Empirical Methods for Policy Evaluation

Second Part

Matteo Bobba

Toulouse School of Economics (TSE)

TSE PhD Program (MRes)
Fall 2024

Outline and Readings for this Section (3 Classes)

- Difference-in-Differences
  - Two-way fixed effect regressions (de Chaisemartin-D'Hautfœuille Book/Survey paper)
  - Heterogeneity-robust DID estimators (dCDH, Book/Survey paper)
- DID and empirical job search models
  - **Bobba, Flabbi and Levy (IER, 2022)**

Two-way fixed effect regressions

Groups and Time Periods

- We consider observations that can be divided into G groups and T periods
- For every $(g, t) \in \{1, ..., G\} \times \{1, ..., T\}$: $=$ nb of obs in group g at period t
- Panel/repeated cross-section data set where groups are, e.g., individuals, firms, counties, etc.
- Cross-section data set where cohort of birth plays the role of time
- One may have $N_{g,t} = 1$, e.g. b/c group=individual or a firm
- For simplicity, we assume hereafter balanced panel of groups:

$$\text{For all } (g, t) \in \{1, ..., G\} \times \{1, ..., T\}, N_{g,t} > 0$$

Treatment and Design

- $D_{g,t}$: treatment of group g and at period t
- $D_{g,t}$ may be non-binary and multivariate
- In some case the treatment may vary across individuals within a group: "fuzzy designs", not considered here
- When $D_{g,t} \in R^+$ increases only once, constant otherwise: "staggered adoption design".

Potential Outcomes, SUTVA, and Covariates

- Let $(d_1, ..., d_T)$ denote a treatment trajectory
- Corresponding potential outcomes: $Y_{g,t}(d_1, ..., d_T)$
- Then observed outcome: $Y_{g,t} = Y_{g,t}(D_{g,1}, ..., D_{g,T})$
- We maintain the usual SUTVA assumption:

$$(Y_{g,1}(d_1, ..., d_T), ..., Y_{g,T}(d_1, ..., d_T)) \amalg (D_{g',t'})_{g' \neq g, t'=1,...,T}, \forall (g, t, d_1, ..., d_T)$$

- For any variable $X_{g,t}$, let $\boldsymbol{X}_g = (X_{g,1}, ..., X_{g,T})$ and $\boldsymbol{X} = (\boldsymbol{X}_1, ..., \boldsymbol{X}_G)$.

The Pervasiveness of Two-way Fixed Effect Regressions

- Researchers often consider two-way fixed effects (TWFE) models of the kind:

$$Y_{g,t} = \alpha_g + \gamma_t + \beta_{fe} D_{g,t} + \epsilon_{g,t}.$$

- E.g.: employment in county g and year t regressed on county FEs, year FEs, and minimum wage in county g year t
- 26 out of the 100 most cited 2015-2019 AER papers estimate TWFE
- Also commonly used in other social sciences
- Other popular method: event-study regressions=dynamic version of TWFE

In the Simplest Set-up, TWFE = DID

- $D_{g,t}$ binary, two groups & time periods
- $Y_{g,t}$ is the outcome in location $g \in \{s, n\}$ at period $t = \{1, 2\}$
- $Y_{g,t}(0), Y_{g,t}(1)$ are the counterfactual outcomes without and with treatment
  - E.g., $Y_{g,t}(0)$ is the employment in location g at t with a low minimum wage
  - $Y_{g,t}(1)$ is the employment in location g at t with a high minimum wage
- $\beta_{fe} := Y_{s,2} - Y_{s,1} - (Y_{n,2} - Y_{n,1})$
- The before-after diff is combined with the treated-control diff

