

# Heterogeneity, Endogeneity and Causal Effect Estimation

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$$\phi_r = \frac{1}{\sqrt{2\pi\sigma^2}}$$

$$D_x = \sigma^2 = M_x^2 - (M_x^2)^2$$

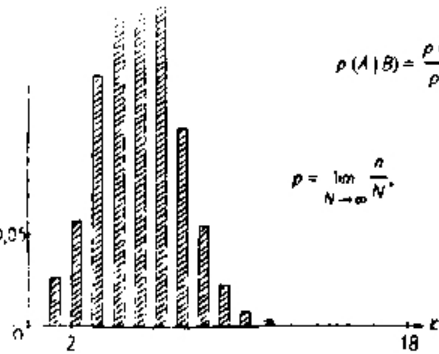
$$p_x(\lambda) = \frac{\lambda^e}{e!} e^{-\lambda}$$

$$p(x_1 \leq x \leq x_2) = \int_{x_1}^{x_2} \phi(x) dx$$

$$\phi(x) = \frac{1}{\sqrt{2\pi\sigma^2}} e^{-\frac{(x-\mu)^2}{2\sigma^2}}$$

$$\phi(x) = \frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{x^2}{2\sigma^2}}$$

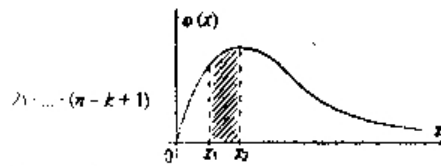
$$\phi(v) = 4\sqrt{\frac{k^3}{\pi}} v^2 e^{-kv^2}$$



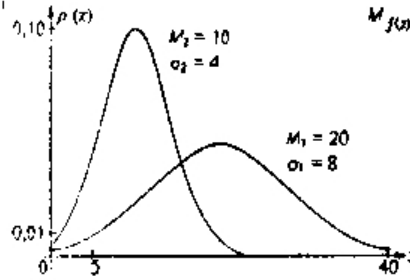
$$p(A|B) = \frac{p(AB)}{p(B)}$$

$$p = \lim_{N \rightarrow \infty} \frac{n}{N}$$

$$D_x = \sum_{i=1}^k p_i (x_i - M_x)^2$$



$$\lambda_1, \dots, (\pi-k+1)$$



$$f(A_p)$$

$$f_p(A_p)$$

$$f_p(A_p) = f(A_p, A_p)$$

$$D_x = \int_{-\infty}^{\infty} (x - M_x)^2 \phi(x) dx$$

$$M_x = \int_{-\infty}^{\infty} x \cdot \phi(x) dx$$

$$M_{f(x)} = \int_{-\infty}^{\infty} f(x) \phi(x) dx$$

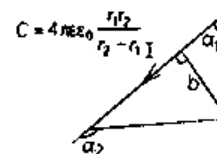
$$S = v_0^2 + \frac{\sigma^2}{2}$$

$$F = G \frac{m_1 m_2}{R^2}$$

$$f(v) = 4\pi \left( \frac{m_0}{2\pi kT} \right)^{3/2} v^2 e^{-\frac{mv^2}{2kT}}$$

$$\phi(\ln x) d(\ln x) = \frac{1}{\sqrt{2\pi\sigma^2}} e^{-\frac{(\ln x - \mu)^2}{2\sigma^2}} d(\ln x) = \frac{1}{\sqrt{2\pi\sigma x}} e^{-\frac{(\ln x - \mu)^2}{2\sigma^2}} dx$$

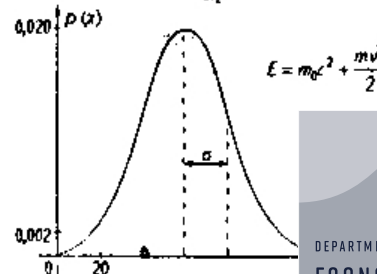
$$\langle r \rangle = \frac{\langle r^2 \rangle}{n\sqrt{2\pi d^2}}$$



$$B = \frac{\mu_0 f}{2\pi b} (\cos \alpha_1 - \cos \alpha_2)$$

$$A^2 = A_1^2 + A_2^2 + 2A_1 A_2 \cos(\varphi_2 - \varphi_1)$$

$$C = \frac{\epsilon \epsilon_0 S}{d}$$



$$E = m_0 c^2 + \frac{mv^2}{2}$$

$$h\nu = A + \frac{mv^2}{2}$$



- Potential Outcomes
- Challenges in Effect Estimation
- Experimental and Quasi-Experimental Data
  - Randomized Controlled Experiments and ATE
  - Imperfect Compliance and LATE
- Observational Data
  - Regression Discontinuity
  - Difference-in-Difference
  - Panel Models

- Observed outcome for individual or firm  $i$

$$Y_i$$

- $D_i$  is the treatment status variable for individual  $i$

$$D_i = \begin{cases} 0 & \text{if untreated} \\ 1 & \text{treated} \end{cases}$$

- Outcome variable is determined by

$$Y_i = \beta_{0i} + \beta_{1i}D_i$$

- $\beta_{1i}$  is a heterogeneous treatment effect for individual  $i$
- Also known as the *potential outcomes* model
- Two outcomes

$$Y_i(0) = \beta_{0i} \text{ and } Y_i(1) = \beta_{0i} + \beta_{1i}$$

## Definition (Average Treatment Effect (ATE))

The Average Treatment Effect measures the average effect of treatment across the entire population

$$ATE = E[\beta_{1i}] = E[Y_i(1)] - E[Y_i(0)]$$

## Definition (Average Treatment Effect on the Treated (TOT))

The Average Treatment Effect on the Treated measures the effect of treatment on the treated

$$TOT = E[\beta_{1i}|D = 1] = E[Y_i(1)|D = 1] - E[Y_i(0)|D = 1]$$

- ATE is a weighted average

$$ATE = \omega TOT + (1 - \omega) TUT$$

- Average Treatment Effect on the Untreated (TUT)

$$TUT = E[\beta_{1i}|D=0] = E[Y_{1i}|D=0] - E[Y_{0i}|D=0]$$

- $\omega = \Pr[D=1]$  if the probability treated
- Should we measure ATE or TOT?
  - TOT makes sense when treatment is non-compulsory
    - Individuals who do not undertake treatment are not relevant for cost-benefit calculation
  - ATE is more sensible for mandatory programs
    - Measures the effect on both those who would like to participate and those who would not

- Estimate the regression on observed data

$$Y_i = b_0 + b_1 D_i + \epsilon_i$$

- $\hat{b}_{0i} \xrightarrow{p} E[Y_i | D = 0]$
  - $\hat{b}_1 \xrightarrow{p} E[Y_i | D = 1] - E[Y_i | D = 0]$ .

- Leads to selection bias

$$\underbrace{E[Y_i | D = 1] - E[Y_i | D = 0]}_{\text{Observed Effect}} = \underbrace{E[Y_i(1) | D = 1] - E[Y_i(0) | D = 1]}_{\text{Avg. Treatment Effect on the Treated (TOT)}} + \underbrace{E[Y_i(0) | D = 1] - E[Y_i(0) | D = 0]}_{\text{Selection Bias (SB)}}$$

- In terms of the regression

$$\underbrace{E[\hat{b}_1]}_{\text{Observed Effect}} = \underbrace{E[\beta_{1i} | D = 1]}_{\text{TOT}} + \underbrace{E[\beta_{0i} | D = 1] - E[\beta_{0i} | D = 0]}_{\text{Selection Bias (SB)}}$$

- SB is the difference in the no-treatment outcomes for the treated and untreated

- Fundamental problem: Cannot see counterfactual

		Treatment ( $D_i$ )	
		0	1
Observe		$Y_i(0) = \beta_{0i}$	$Y_i(1) = \beta_{0i} + \beta_{1i}$
Counterfactual		$Y_i(1) = \beta_{0i} + \beta_{1i}$	$Y_i(0) = \beta_{0i}$

- No data on  $Y_i(1)$  when  $D_i = 0$  and  $Y_i(0)$  when  $D_i = 1$
- TOT measures the effect conditional on receiving treatment
  - Missing counterfactual:  $E[Y_i(0) | D = 1]$
- Observed effect is contaminated with selection bias

