | 94.2 |
| Scheduled Tribe | 0.541 | 0.001 | 60.8 |
| Other Backwards Caste | -0.024* | 0.015 | 39.2 |
| General Class | -0.014* | -0.013 | 2.8 |
| Native to village | -0.083*** | -0.012 | 85.6 |
| Moved due to marriage | 0.092*** | 0.011 | 88.4 |
| Years spent in village | 1.473*** | 0.174 | 93.0 |
| Multilingual | -0.01 | -0.004 | 59.2 |
| Muslim | -0.007 | -0.003 | 59.9 |
| Worked last week | 0.128*** | 0.050*** | 60.3 |
| Has latrine | -0.06*** | -0.008 | 87.0 |
| Has electricity | -0.006 | 0.006 | -6.1 |
| Number of adults in household | -0.484*** | 0.005 | 99.0 |
| Number of rooms per adult | 0.048*** | -0.005 | 90.1 |
| Number of beds per adult | -0.011 | -0.009 | 14.5 |
| N | 9263 | 9263 | |

Note: * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests).

Weighted and matched results

| Variable | Ties from all nonkin | | Ties from nonkin SHG members | Ties from nonkin non-members |
|-------------------|-----------------------------|---------------------|------------------------------|------------------------------|
| | Model 4: | Model 5: | Model 6: | Model 7: |
| | Full sample ATT weighted | Matched only | Matched only | Matched only |
| SHG participation | 1.351*** (0.034) | 1.287*** (0.074) | 2.038*** (0.174) | 1.054 (0.070) |

Propensity score matching and weighting

Critiques of propensity score matching

- ▶ King and Nielsen's (2019) critique:
 - ▶ PSM approximates a completely randomized experiment, not an efficient blocked randomized experiment
 - ▶ Matching on propensity scores can increase imbalance relative to matching directly on covariates
 - ▶ We lose information by reducing the comparison to a single score
- ▶ PSM can introduce *bias*, *inefficiency*, and *model dependence*
- ▶ Solution: Directly match on covariates (e.g., Mahalanobis distance matching, coarsened exact matching)

Propensity score matching and weighting

Practical advice

- ▶ Estimating propensity scores is a useful approach for weighting
- ▶ For matching, it is better to match directly on covariates
- ▶ Always:
 - ▶ Check covariate balance after matching or weighting
 - ▶ Note that neither approach can correct for unobserved confounders

Instrumental variables

- ▶ Find an exogenous regressor to explain random variation in a treatment
 - ▶ Relaxes selection on observables assumption
- ▶ Random variation in the treatment can be isolated and treated as a causal effect
- ▶ A good IV has a “certain ridiculousness” (Cunningham) with respect to the outcome

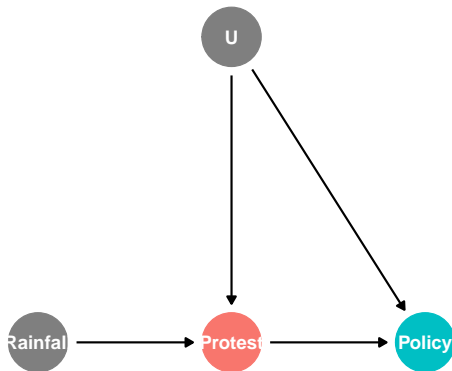
Instrumental variables

Rainfall and protest

- ▶ We want to infer effect of protest (treatment) on policy change (outcome)
- ▶ But protest is not randomly assigned; unmeasured confounders
- ▶ Rainfall is an **instrument** insofar as it effects protest and indirectly effects policy change *only through* its effect on protest

Instrumental variables

Rainfall and protest



Instrumental variables in sociology

Kirk (2009)



Laidley and Conley (2018)



Sampson and Winter (2018)



Harding et al. (2018)



Instrumental variables

Estimation

- ▶ IV is estimated using two-stage least squares (2SLS)
- ▶ Where Y is the outcome, D is the treatment, and Z is the instrument:
 - ▶ First stage: $\hat{D}_i = \hat{\gamma}_0 + \hat{\gamma}_1 Z_i + \epsilon_i$
 - ▶ Second stage: $\hat{Y}_i = \hat{\beta}_0 + \hat{\beta}_1(\hat{D}_i) + u_i$
- ▶ Additional controls can be included in both stages.