The Parallel (//) Trend Assumption

- In the absence of treatment, same average outcome evolution across groups

$$\mathbb{E}[Y_{s,2}(0) - Y_{s,1}(0)] = \mathbb{E}[Y_{n,2}(0) - Y_{n,1}(0)]$$

- Weaker than imposing that s and n have same untreated-outcome levels

$$\mathbb{E}[Y_{s,t}(0)] = \mathbb{E}[Y_{n,t}(0)] \text{ for all } t$$

- Also weaker than imposing no variation in average untreated outcomes

$$\mathbb{E}[Y_{g,2}(0)] = \mathbb{E}[Y_{g,1}(0)] \text{ for all } g$$

- Appeal of // trends: has testable implications (no pre-trends)

In General, TWFE $\neq$ DID

- Under // trends, DID is unbiased for the ATE in location s at period 2

$$\begin{aligned}
\mathbb{E}(DID) &= \mathbb{E}[Y_{s,2} - Y_{s,1} - (Y_{n,2} - Y_{n,1})] \\
&= \mathbb{E}[Y_{s,2}(1) - Y_{s,1}(0) - (Y_{n,2}(0) - Y_{n,1}(0))] \\
&= \mathbb{E}[Y_{s,2}(1) - Y_{s,2}(0)] + \mathbb{E}[Y_{s,2}(0) - Y_{s,1}(0)] - \mathbb{E}[Y_{n,2}(0) - Y_{n,1}(0)] \\
&= \mathbb{E}[Y_{s,2}(1) - Y_{s,2}(0)]
\end{aligned}$$

- Under // trends, TWFE **does not** identify the ATE parameter
- It also requires constant TE, which is often implausible
  - E.g., effect of minimum wage on employment likely differ across counties

Additive Separability of TWFE

- Static Case with a Single D:

$$D_{g,t} \in R^{+} \text{and for all } (g, t, d_1, ..., d_T), Y_{g,t}(d_1, ..., d_T) = Y_{g,t}(d_t)$$

- Parallel trends: for all $t \geq 2$, $E[Y_{g,t}(0) - Y_{g,t-1}(0)] = \gamma_t$
- It follows that: $E[Y_{g,t}(0) - Y_{g,1}(0)] = \gamma_t$, and let $\alpha_g = E[Y_{g,1}(0)]$. Then,

$$E[Y_{g,t}(0)] = E[Y_{g,1}(0)] + E[Y_{g,t}(0) - Y_{g,1}(0)] = \alpha_g + \gamma_t$$

Parameter of Interest

- Average treatment response

$$\Delta^{TR} = \frac{1}{\sum_{g,t} D_{g,t}} \sum_{g,t} \left(Y_{g,t}(D_{g,t}) - Y_{g,t}(0)\right)$$

- Then, let $\delta^{TR} = E[\Delta^{TR}]$. With a binary D, δ^{TR} =ATT
- Analogously, in (g,t):

$$\Delta_{g,t} = \frac{1}{D_{g,t}} \left[Y_{g,t}(D_{g,t}) - Y_{g,t}(0)\right] \text{ if } D_{g,t} \neq 0$$

- Then:

$$\delta^{TR} = E\left[\sum_{(g,t):D_{g,t}>0} W_{g,t}\Delta_{g,t}\right], \quad \text{with } W_{g,t} = \frac{D_{g,t}}{\sum_{(g,t):D_{g,t}>0} D_{g,t}}$$

TWFE Regression(s)

- $\widehat{\beta}_{fe}$= OLS coeff. of $D_{g,t}$ in a reg. of $Y_{g,t}$ on group FEs, time FEs and $D_{g,t}$
- We then let $\beta_{fe} = E[\widehat{\beta}_{fe}]$
- Other popular estimator: $\widehat{\beta}_{fd}$= OLS coeff. of $D_{g,t} - D_{g,t-1}$ in a regression of $Y_{g,t} - Y_{g,t-1}$ on time FEs and $D_{g,t} - D_{g,t-1}$
- We then let $\beta_{fd} = E[\widehat{\beta}_{fd}]$
- Oftentimes, we also include covariates $X_{g,t}$ in the regression. Let $\widehat{\beta}^X_{fe}$ denote the coeff. of $D_{g,t}$ in such a regression and $\beta^X_{fe} = E[\widehat{\beta}^X_{fe}]$
- We first focus on β_{fe}, but we will extend the results to β_{fd} and β^X_{fe}