- Example: Financial Stress and Payday loans
- Outcome is a measure of financial distress: 90-days delinquent on a debt
- Treatment is taking out a payday loan
- *TOT*: Difference in delinquency if loan taken or not *given* loan wanted ( $D = 1$ )
- *SB*: Difference in outcome if loan not taken for those who want a loan and those who do not want a loan
  - Plausible *TOT* is negative but *SB* is positive
  - Positive *SB* if

$$E [\beta_{0i}|D = 1] > E [\beta_{0i}|D = 0]$$

- Default rates absent a loan are higher for loan takers than for non-takers
- Observed effect could have either sign



- Randomization removes selection bias
- Well executed Randomized Controlled Trials are the gold standard for causal effect estimation
- A RCT ensures that

$$\{\beta_{0i}, \beta_{1i}\} \perp\!\!\!\perp D_i \text{ and } \{Y_i(0), Y_i(1)\} \perp\!\!\!\perp D_i$$

- Randomly give loans only to those seeking them
  - Creates group with  $Y_i(0)$  as if  $D = 1$

## Independence and Conditioning

If  $Z$  and  $W$  are independent random variables, then  $E[Z|W = w_1] = E[Z|W = w_2] = E[Z]$ .

- Knowledge of  $W$  provides no information about  $Z$ .

## Gains to Randomization

$E[Y_i(0)|D=0] = E[Y_i(1)|D=1]$  and  $E[\beta_{0i}|D=1] = E[\beta_{0i}|D=0]$  since treatment independent of desire to be treated

- Track outcomes of both groups

$$\underbrace{E[Y_i|D=1] - E[Y_i|D=0]}_{\text{Observed Effect with Randomization}} = E[Y_i(1)|D=1] - E[Y_i(0)|D=0]$$
$$= E[Y_i(1)|D=1] - E[Y_i(0)|D=1]$$

- In the notation of a regression model

$$\begin{aligned} E[\hat{b}_1] &= E[\beta_{1i}|D=1] + \{E[\beta_{0i}|D=1] - E[\beta_{0i}|D=0]\} \\ &= E[\beta_{1i}|D=1] + \{E[\beta_{0i}|D=1] - E[\beta_{0i}|D=1]\} \\ &= E[\beta_{1i}|D=1] \end{aligned}$$

- Internal Validity: are the results valid for the sample used?
  - Is the assignment actual random?

$$X_{it} = \alpha + \beta D_{it} + \epsilon_t, H_0 : \beta = 0, H_1 : \beta \neq 0$$

- Are participants complying?
  - Are there spill-overs of non-rival treatments to non-treated?
  - Hawthorne Effect: studying a subject changes their behavior
- External Validity: do the results generalize to a broader sample?
  - Is the RCT sample representative of the target population?
  - Are there other key personnel that are essential for success?

- Previous result requires perfect compliance
  - Treated if offered, not-treated if not offered
- When treatment is not random, or compliance is not perfect, simple estimators are not consistent
- Possible to use an *instrument* to recover a meaningful measure of treatment effect
- Measure is *local* in the sense that it measures the effect of a particular subgroup of the treated
- Notation
  - $D_i$  is treatment status
  - $Z_i$  is treatment *assignment* (offer to treat)
- Compliance
  - Perfect if  $D_i = Z_i$
  - Imperfect if  $D_i \neq Z_i$  for some  $i$
- $Z_i$  may be random even if  $D_i$  is not
  - Treatment assignment is made by lottery due to limited capacity ( $Z_i$ )
  - Treatment status *conditional on offer* depends on expected benefits ( $D_i$ )

- Leads to two-equation system

$$\text{Structural Equation } Y_i = \beta_{0i} + \beta_{1i}D_i$$

$$\text{Treatment Equation } D_i = \pi_{0i} + \pi_{1i}Z_i$$

- Causal chain  $Z_i \rightarrow D_i \rightarrow Y_i$
- Treatment equation measures potential treatment status

$$D_i(z) = \pi_{0i} + \pi_{1i}z$$

- $D_i(0) = \pi_{0i}$  is status when not assigned
  - $D_i(1) = \pi_{0i} + \pi_{1i}$  is status when assigned
  - Both  $D_i(0)$  and  $D_i(1)$  may be 0 or 1
- Treatment responsiveness  $\pi_{1i}$  is heterogeneous like treatment effect  $\beta_{1i}$

## Assumption (Independence)

*The potential outcomes and potential treatment assignments are independent of  $Z_i$*

$$\{\beta_{0i}, \beta_{1i}, \pi_{0i}, \pi_{1i}\} \perp\!\!\!\perp Z_i$$

- Often described as *as if* randomly assigned
- Note that the instrument is independent of the potential treatment status
- $Z_i$  does not affect the probability that either occur ( $\pi_{\bullet i}$ )
- $Z_i$  does not affect the outcomes if treatment is taken or not ( $\beta_{\bullet i}$ )
- **Is this a reasonable assumption?**
  - Often plausible when  $Z_i$  is assigned using randomization (lottery)
  - Sometimes plausible for  $Z_i$  taken from observational data

## Assumption (Exclusion Restriction)

*The instrument does not appear in the structural equation so that only treatment assignment affect the outcome.*

- Violations of the exclusion restriction mean that  $Z_i$  affects  $Y_i$  through more than just  $D_i$
- Classic example is when  $Z_i$  directly affects both  $Y_i$  and  $D_i$
- In many cases,  $Z_i$  affects  $D_i$  and another variable  $X_i$  which in turn affects  $Y_i$

$$Z_i \rightarrow D_i \rightarrow Y_i, \quad Z_i \rightarrow X_i \rightarrow Y_i$$

- Suppose selection for a randomly assigned government funding program increases probability of program participation ( $Z_i \rightarrow D_i$ )
- If selection also increases the probability that a firm receives series  $B$  funding, than effect confounded with fund raising ( $Z_i \rightarrow X_i$ )
- Exclusion restriction ensures that  $Z$  does not affect the potential outcome

$$Y(0)_i = \beta_{0i} \text{ and } Y(1)_i = \beta_{0i} + \beta_{1i} \text{ for } Z \in \{0, 1\}$$

- The 2SLS estimator obtained by
  1. Regress  $D_i = p_0 + p_1 Z_i + \eta_i$  and retain  $\hat{D}_i = \hat{p}_0 + \hat{p}_1 Z_i$
  2. Regress  $Y_i = b_0 + b_1 \hat{D}_i + \epsilon_i$
- In large samples

$$\hat{b}_1^{2SLS} \xrightarrow{p} \frac{E[\beta_{1i}\pi_{1i}]}{E[\pi_{1i}]} = E\left[\beta_{1i} \frac{\pi_{1i}}{E[\pi_{1i}]}\right] = LATE$$

- *LATE* is a weighted average of treatment effects
- Weights are determined by responsiveness to treatment assignment
  - Holds if either of  $D_i$  or  $Z_i$  are not binary
- If effects are not heterogeneous ( $\beta_{1i} = \beta_1$  or  $\pi_{1i} = \pi_1$ ) then  $LATE = ATE$



- Useful to describe structure implied by  $D_i$  and  $Z_i$
- Four types of program participants
  - Compliers:  $D_i = Z_i$  ( $\pi_{0i} = 0, \pi_{1i} = 1$ )
  - Always-takers:  $D_i = 1$  for any  $Z_i$  ( $\pi_{0i} = 1, \pi_{1i} = 0$ )
  - Never-takers:  $D_i = 0$  for any  $Z_i$  ( $\pi_{0i} = \pi_{1i} = 0$ )
  - Defiers:  $D_i = 1 - Z_i$  ( $\pi_{0i} = 1, \pi_{1i} = -1$ )
- Compliers are the ideal candidates and ultimately what we can measure
- Defiers invalidate measurement using the instrument
- *LATE* is determined only by compliers and defiers

## Assumption (No Defiers)

*There are no defiers, so that  $\pi_{1i} \geq 0$ .*

With this additional assumption

$$LATE = E [\beta_{1i} | \pi_{1i} = 1]$$

so that *LATE* only measures the treatment response of the compliers.