Instrumental variables

Estimation

- ▶ Estimates
 - ▶ First stage isolates unconfounded variation in treatment
 - ▶ Second stage estimates for the causal effect of instrumented treatment on outcome

Instrumental variables

Assumptions

- ▶ IV relies on *strong assumptions* for valid causal inference:
 - ▶ *Relevance*: Instrument must causally affect the treatment
 - ▶ *Unconfoundedness*: Instrument is independent of unobserved confounders
 - ▶ *Exclusion restriction*: Instrument affects the outcome only through the treatment
 - ▶ *Monotonicity*: Instrument does not push some units toward treatment and others away
 - ▶ *SUTVA and positivity*: No interference, and all units must have a chance to receive either value of the instrument

Instrumental variables

Relevance

- ▶ The most basic requirement is that the instrument must causally affect treatment uptake
- ▶ Measured by the first-stage regression:
 - ▶ Strong instrument: Large first-stage coefficient $\hat{\gamma}_1 Z_i$, high F-statistic
 - ▶ Multiple instruments can be used, so t-statistics are not sufficient to assess strength
 - ▶ Weak instrument: Small first-stage coefficient, low F-statistic
- ▶ Important to report first-stage F-statistics
 - ▶ Heuristic: $F > 10$ implies a strong instrument, but maybe bigger is needed (20+)

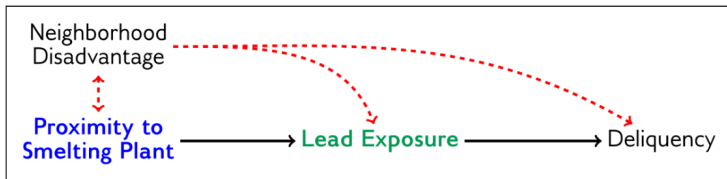
Instrumental variables

Unconfounded instrument

- ▶ Instrument must be *as good as randomly assigned*
- ▶ No unmeasured common causes with treatment or outcome

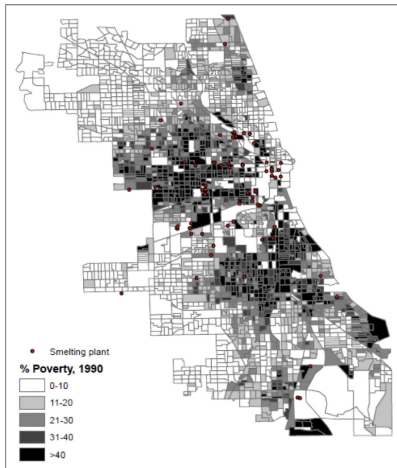
Instrumental variables

Confounded instrument?



Instrumental variables

Confounded instrument?



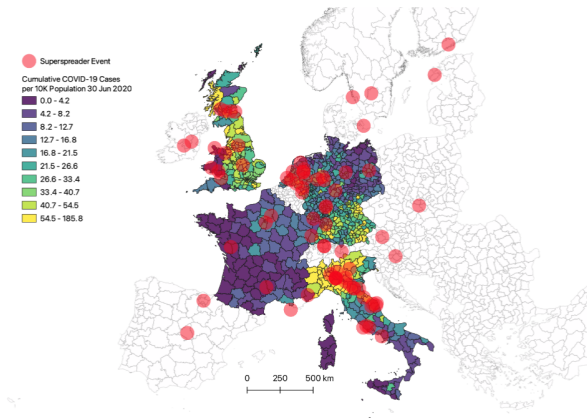
Instrumental variables

Example: COVID-19 and right-wing populism (Lall, Davidson, and Hagemeister)

- ▶ Did COVID-19 increase support for right-wing populism?
- ▶ Confounding: COVID-19 infections not randomly assigned
- ▶ Solution: Super-spreader events as instrument

Locations of super-spreader events

FIGURE 1. COVID-19 INCIDENCE AND SUPERSPREADER EVENTS IN EUROPE, EARLY 2020



Notes: Distribution of SSEs across Europe and of COVID-19 incidence across 792 NUTS-3 regions of five countries (FR, DE, IT, NL, GB) between 1 February and 30 June 2020.