β_{fe} = weighted sum of ATEs under // trends

- de Chaisemartin-D'Hautfœuille (AER, 2020) show that:

$$\beta_{fe} = E\left[\sum_{(g,t):D_{g,t}>0} W_{fe,g,t}\Delta_{g,t}\right]$$

- $W_{fe,g,t} = \frac{D_{g,t}\epsilon_{fe,g,t}}{\sum_{(g,t):D_{g,t}>0} D_{g,t}\epsilon_{fe,g,t}}$
- $\epsilon_{fe,g,t}$= residual of the reg. of $D_{g,t}$ on a constant, group FEs, and time FEs
- In general, $\beta_{fe} \neq \delta^{TR}$ because $W_{fe,g,t} \neq W_{g,t}$
- We may have $W_{fe,g,t} < 0$: if $\epsilon_{fe,g,t} < 0$ while $D_{g,t} > 0$
- Then, $\widehat{\beta}_{fe}$ does not satisfy "no-sign-reversal": $E\left[\widehat{\beta}_{fe}\right]$ may be, say, < 0 even if $Y_{g,t}(d) > Y_{g,t}(0)$ for all (g,t) and $d > 0$

What is Special about DID?

- In standard DIDs, $D_{g,t} = I_g 1\{t \geq t_0\}$ with $I_g = 1\{g \text{ belongs to treated groups}\}$

$$\begin{aligned} D_{g,t}\epsilon_{g,t} &= D_{g,t}(I_g - \overline{I})(1\{t \geq t_0\} - (1 - (t_0 - 1)/T)) \\ &= D_{g,t}(1 - \overline{I})(1 - (1 - (t_0 - 1)/T)) \end{aligned}$$

$\Rightarrow$ $W_{fe,g,t} = W_{g,t}$ and $\beta_{fe} = \delta^{TR}$

- But does not hold with missing data/unequally sized groups

Characterizing (g, t) cells weighted negatively by β_{fe}

- Let $D_{g,.}$ =average treat. rate of g and $D_{.,t}$ =average treat. rate at t
- Under // trends, $W_{fe,g,t}$ is decreasing with $D_{g,.}$ and $D_{.,t}$

  $\Rightarrow$ β_{fe} more likely to assign negative weight to periods where a large fraction of observations treated, and to groups treated for many periods

- In staggered adoption designs ($D_{g,t} \geq D_{g,t-1}$), $W_{fe,g,t} < 0$ more likely in the last periods and for groups adopting the treatment earlier

  $\Rightarrow$ We can remove negative weights by removing always treated groups and/or the last periods

Forbidden Comparison 1: $\widehat{\beta}_{fe}$ may Compare Switchers to Always Treated

- When D binary and design staggered, Goodman-Bacon (JoE, 2021) show that $\widehat{\beta}_{fe} =$ weighted avg of two types of DIDs:
  - DID_1, comparing group s switching from untreated to treated to group n untreated at both dates
  - DID_2, comparing switching group s to group a treated at both dates.
- Negative weights in β_{fe} originate from the second type of DIDs

Forbidden Comparison 1: An Example

- Example: group e treated at $t = 2$, group ℓ treated at $t = 3$. Then:

$$\widehat{\beta}_{fe} = \frac{1}{2} \times \underbrace{DID^{1-2}_{e-\ell}}_{DID_1} + \frac{1}{2} \times \underbrace{DID^{2-3}_{\ell-e}}_{DID_2}$$

- At periods 2 and 3, e's outcome = treated potential outcome, so

$$Y_{e,3} - Y_{e,2} = Y_{e,3}(1) - Y_{e,2}(1) = Y_{e,3}(0) + \Delta_{e,3} - (Y_{e,2}(0) + \Delta_{e,2}).$$