- Common to report the effect on the intention-to-treat

$$\begin{aligned}ITT &= E[Y_i|Z = 1] - E[Y_i|Z = 0] \\&= E[\beta_{1i}\pi_{1i}] \\&= LATE \times E[\pi_{1i}]\end{aligned}$$

- Difference in outcomes conditional on only the instrument which measures the intention to treat
- With perfect compliance  $ITT = LATE$

- 2008 Medicaid expansion in US state of Oregon
- Used a lottery to choose participants from a waiting list
- Constructed a control group from non-winners
- Not everyone selected participated in the program (imperfect compliance)
- Non-selected prohibited from participation

Finkelstein, A., Taubman, S., Wright, B., Bernstein, M., Gruber, J., Newhouse, J.P., Allen, H., Baicker, K. and Oregon Health Study Group, 2012. The Oregon Health Insurance Experiment: evidence from the first year. *The Quarterly journal of economics*, 127(3), pp.1057-1106.

- Estimating Intent-to-Treat (*ITT*)

$$Y_{ihj} = \beta_0 + \beta_1 LOTTERY_h + \mathbf{X}_{ih}\boldsymbol{\beta}_2 + \mathbf{V}_{ih}\boldsymbol{\beta}_3 + \epsilon_{ihj}$$

- $i$ : individual,  $h$ : household,  $j$ : domain of variable
- $LOTTERY_h$  indicates household was selected by the lottery ( $Z_i = 1$ )
- $\mathbf{X}_{ih}$  are required controls and  $\mathbf{V}_{ih}$  are optional controls

- Estimating *LATE*

$$Y_{ihj} = \pi_0 + \pi_1 INSURANCE_{ih} + \mathbf{X}_{ih}\boldsymbol{\pi}_2 + \mathbf{V}_{ih}\boldsymbol{\pi}_3 + \nu_{ihj}$$

- $INSURANCE_{ih}$  is a measure of insurance coverage ( $D_i$ )
- Use 2SLS

$$INSURANCE_{ih} = \delta_0 + \delta_1 LOTTERY_h + \mathbf{X}_{ih}\boldsymbol{\delta}_2 + \mathbf{V}_{ih}\boldsymbol{\delta}_3 + \xi_{ihj}$$

- Insurance is “Ever on Medicaid”

TABLE VIII  
FINANCIAL STRAIN (SURVEY DATA)

	Control mean (1)	ITT (2)	LATE (3)	<i>p</i> -values (4)
Any out of pocket medical expenses, last six months	0.555 (0.497)	−0.058 (0.0077)	−0.200 (0.026)	[<0.0001] {<0.0001}
Owe money for medical expenses currently	0.597 (0.491)	−0.052 (0.0076)	−0.180 (0.026)	[<0.0001] {<0.0001}
Borrowed money or skipped other bills to pay medical bills, last six months	0.364 (0.481)	−0.045 (0.0073)	−0.154 (0.025)	[<0.0001] {<0.0001}
Refused treatment because of med- ical debt, last six months	0.081 (0.273)	−0.011 (0.0041)	−0.036 (0.014)	[0.01] {0.01}
Standardized treatment effect		−0.089 (0.010)	−0.305 (0.035)	[<0.0001]

- Repeated cross-sections across multiple periods
- Examine evidence between treated group and control group
- Control chosen to be similar except for treatment
- Basic model

$$Y_{it} = \alpha_i + \beta_t + \epsilon_{it}$$

- Assume two periods, treatment only in second
- Treated individuals or firms have

$$Y_{it} = \alpha_i + \beta_t + \delta D_{it} + \epsilon_{it}$$

- Two groups,  $\mathcal{A}$  (treated) and  $\mathcal{B}$  (untreated)
- Construct averages

$$E[\bar{Y}_{\mathcal{A}t}] = \alpha_{\mathcal{A}} + \beta_t + \delta I_{[t=2]}, \quad t = 1, 2$$

$$E[\bar{Y}_{\mathcal{B}t}] = \alpha_{\mathcal{B}} + \beta_t$$

- Difference across groups removes own effects

$$E[\bar{Y}_{\mathcal{A}2} - \bar{Y}_{\mathcal{A}1}] = \beta_2 - \beta_1 + \delta$$

$$E[\bar{Y}_{\mathcal{B}2} - \bar{Y}_{\mathcal{B}1}] = \beta_2 - \beta_1$$

- Difference across time removes time effects

$$E[\bar{Y}_{\mathcal{A}2} - \bar{Y}_{\mathcal{B}2}] = \alpha_{\mathcal{A}} - \alpha_{\mathcal{B}} + \delta$$

$$E[\bar{Y}_{\mathcal{A}1} - \bar{Y}_{\mathcal{B}1}] = \alpha_{\mathcal{A}} - \alpha_{\mathcal{B}}$$

- Solution is to difference twice

$$E[\bar{Y}_{\mathcal{A}2} - \bar{Y}_{\mathcal{A}1} - \bar{Y}_{\mathcal{B}2} - \bar{Y}_{\mathcal{B}1}] = \delta$$



- Key assumption

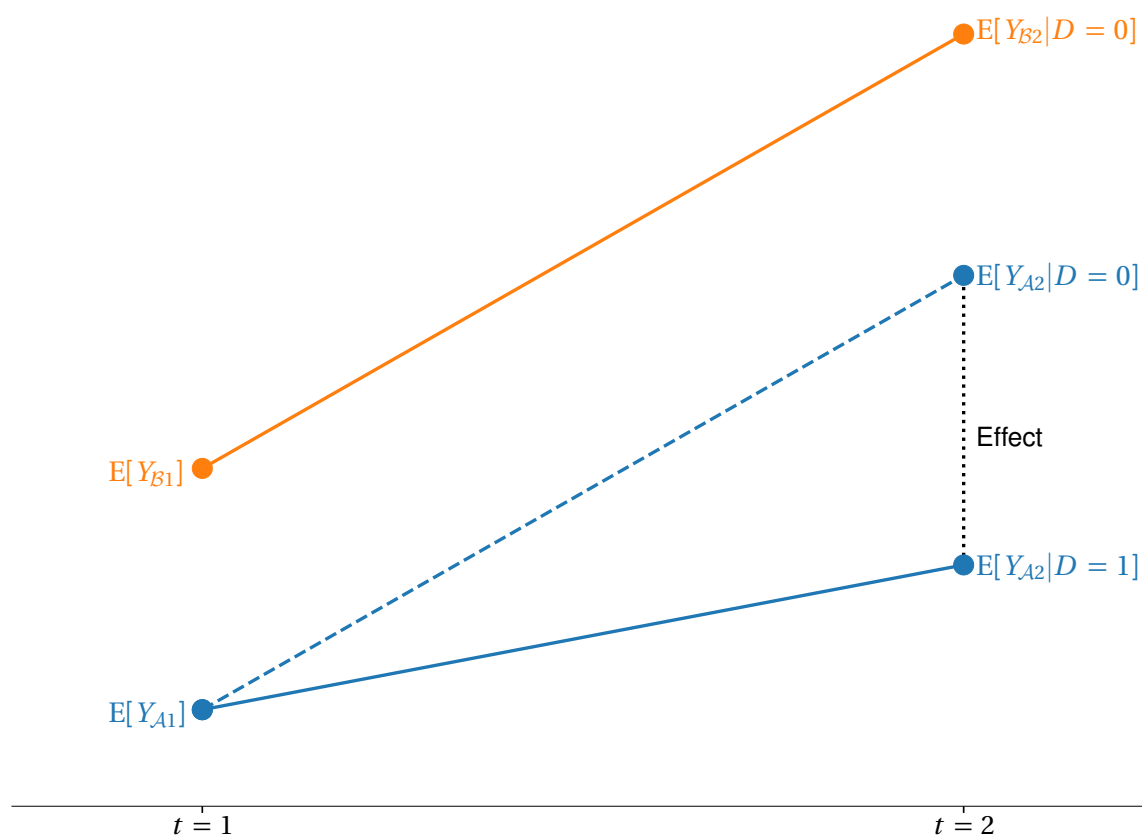
## Assumption (Counterfactual (Parallel Trends))

$$E[Y_{i2} - Y_{i1} | D = 1] = E[Y_{i2} - Y_{i1} | D = 0]$$

*The growth rate does not depend on whether the unit is treated ( $D = 1$ ) or untreated.*