Checking balance

TABLE 1. BALANCE TABLE: REGIONS WITH AND WITHOUT EARLY SUPERSPREADER EVENTS

| | NUTS Level | Regions without SSEs | | Regions with SSEs | | T-Test: Diff. in Means | | |
|--|------------|----------------------|----------|-------------------|----------|------------------------|----------|---------------------------------------|
| | | Mean | St. Dev. | Mean | St. Dev. | <i>t</i> | <i>p</i> | <i>p</i> ₁₀₀₀ [*] |
| Populist Vote Share (%), 2019 | 3 | 8.18 | 5.67 | 8.03 | 2.72 | -0.39 | 0.7 | 0.67 |
| Mean Populist Vote Share (%), 2014-19 | 3 | 8.77 | 5.04 | 8.34 | 2.96 | -1.1 | 0.28 | 0.27 |
| Annual Δ Populist Vote Share (%), 2014-19 | 3 | 19.31 | 218 | 24.06 | 48.5 | 0.54 | 0.59 | 0.66 |
| Population per km ² , 2019 | 3 | 382.34 | 1,019.38 | 594.36 | 1,072.69 | 1.41 | 0.16 | 0.12 |
| Share of Urban Households (%), 2019 | 2 | 39.25 | 23.4 | 43.99 | 21.3 | 1.38 | 0.17 | 0.14 |
| Women per 100 Men, 2019 | 3 | 103.03 | 4.04 | 103.28 | 2.99 | 0.68 | 0.5 | 0.5 |
| Share of People under 20 y/o (%), 2019 | 3 | 20.72 | 4.23 | 20.58 | 2.71 | -0.42 | 0.67 | 0.70 |
| Share of People 65+ y/o (%), 2019 | 3 | 20.36 | 4.57 | 20.85 | 3.29 | 1.26 | 0.21 | 0.19 |
| Median Male Age, 2019 | 3 | 42.4 | 4.78 | 42.87 | 3.19 | 1.25 | 0.22 | 0.21 |
| Share of Low-Educated Males (%), 2019 | 2 | 24.55 | 16.55 | 23.07 | 10.08 | -0.98 | 0.33 | 0.36 |
| Share of Foreign Citizens (%), 2019 | 2 | 8.06 | 7.58 | 9.25 | 5.19 | 1.35 | 0.18 | 0.17 |
| Net Migration per Capita, 2014-19 | 3 | 0.003 | 0.10 | 0.003 | 0.06 | 1.54 | 0.13 | 0.11 |
| Annual Population Growth (%), 2014-19 | 2 | 7.48 | 8.77 | 3.86 | 4.52 | 1.15 | 0.25 | 0.28 |
| GDP per Capita Growth (%), 2014-2019 | 3 | 17.12 | 15.5 | 15.88 | 17.5 | -0.56 | 0.58 | 0.73 |
| Industrial Employment (%), 2019 | 3 | 18.50 | 9.26 | 17.69 | 10.2 | -0.56 | 0.58 | 0.6 |
| Manufacturing Employment (%), 2019 | 3 | 16.54 | 8.68 | 16.51 | 10.2 | -0.02 | 0.98 | 0.99 |
| Mean China Shock Instrument, 1988-2007 | 2 | 1.45 | 1.89 | 1.89 | 1.63 | 1.32 | 0.2 | 0.15 |

Notes: Comparison of European regions with and without early SSEs (1 February — 30 June 2020) on measures and predictors of populist support before the COVID-19 pandemic. The last three columns report the results of a two-sample *t*-test — regular and bootstrapped with 1,000 samples — for the difference in means between the two sets of regions.