- On the other hand, group ℓ only treated at period 3, so

$$Y_{\ell,3} - Y_{\ell,2} = Y_{\ell,3}(0) + \Delta_{\ell,3} - Y_{\ell,2}(0)$$

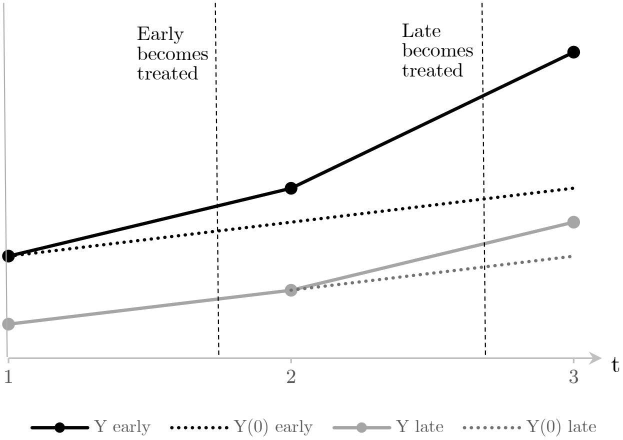
Forbidden Comparison 1: An Example (continued)

- $E\left[DID_{\ell-e}^{2-3}\right] = E\left[Y_{\ell,3} - Y_{\ell,2} - (Y_{e,3} - Y_{e,2})\right] = E\left[\Delta_{\ell,3} + \Delta_{e,2} - \Delta_{e,3}\right]$ so $\Delta_{e,3}$ enters with negative weight in β_{fe}
- Note: if $\Delta_{e,2} = \Delta_{e,3}$, $E[DID_{\ell-e}^{2-3}] = E\left[\Delta_{\ell,3}\right]$
- More generally, if $\Delta_{g,t} = \Delta_{g,t'}$, $W_{fe,g,t} \geq 0$. But restrictive!
- Note:

$$Y_{g,t}(0) - Y_{g,t-1}(0) = Y_{g,t}(1) - Y_{g,t-1}(1) \Longleftrightarrow \Delta_{g,t} = \Delta_{g,t-1}$$

- Seemingly mild assumption (trends on $Y_{g,t}(0)$ and $Y_{g,t}(1)$ are the same) is actually equivalent to time-invariant effects!

Forbidden Comparison 1: Graphical Illustration



Forbidden Comparison 2: Comparing "Switching More" to "Switching Less"

- Suppose the treatment D is not binary
- Then, $\widehat{\beta}_{fe}$ may leverage DIDs comparing group m whose D increases more to group ℓ whose D increases less
- In fact, with two groups m and ℓ and two periods,

$$\widehat{\beta}_{fe} = \frac{Y_{m,2} - Y_{m,1} - (Y_{\ell,2} - Y_{\ell,1})}{D_{m,2} - D_{m,1} - (D_{\ell,2} - D_{\ell,1})}$$

- de Chaisemartin-D'Hautfœuille (ReStud, 2018) show that this "Wald-DID" estimator may not estimate convex combination effects, even if TE constant over time

Forbidden Comparison 2: An Example

- Assume m goes from 0 to 2 units of treatment while ℓ goes from 0 to 1

$\Rightarrow$ Denominator of the Wald-DID is $2-0-(1-0)=1$

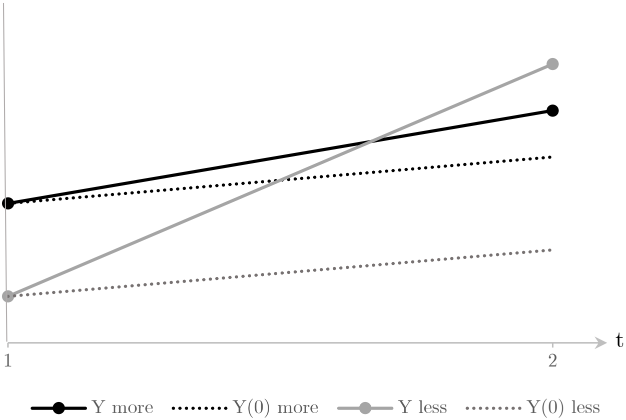
- Potential outcomes linear in treatment:

$$\begin{aligned} Y_{m,t}(d) =& Y_{m,t}(0)+\delta_m d \\ Y_{\ell,t}(d) =& Y_{m,t}(0)+\delta_\ell d, \end{aligned}$$