- Uses lagged average of  $\mathcal{A}$  to remove group-specific effects
- Generates counter-factual using group  $\mathcal{B}$

- Counter-factual can be interpreted as parallel line



- Estimated as a regression

$$Y_{it} = \alpha_i + \beta_t + \delta I_{[i \in \mathcal{A}, t=2]} + \epsilon_{it}$$

- Extends trivially to include other controls

$$Y_{it} = \alpha_i + \beta_t + \delta I_{[i \in \mathcal{A}, t=2]} + \mathbf{X}_{it}\boldsymbol{\gamma} + \epsilon_{it}$$

- Uses dummy variables

$$Y_{it} = \alpha_{\mathcal{A}} I_{[i \in \mathcal{A}]} + \alpha_{\mathcal{B}} I_{[i \in \mathcal{B}]} + \beta_1 I_{[t=1]} + \beta_2 I_{[t=2]} + \delta I_{[i \in \mathcal{A}, t=2]} + \epsilon_{it}$$

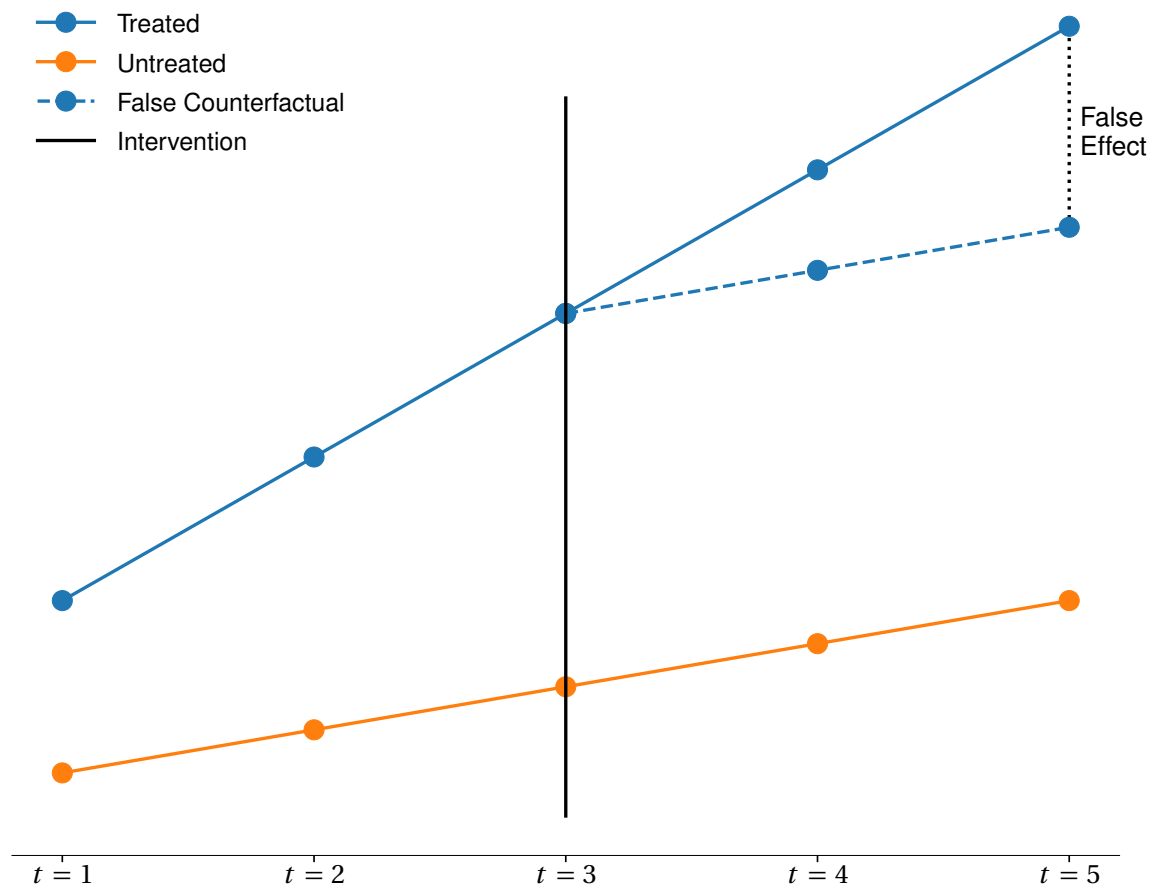
- Extends to multiple groups and time periods

$$Y_{it} = \sum_{g=1}^G \alpha_g I_{[i \in \mathcal{G}_g]} + \sum_{j=1}^T \beta_j I_{[t=j]} + \delta I_{[t \geq \tau_i]} + \mathbf{X}_{it}\boldsymbol{\gamma} + \epsilon_{it}$$

where  $\tau_i$  is the treatment time for individual  $i$

- Main issue: Violation of parallel trends assumption

$$\beta_{\mathcal{A}t} \neq \beta_{\mathcal{B}t}$$

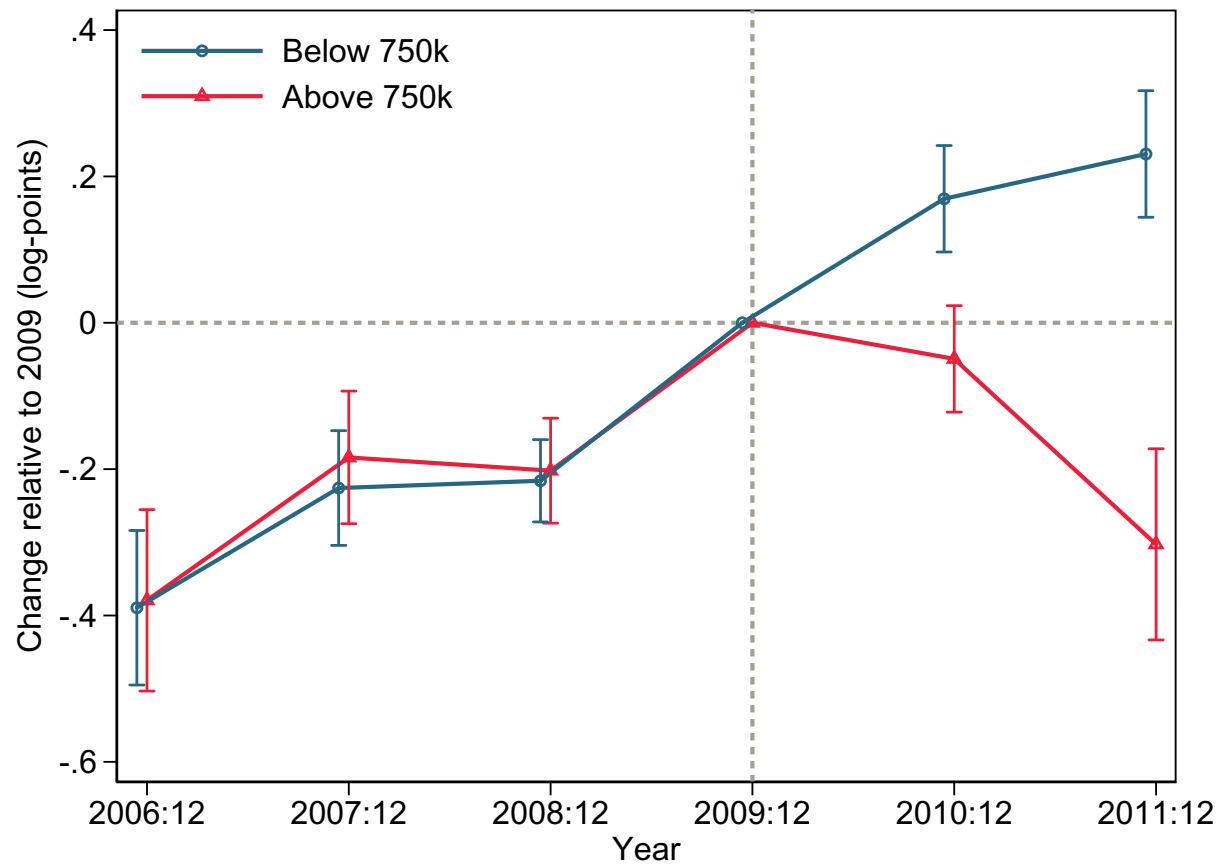


- Examine how uninsured deposits are affected by implicit *Too Big Too Fail* (TBTF) guarantees
- Variation introduced by changes in the limits of the amounts insured
- Main model

$$\ln(\text{Deposits})_{btk} = \alpha + \beta \times \text{Above}_k + \beta_2 \times \text{After}_t + \beta_3 \times \text{Above}_k \times \text{After}_t + \mathbf{X}_i \boldsymbol{\gamma} + \epsilon_{btk}$$