2SLS estimates: Social media engagement

TABLE 2. RELATIONSHIP BETWEEN COVID-19 EXPOSURE AND POPULIST TWITTER ENGAGEMENT

| Outcome: | Log Populist Mentions (1) | Log Populist Retweets (2) | Log Populist Retweets (3) | Log Populist Retweets (4) | Log Populist Likes (5) | Log Populist Likes (6) |
|--|------------------------------|------------------------------|------------------------------|------------------------------|---------------------------|---------------------------|
| <i>Panel A: OLS Estimates</i> | | | | | | |
| Log Daily COVID Cases per 10,000 Population | 0.026*** (0.009) | 0.022** (0.009) | 0.105*** (0.024) | 0.056*** (0.020) | 0.059*** (0.020) | 0.014 (0.018) |
| <i>Panel B: 2SLS Estimates (Instrument = SSE Exposure)</i> | | | | | | |
| Log Daily COVID Cases per 10,000 Pop. (Instrumented) | 0.197*** (0.070) | 0.175** (0.072) | 1.010*** (0.229) | 0.682*** (0.206) | 0.893*** (0.217) | 0.592*** (0.203) |
| First-Stage F-Statistic | 89.680 | 81.217 | 89.680 | 81.217 | 89.680 | 81.217 |
| Day FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| NUTS-3 FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Lockdown Stringency | | ✓ | | ✓ | | ✓ |
| Mean Outcome Variable | 0.09 | 0.09 | 0.84 | 0.84 | 0.98 | 0.98 |
| N | 119,520 | 119,520 | 119,520 | 119,520 | 119,520 | 119,520 |

Notes: NUTS-3-day-level OLS estimates (panel A) and 2SLS estimates (panel B) with robust standard errors, clustered by NUTS-3 region, in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2SLS estimates: Electoral support

TABLE 4. LOCAL COVID-19 EXPOSURE AND POPULIST SUPPORT IN FRENCH MUNICIPAL ELECTIONS

| Outcome: RN Vote Share (mean= 0.01) | (1) | (2) | (3) | (4) | (5) |
|--|-------------------------|-----------------------|-----------------------|-------------------------|------------------------|
| <i>Panel A: 2SLS Estimates (Instrument = SSE Exposure)</i> | | | | | |
| Log Cumulative COVID Cases per 10,000 Pop. (Instrumented) | 11.925** (4.800) | 13.169** (5.303) | 13.178** (5.424) | 12.768** (5.189) | 15.172** (6.796) |
| First-Stage F-Statistic | 361.127 | 345.609 | 320.145 | 309.147 | 169.741 |
| <i>Panel B: OLS Estimates (Treatment = Instrument)</i> | | | | | |
| SSE Exposure Instrument | 0.00002*** (0.00001) | 0.0001** (0.00002) | 0.0001** (0.00003) | 0.00003*** (0.00001) | 0.00002** (0.00001) |
| Instrument Maximum Radius (ϕ) | 400 | 200 | 200 | 400 | 500 |
| Instrument Maximum Lag (τ) | 30-40 | 30-40 | 20 | 20 | 30-40 |
| Municipality FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Election FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Municipality-Level Controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 19,217 | 19,217 | 19,217 | 19,217 | 19,217 |

Notes: Municipality-election-level 2SLS (panel A) and OLS (panel B) estimates with robust standard errors, clustered by municipality, in parentheses. Controls: male-female ratio, population density, share of young people, share of elderly people, share of people with no schooling, share of industrial workers, share of unemployed people, manual share of employment, share of recent overseas migrants, total RN voters, size of housing stock. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

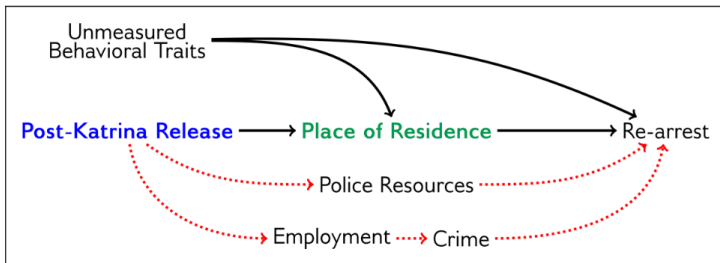
Instrumental variables

Exclusion restriction

- ▶ Instrument affects the outcome *only through treatment*
- ▶ Violations can arise:
 - ▶ If instrument affects outcome through other causal pathways
 - ▶ If treatments are coarsely measured
- ▶ Violations are hard to rule out and can severely bias results