- Then, under // trends:

$$\begin{aligned} E\left[\widehat{\beta}_{fe}\right] =& E\left[Y_{m,2}-Y_{m,1}-(Y_{\ell,2}-Y_{\ell,1})\right] \\ =& E\left[Y_{m,2}(0)+2\delta_m-Y_{m,1}(0)-(Y_{\ell,2}(0)+\delta_\ell-Y_{\ell,1}(0))\right] \\ =& E\left[Y_{m,2}(0)-Y_{m,1}(0)\right]-E\left[Y_{\ell,2}(0)-Y_{\ell,1}(0)\right]+2\delta_m-\delta_\ell \\ =& 2\delta_m-\delta_l \end{aligned}$$

Forbidden Comparison 2: Graphical Illustration



Extensions

- dCDH (2020) extends to β_{fd}, but with different weights $W_{fd,g,t}$

$\Rightarrow$ If $\beta_{fd} \neq \beta_{fe}$, we reject homogeneous TE under // trends

- With covariates, we modify the // trends by assuming that for some λ,

$$
\begin{aligned}
&E[Y_{g,t}(0) - Y_{g,t-1}(0) - (X_{g,t} - X_{g,t-1})'\lambda | \boldsymbol{D}_g, \boldsymbol{X}_g] \\
=&E[Y_{g,t}(0) - Y_{g,t-1}(0) - (X_{g,t} - X_{g,t-1})'\lambda],
\end{aligned}
$$

  which does not depend on g.

- Let $\epsilon^X_{fe,g,t}$= residual of the reg. of $D_{g,t}$ on a constant, group FEs, time FEs and $X_{g,t}$.
- Then, same result as above but with $\epsilon^X_{fe,g,t}$ instead of $\epsilon_{fe,g,t}$ in $W_{fe,g,t}$.

Software Implementations

- `bacondecomp` Stata and R packages compute the DIDs and their corresponding weights entering in $\widehat{\beta}_{fe}$
- The `twowayfeweights` Stata and R commands compute the weights $W_{fe,g,t}$ and $W_{fd,g,t}$, possibly with covariates
  - Worst-case scenario of std dev on $\Delta_{g,t}$ where the weights are maximally correlated with TEs
  - Correlation between weights and proxies of $\Delta_{g,t}$

Example: What is the Effect of Newspapers on Electoral Turnout?

- Gentzkow et al. (AER, 2011) use US data on presidential elections
- They regress change in turnout from $t-1$ to t in county g on change in $\#$ newspapers and state-year FE
- One could also estimate the FE regression

Regression	$\widehat{\beta}$ (s.e.)	% of <0 weights	Sum of <0 weights
$\widehat{\beta}_{fe}$	-0.0011 (0.0011)	40.1%	-0.53
$\widehat{\beta}_{fd}$	0.0026 (0.0009)	45.7%	-1.43

$\Rightarrow$ Under // trends, we reject the null hypothesis that $\Delta_{g,t} = \Delta \ \forall (g,t)$

Example: Robustness measures in Gentzkow et al. (AER, 2011)

Reg.	$\widehat{\beta}$	$\underline{\widehat{\sigma}}$	$\underline{\underline{\widehat{\sigma}}}$
$\widehat{\beta}_{fe}$	-0.0011	3×10^{-4}	7×10^{-4}
$\widehat{\beta}_{fd}$	0.0026	4×10^{-4}	6×10^{-4}

- A std dev of 4×10^{-4} on $\Delta_{g,t}$ sufficient to rationalize $\delta^{TR} < 0$
- A std dev of 6×10^{-4} on $\Delta_{g,t}$ sufficient to rationalize $E[\Delta_{g,t}|\boldsymbol{D}] < 0 \ \forall (g,t)$
- Weights attached to $\widehat{\beta}_{fd}$ negatively correlated (corr=-0.06, t-stat=-3.28) with the election year