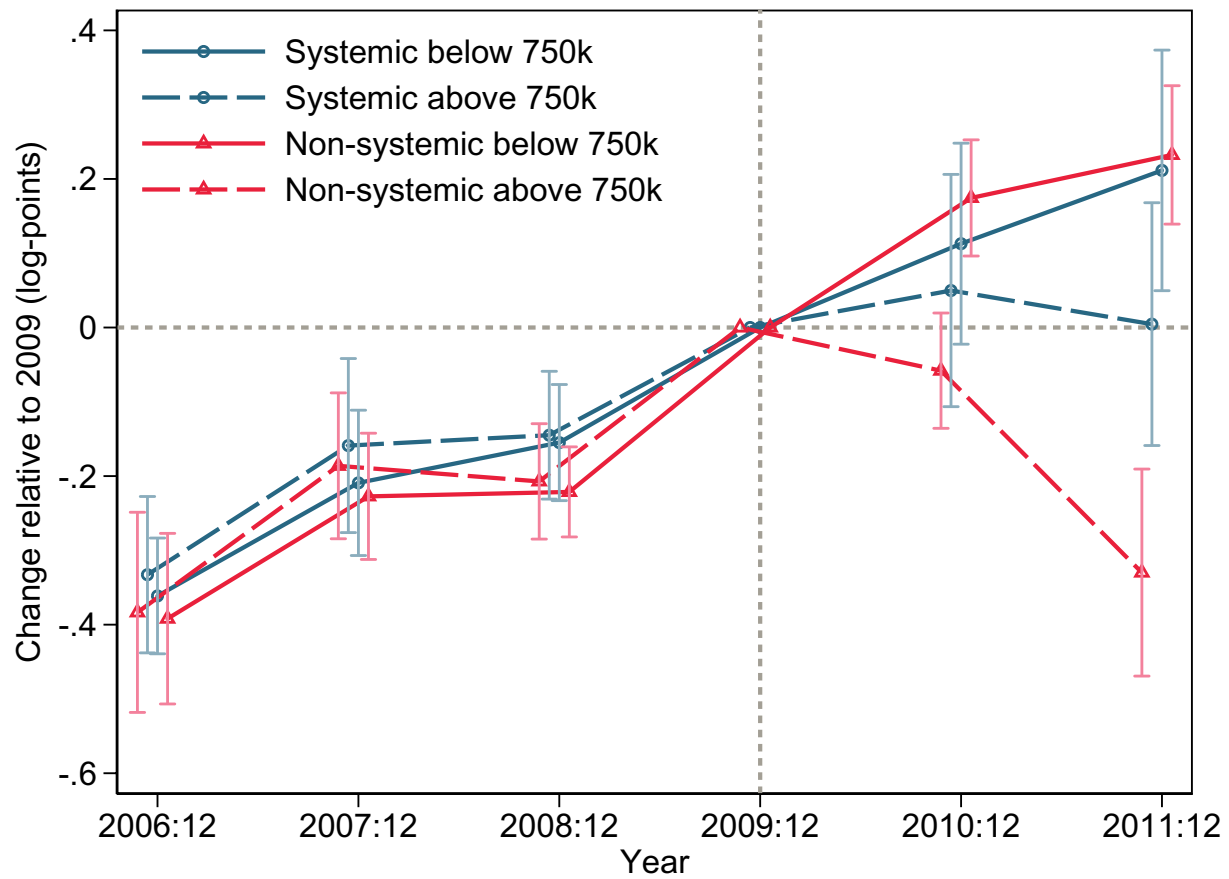
- Bank  $b$ , Year  $t$
  - $k$  indicates the range of deposits in DKK 50,000 bins (e.g., 0–50K, 50K–100K, ...)
  - Interest in the difference between accounts that remain insured with those that become uninsured
- Key variable is  $\text{Above}_k \times \text{After}_t$  which would have a 0 coefficient if no effect

Iyer, R., Lærkholm Jensen, T., Johannesen, N., & Sheridan, A. (2019). The Distortive Effects of Too Big To Fail: Evidence from the Danish Market for Retail Deposits. *The Review of Financial Studies*, 32(12), 4653-4695.



**Table 2**  
**Baseline results**

	(1)	(2)	(3)	(4)
	Deposits (in logs)			
After reform × Above limit	−0.363*** (0.0462)	−0.373*** (0.0430)	−0.378*** (0.0501)	−0.366*** (0.0500)
After reform	0.292*** (0.0403)	0.304*** (0.0394)	0.305*** (0.0456)	
Above limit	−0.578*** (0.0214)	−0.593*** (0.0187)		
Equity / debt (in 2007)	1.941 (1.452)			
Loans / assets (in 2007)	1.302 (0.834)			
Log of assets (in 2007)	0.802*** (0.0398)			
Constant	1.154 (0.773)			
Observations	3,507	3,507	3,507	3,507
R-squared	.869	.951	.970	.990
Bank FEs	No	Yes	Yes	Yes
Bank-range FEs	No	No	Yes	Yes
Bank-time FEs	No	No	No	Yes





- Regression discontinuity exploits discrete jumps in treatment as a function of an observable  $X_i$
- Individuals with  $X_i \leq \bar{X}$  are untreated
- Individuals with  $X_i > \bar{X}$  are treated
- Theoretical framework examines the difference locally with  $\bar{X}$

$$E[Y_i | 0 < X_i - \bar{X} < h] - E[Y_i | 0 < \bar{X} - X_i < h]$$

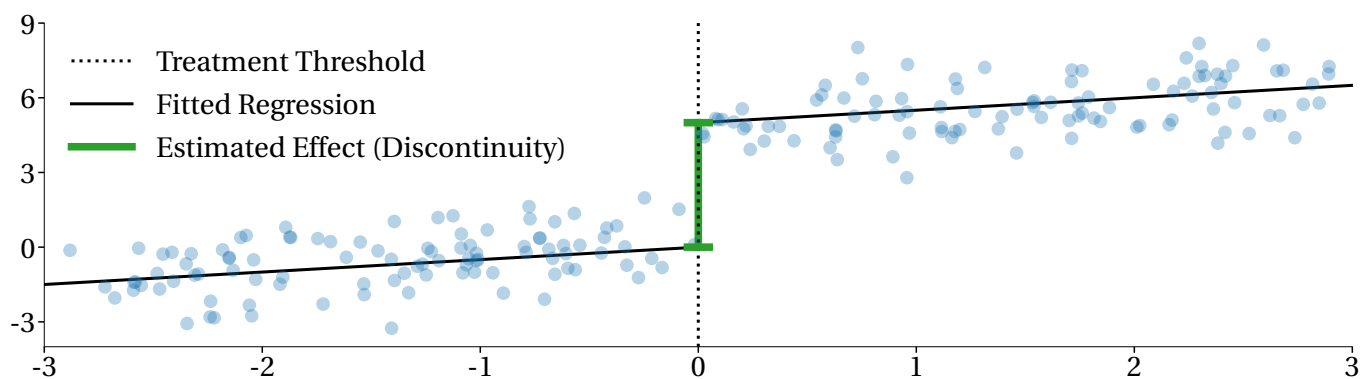
- $h$  is a bandwidth parameter that shrinks to 0 as the sample size increase
- Intuition is that individuals are homogeneous local to  $\bar{X}$