Instrumental variables

Exclusion restriction

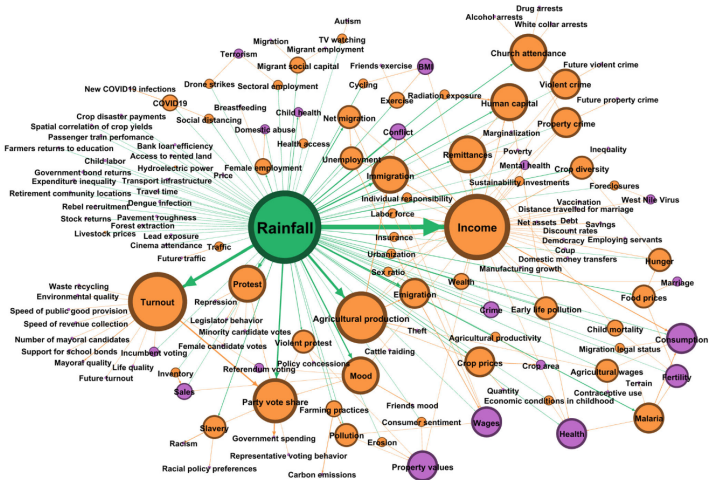


Instrumental variables

Example: Weather instruments

- ▶ Mellon (2024) surveys weather-IV studies
 - ▶ Extensive evidence of many relationships between weather and social outcomes
 - ▶ Hard to maintain exclusion assumption

Potential exclusion violations for rainfall



Mellon 2024.

Instrumental variables

Exclusion restriction violations

- ▶ Similar problems apply to many common instruments:
 - ▶ Historical shocks (e.g. recession, war, colonialism)
 - ▶ Geographic variation (e.g. altitude, distance)
 - ▶ Policy changes

Instrumental variables

Sensitivity to violations

- ▶ Many IV estimates vulnerable to small biases
 - ▶ Tiny relationships between exclusion variables and outcomes can nullify findings
 - ▶ Sensitivity analysis essential for credible IV claims

Instrumental variables

IV is powerful but fragile

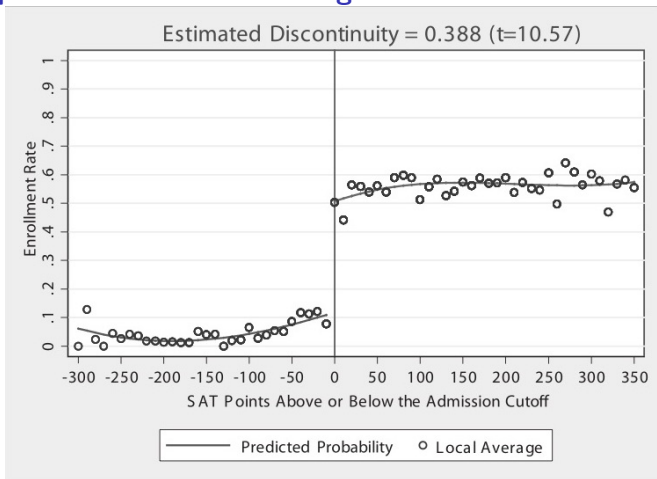
- ▶ IV can identify causal effects when unmeasured confounding exists
- ▶ But IV is *highly sensitive* to assumption violations
 - ▶ Small violations (especially with weak instruments) can cause large bias
 - ▶ Requires careful assessment, diagnostics, and transparency

Regression discontinuity designs (RDD)

- ▶ Treatment is assigned based on a cutoff in a continuous, “running” variable
- ▶ Provides a way to estimate causal effects near the cutoff

Regression discontinuity designs

Example: Test scores and college enrollment



Hoekstra (2009), from Cunningham 2021.

Regression discontinuity designs

Assumptions

- ▶ Continuity: No unobserved confounders at the cutoff
- ▶ No manipulation: No one can manipulate the running variable to gain treatment
- ▶ Local randomization: Units just above and below the cutoff are similar
- ▶ Treatment effect is local: The estimated effect is valid only for units near the cutoff