$\Rightarrow$ $\widehat{\beta}_{fd}$ biased if treatment effect changes over time

Heterogeneity-robust DID estimators

Robust DIDs

- Avoid making the forbidden comparisons leveraged by TWFE:
    1. Never compare switcher to switcher: only compare switcher to stayer
    2. Never compare a switcher to a stayer with a different baseline treatment (e.g.: group going from untreated to treated compared to always treated)

- The comparisons we use depend on whether we allow for dynamic effects
    - Is it plausible that groups' outcome at t only depends on treatment at t?

- If so, we can consider each pair of consecutive time periods independently, and compare $t-1$ to t outcome trends of:
    - $t-1$ to t switchers: groups whose treatment changes from $t-1$ to t
    - $t-1$ to t stayers: groups whose treatment does not change from $t-1$ to t, with same $t-1$ treatment as switchers

Robust DIDs

- If not, we need to control for groups' full treatment history, and compare $t-1$ to $t+\ell$ outcome trends of
  - $t-1$ to t first-time switchers: groups whose treatment changes for the first time from $t-1$ to t
  - 1 to $t+\ell$ stayers: groups whose treatment does not change from period 1 to $t+\ell$, with same $t-1$ treatment as switchers

$\Rightarrow$ Allowing for dynamic effects is appealing (not covered here), but may lead to less precise and interpretable effects, especially in complicated designs

Parameters of interest

- Suppose first that D is binary
- Let us define

$$\mathcal{S} = \{(g,t) : t \geq 2,\ D_{g,t} \neq D_{g,t-1},\ \exists g' :\ D_{g',t} = D_{g',t-1} = D_{g,t-1}\}$$

  $\mathcal{S}$= $t-1$-to-t switchers that can be matched with a $t-1$-to-t stayer with the same $t-1$ treatment

- N_S =card($\mathcal{S}$)
- Then, ATE across "matchable switchers" is

$$\delta^S = E\left[\frac{1}{N_S}\sum_{(g,t)\in\mathcal{S}} Y_{g,t}(1) - Y_{g,t}(0)\right]$$

Assumptions for identifying δ^S

- δ^S can be unbiasedly estimated under the following // trends conditions:
  1. $E[Y_{g,t}(0) - Y_{g,t-1}(0)|\boldsymbol{D}_g] = E[Y_{g,t}(0) - Y_{g,t-1}(0)] = \gamma_{0,t}$
  2. $E[Y_{g,t}(1) - Y_{g,t-1}(1)|\boldsymbol{D}_g] = E[Y_{g,t}(1) - Y_{g,t-1}(1)] = \gamma_{1,t}$
- Usual // trends on $Y_{g,t}(0)$ sufficient if we focus on "switchers in":

$$\mathcal{S}_+ = \{(g,t) : t \geq 2,\ D_{g,t} = 1 > D_{g,t-1} = 0,\ \exists g' : \ D_{g',t} = D_{g',t-1} = 0\}$$

- Weaker exogeneity assumption sufficient to consistently estimate δ^S:

$$E[Y_{g,t}(0) - Y_{g,t-1}(0)|D_{g,1}, ..., D_{g,t}] = E[Y_{g,t}(0) - Y_{g,t-1}(0)]$$

$\Rightarrow$ Allows for possibility that $Y_{g,t}(0) - Y_{g,t-1}(0)$ affects $D_{g,t+1}$ etc.