- In practice often included as part of a model

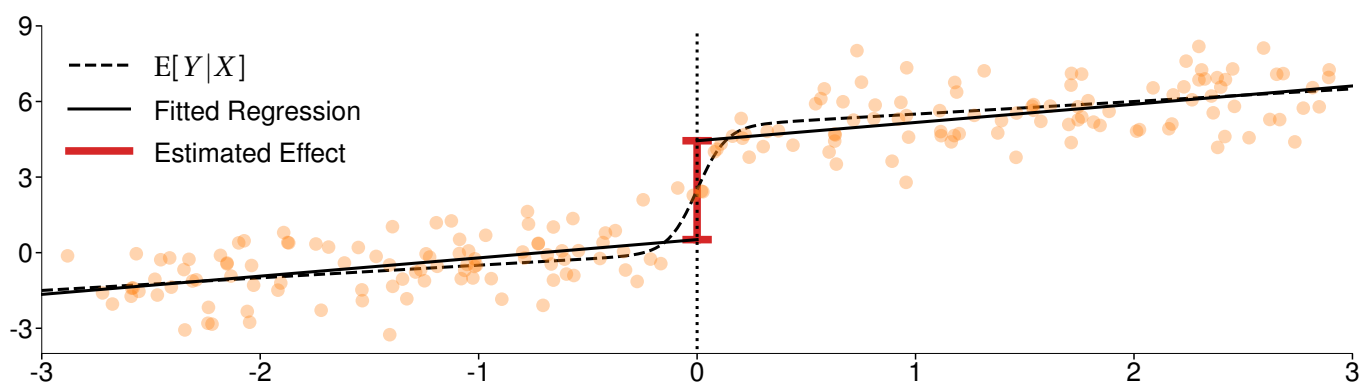
$$Y_i = \beta_0 + \beta_1 X_i + \delta I_{[X_i \geq \bar{X}]} + \epsilon_i$$

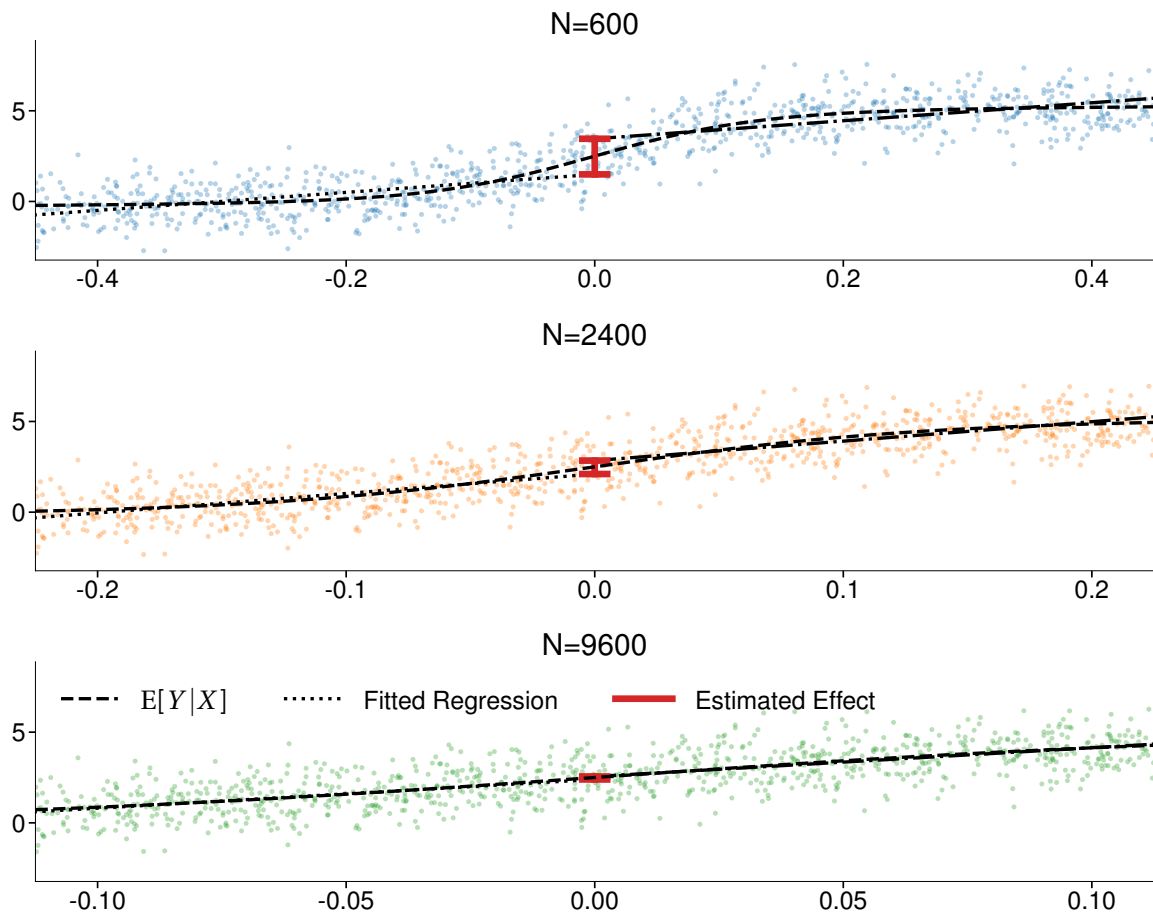
- Can use more sophisticated models
- Causality from model requires treatment indicator  $I_{[X_i \geq \bar{X}]}$  to be uncorrelated with omitted variables
- Also requires functional form to be correct that that  $E[\epsilon_i | X] = 0$ 
  - Rules out neglected nonlinearities

## Linear Model with a Discontinuity



## Neglected Nonlinearity





- Investigate the effect of CSR proposals that just pass or just fail board votes
- **Key assumption:** the difference between just passing and failing is *as if* random
  - Close failures and close passes are identical aside from the vote
- Regress abnormal returns on a dummy for passing
  - Returns computed using Carhart 4 factor model (FF3 + Momentum)
- Use RDD estimate is based on a small window near a tied vote
- Also considers a full model which is piece-wise polynomial

$$y_{it} = \beta \times D_{it} + P_l(v_{it}, \gamma_l) + P_r(v_{it}, \gamma_r)$$

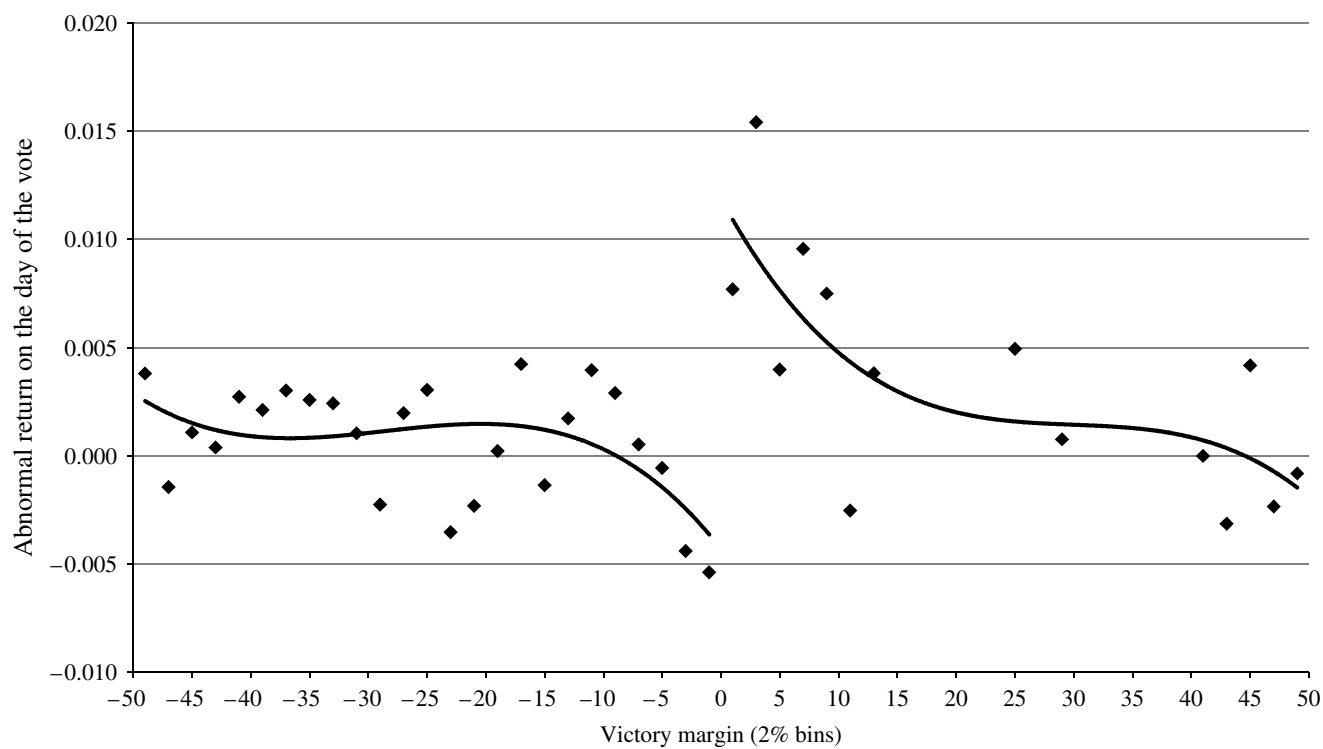
- $P_{\bullet}(v_{it}, \gamma_{\bullet})$  is a polynomial in the vote shape for left and right of passing