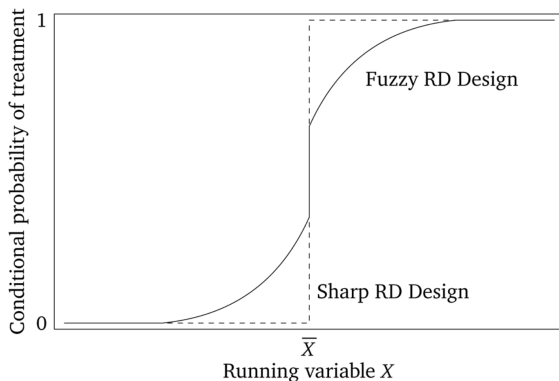
Regression discontinuity designs

Sharp vs. fuzzy designs

- ▶ Sharp RDD: Treatment status perfectly determined at the cutoff
- ▶ Fuzzy RDD: Probability of treatment changes at the cutoff
- ▶ Both can be used for causal inference if assumptions hold

Regression discontinuity designs

Sharp vs. fuzzy RDD



Cunningham 2021.

Difference-in-differences

- ▶ Compare changes over time between treated and control groups
- ▶ Basic idea: Subtract differences to account for trends
 - ▶ Removes any bias due to fixed unobservables

Difference-in-differences

Basic DiD estimator

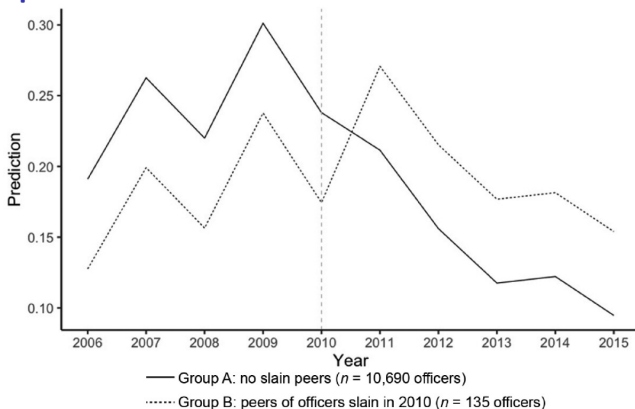
- ▶ Where T denotes treatment and C denotes control, and 0 is before and 1 is after:

$$\hat{\text{DiD}} = (\bar{Y}_{T,1} - \bar{Y}_{T,0}) - (\bar{Y}_{C,1} - \bar{Y}_{C,0})$$

- ▶ First difference: Change over time in treated group
- ▶ Second difference: Change over time in control group
- ▶ DiD compares these changes to estimate treatment effect

Difference-in-differences

Example: Police officer deaths and excessive force



Zhao, L., & Papachristos, A. V. 2024. Threats to Blue Networks: The Effect of Partner Injuries on Police Misconduct. *American Sociological Review*, 89(1), 159-195.

Difference-in-differences

Key assumption

- ▶ Parallel trends
 - ▶ In the absence of treatment, treated and control groups would have moved in parallel (allowing common shocks)
 - ▶ No time-varying unobservable confounders

Difference-in-differences

Extensions and considerations

- ▶ Multiple periods allow testing for pre-treatment trends
- ▶ Event study designs can show treatment dynamics over time
- ▶ Staggered adoption of treatment complicates standard DiD

Causal inference and regression

Causation and description

- ▶ Descriptive claims are valuable and descriptive regression models can provide rich insights into social processes
- ▶ Causal thinking is important for model specification and inference techniques can be used to make stronger causal claims
- ▶ But causal inference often requires that we focus on a very narrowly defined problem, losing some of the richness of description

Causal inference and regression

Using causal inference techniques

- ▶ Causal inference is difficult in observational settings due to selection bias and typically requires strong assumptions
- ▶ Triangulation across multiple methodologies is often necessary
- ▶ Complex methodological literature associated with each methodology
 - ▶ Diagnostic tests and bias analyses important
 - ▶ Robustness checks using alternative estimators and specifications

Causal inference and regression

Four designs, one goal

- ▶ **PSM/weighting**: assume selection on observables; make treated & control alike
- ▶ **IV**: let Z create exogenous variation in D (relevance + exclusion)
- ▶ **RDD**: near cutoff, units are as-if random
- ▶ **DiD**: differencing removes *time-invariant* unobservables; needs parallel trends

Summary

- ▶ Causal inference is difficult in observational settings due to selection bias
- ▶ Various regression-based techniques can be used to infer causality
- ▶ All techniques entail strong assumptions and require careful evaluation to make valid inferences