Flammer, C. (2015). Does corporate social responsibility lead to superior financial performance? A regression discontinuity approach. *Management Science*, 61(11), 2549-2568.

**Table 5** Abnormal Returns Around the Majority Threshold

	Vote share				Full model (7)	Full model with controls (8)
	$\pm 10\%$ (3)	$\pm 5\%$ (4)	$\pm 2.5\%$ (5)	$\pm 1.5\%$ (6)		
<i>Pass</i>	0.0107** (0.0046)	0.0136** (0.0058)	0.0109*** (0.0038)	0.0117 (0.0074)	0.0118** (0.0053)	0.0107** (0.0052)
<i>R-squared</i>	0.056	0.099	0.255	0.204	0.007	0.044
<i>Observations</i>	122	61	23	9	2,729	1,780

## Does Corporate Social Responsibility Lead to Superior Financial Performance? A RD Approach



- Panel data is double indexed

$$Y_{it}$$

- $i$  is the entity (or unit): Traders, Firms, Borrowers, ...
  - $t$  is the time period
- Panels track entities over time
- $N$  entities,  $T$  time periods
  - $N$  is assumed to be large,  $T$  is usually small
  - Asymptotics assume  $N \rightarrow \infty$



- Panel data can be used to estimate pooled OLS models

$$Y_{it} = \mathbf{X}_{it}\boldsymbol{\beta} + \epsilon_{it}$$

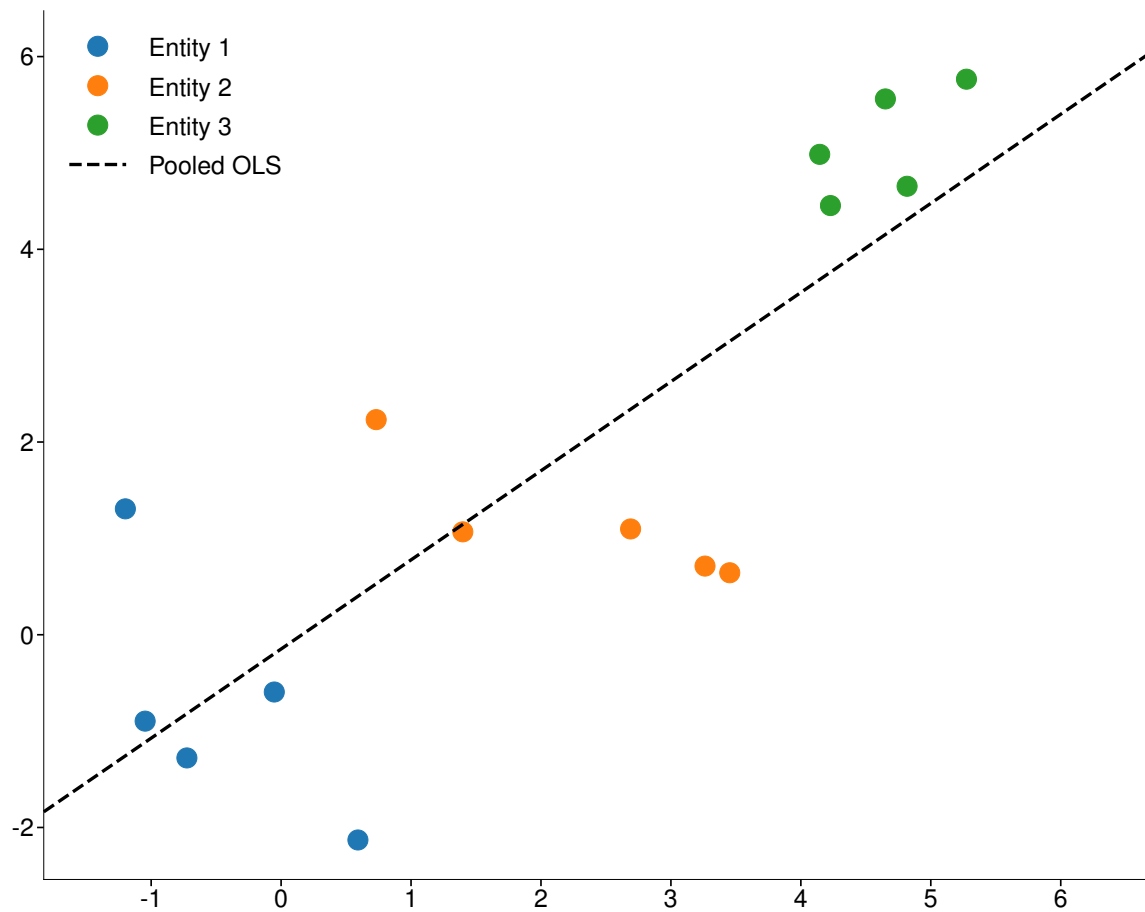
- Ignores the panel structure
- Panel structure allows us to model unobserved heterogeneity

$$Y_{it} = \mathbf{X}_{it}\boldsymbol{\beta} + \mathbf{W}_i\boldsymbol{\gamma} + \epsilon_{it}$$

- $\mathbf{W}_i$  is a vector of entity-specific characteristics
- Key: Assumed to be time invariant
- Estimating pooled OLS results in biased coefficients if  $\mathbf{W}_i$  is correlated with  $\mathbf{X}_{it}$
- In large samples,

$$\begin{aligned}\hat{\boldsymbol{\beta}} &\xrightarrow{p} \boldsymbol{\beta} + \boldsymbol{\Lambda}\boldsymbol{\gamma} \\ \mathbf{W}_i &= \mathbf{X}_{it}\boldsymbol{\Lambda} + \boldsymbol{\eta}_{it}\end{aligned}$$

- This is omitted variable bias



- Could collect data on  $\mathbf{W}_i$  if available, and include in the model
- In many plausible scenarios it is not observable
  - Individual ability or intrinsic motivation
  - Firm management culture
- Fixed Effect estimator allow  $\boldsymbol{\beta}$  to be estimated when  $\mathbf{W}_i$  is not known
- Note that  $\mathbf{W}_i\boldsymbol{\gamma}$  is a constant for entity  $i$

$$Y_{it} = \mathbf{X}_{it}\boldsymbol{\beta} + \omega_i + \epsilon_{it}$$

- Demean entity-by-entity

$$Y_{it} - \bar{Y}_i = (\mathbf{X}_{it} - \bar{\mathbf{X}}_i) \boldsymbol{\beta} + (\omega_i - \bar{\omega}_i) + (\epsilon_{it} - \bar{\epsilon}_i)$$

- $\omega_i$  is time-invariant so  $\omega_i - \bar{\omega}_i = 0$

$$Y_{it} - \bar{Y}_i = (\mathbf{X}_{it} - \bar{\mathbf{X}}_i) \boldsymbol{\beta} + (\epsilon_{it} - \bar{\epsilon}_i)$$

- Note that FE models cannot estimate time-invariant effects

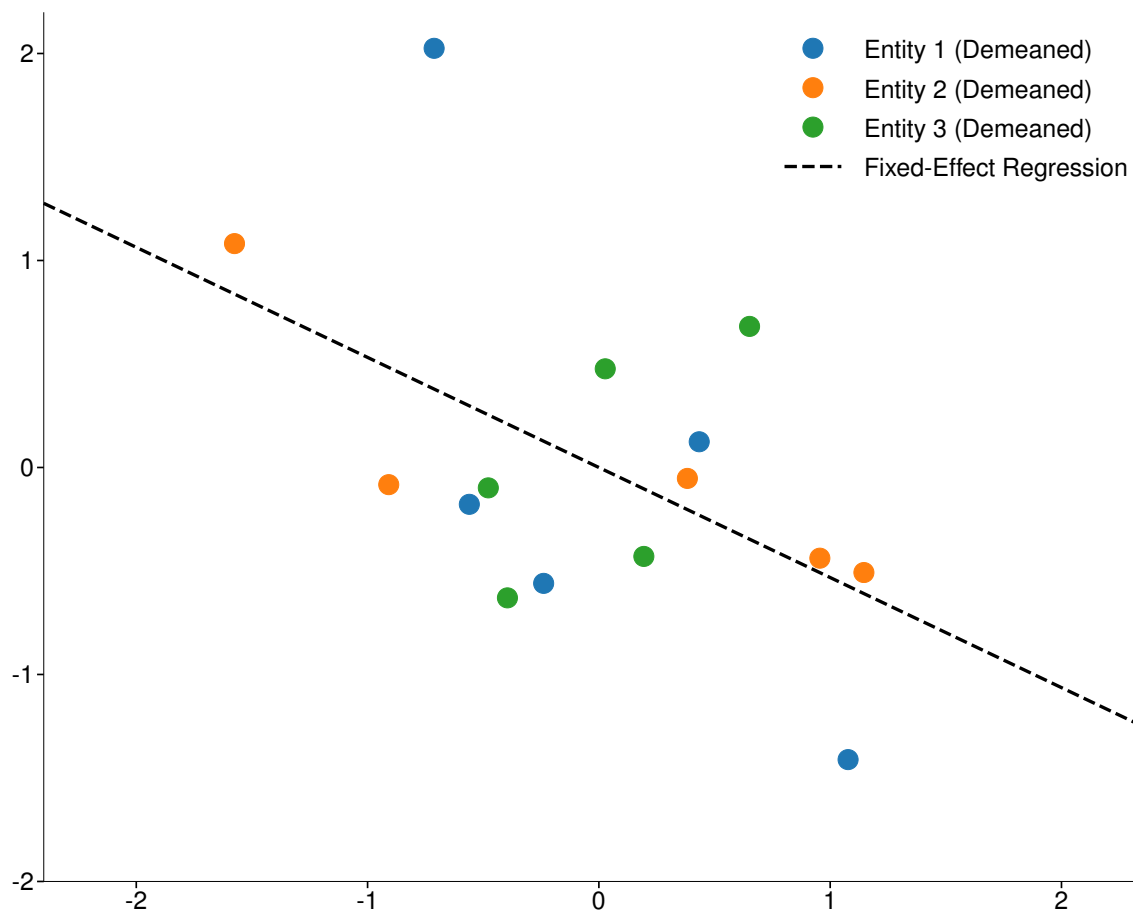
- Model is equivalent to including entity dummies

$$Y_{it} = \mathbf{X}_{it}\boldsymbol{\beta} + D_i\lambda_i + \epsilon_{it}$$

- When  $T = 2$  identical to first-difference estimator

$$Y_{i2} - Y_{i1} = (\mathbf{X}_{i2} - \mathbf{X}_{i1})\boldsymbol{\beta} + (\epsilon_{i2} - \epsilon_{i1})$$

- Inefficient to use first difference estimator for  $T \geq 3$



- Estimates of  $\hat{\omega}_i$  are not consistent when  $T$  is finite
- Estimated using OLS on entity-wise demeaned data

$$\tilde{Y}_{it} = \tilde{\mathbf{X}}_{it}\boldsymbol{\beta} + \tilde{\epsilon}_{it}$$

- Known as the Least Squares Dummy Variable (LSDV) estimator
- Intercept is not meaningful in FE models when reported

- Robust inference requires a clustered variance covariance estimator
  - White Covariance

$$\Sigma_{\mathbf{XX}}^{-1} \mathbf{S} \Sigma_{\mathbf{XX}}^{-1}$$
$$\mathbf{S} = E \left[ \tilde{\epsilon}_{it}^2 \tilde{\mathbf{X}}_{it}' \tilde{\mathbf{X}}_{it} \right], \quad \hat{\mathbf{S}} = (NT)^{-1} \sum_{i=1}^N \sum_{t=1}^T \hat{\epsilon}_{it}^2 \tilde{\mathbf{x}}_{it}' \tilde{\mathbf{x}}_{it}$$

- Clustered (Rogers) covariance

$$\mathbf{S}_C = E \left[ \xi' \xi \right], \quad \hat{\mathbf{S}} = N^{-1} \sum_{i=1}^N \hat{\xi}' \hat{\xi}$$

$$\xi = \sum_{t=1}^T \tilde{\epsilon}_{it} \tilde{\mathbf{X}}_{it}$$

- Replace  $\mathbf{S}$  when an estimator that allows dependence within entity
- $\xi' \xi$  contains all squares and cross-products
- Imposes no restrictions on the dependence *within* an entity

- Panels models often include time effects

$$Y_{it} = \mathbf{X}_{it}\boldsymbol{\beta} + \omega_i + \gamma_t + \epsilon_{it}$$

- $\gamma_t$  is a shock that affects all observations in period  $t$ 
  - Commonly used for model common aggregate movement
- Estimated regression use data demeaned using entity and time period
- When  $N$  is large and  $T$  is small, time effects are consistently estimated
  - Does not need special treatment for inference
  - Identical to including dummies for each time period
- In general, fixed effects can be used to remove constant effects in any dimension with repeated observations
  - Industry
  - City, County, State, or Regional



- Examine how test pass rate is affected by *teacher* financial distress

$$y_{cgt} = \beta \times Bankruptcy_{cgt} + \bar{\mathbf{X}}_{cgt}\boldsymbol{\gamma} + \mathbf{Z}_{cgt}\boldsymbol{\lambda} + \underbrace{\delta_{dt} + \eta_{gt} + \phi_{cg}}_{\text{Fixed Effects}} + \epsilon_{cgt}$$

- Subscripts:
  - $c$ : campus
  - $g$ : grade of student
  - $t$ : year
- $Bankruptcy_{cgt}$  is *teacher* bankruptcy
- $\bar{\mathbf{X}}_{cgt}$  are average teacher characteristics
- $\mathbf{Z}_{cgt}$  are student demographic characteristics

Maturana, G., & Nickerson, J. (2019). Real effects of workers' financial distress: Evidence from teacher spillovers. *Journal of Financial Economics*.

$$y_{cgt} = \beta \times Bankruptcy_{cgt} + \mathbf{X}_{cgt}\boldsymbol{\gamma} + \mathbf{Z}_{cgt}\boldsymbol{\lambda} + \underbrace{\delta_{dt} + \eta_{gt} + \phi_{cg}}_{\text{Fixed Effects}} + \epsilon_{cgt}$$

- Fixed Effects
  - $\delta_{dt}$  - District-Year: Control for local economic conditions
  - $\eta_{gt}$  - Grade-Year: Control for changes to the test across grade and time
  - $\phi_{cg}$  - Campus-Grade: Control for heterogeneity across different campuses and grades
- Estimates are computed as deviations from all three FE
- Standard errors are clustered by campus-grade and campus-year
  - CG allows arbitrary correlation within all students in a single grade and campus in any year
  - CY allows arbitrary correlation within all students in a single campus and year across all grades