Weighted averages of DIDs identify δ^S

- For all $t \in \{1, ..., T\}$ and $d = 0, 1$, let
  - $N_{+,t} =$ card $\{g : D_{g,t} > D_{g,t-1}\}$
  - $N_{-,t} =$ card $\{g : D_{g,t} < D_{g,t-1}\}$
  - $N_{=d,t} =$ card $\{g : D_{g,t} = D_{g,t-1} = d\}$
- And let

$$DID_{+,t} = \sum_{g:D_{g,t}>D_{g,t-1}} \frac{1}{N_{+,t}} (Y_{g,t} - Y_{g,t-1}) - \sum_{g:D_{g,t}=D_{g,t-1}=0} \frac{1}{N_{=0,t}} (Y_{g,t} - Y_{g,t-1})$$

$$DID_{-,t} = \sum_{g:D_{g,t}=D_{g,t-1}=1} \frac{1}{N_{=1,t}} (Y_{g,t} - Y_{g,t-1}) - \sum_{g:D_{g,t}<D_{g,t-1}} \frac{1}{N_{-,t}} (Y_{g,t} - Y_{g,t-1})$$

- Then (dCDH, 2020)

$$E[DIDM] = E\left[\sum_{t=2}^{T} \frac{N_{+,t}}{N_S} DID_{+,t} + \frac{N_{-,t}}{N_S} DID_{-,t}\right] = \delta^S$$

Intuition for $DIDM$

- $DID_{+,t}$ compares evolution of Y between groups becoming treated between $t-1$ and t, and groups that remain untreated
- Under // trends on $Y(0)$, it identifies TE in groups switching into treatment
- Similarly, under // trends on $Y(1)$, $DID_{-,t}$ identifies TE in groups switching out of treatment
- Finally, $DIDM$ is a weighted average of those DID estimands

Placebo estimators

- Intuition: compare switchers' and stayers' outcome evolutions, one period before switchers switch
- Need to restrict attention to groups that are stayers one period before switchers switch
- We could also compare switchers' and stayers' outcome evolutions two, three periods etc. before switchers switch

Discrete Treatments

- If $D \in \mathcal{D}$, consider $DID_{d,d',t}$ $((d,d') \in \mathcal{D}^2)$, a DID comparing groups switching from d to d' from $t-1$ to t, with groups staying at d
- Then $DIDM$= weighted average of those $DID_{d,d',t}$s, scaled by switchers' average treatment change
- $DIDM$ estimates an average outcome change produced by a one unit increase of treatment

Controlling for Time-varying Covariates

- Rationale: // trends only hold if we account for covariates' change:

$$E(Y_{g,t}(d) - Y_{g,t-1}(d)|\boldsymbol{D}_g, \boldsymbol{X}_g) = \gamma_{d,t} + (X_{g,t} - X_{g,t-1})'\lambda_d \quad \forall d \in \mathcal{D}$$

  - Special case: $X_{g,t} = (1\{g = 2\} \times t, ..., 1\{g = G\} \times t)'$: group-specific linear trends

- Let $\epsilon_{g,t}(d)$ residual of the reg. of $Y_{g,t} - Y_{g,t-1}$ on period FEs and $X_{g,t} - X_{g,t-1}$ for (g, t) s.t. $D_{g,t} = D_{g,t-1} = d \in \mathcal{D}$

- Then define $DIDM^X$ as $DIDM$, but using $\epsilon_{g,t}(D_{g,t-1})$ instead of $Y_{g,t} - Y_{g,t-1}$

- Separate reg. for each $d \in \mathcal{D}$, estimated in sample of d-stayers

Controlling for Time-invariant Covariates

- With discrete time-invariant covariate, we propose estimator relying on conditional parallel trends assumption:

$$E(Y_{g,t}(d) - Y_{g,t-1}(d)|\boldsymbol{D}_g, X_g = x) = \gamma_{d,t,x}$$

- Groups with the same value of X_g experience parallel trends, but trends may differ across values of X_g
- E.g.: state-specific trends with county-level data

Software Implementation

- R and Stata command: `did_multiplegt`
- Options to relax the standard // trends
  - Control for time-varying, time-invariant covariates, or linear time trends
- Flexibly specifies the number of placebos to be estimated
- When D takes many values, with D_c coarser than D: match stayers to switchers if they share same baseline value of D_c rather than D
  - But then, $DIDM$ assumes that for $d \neq d' : f(d) = f(d')$, trend affecting $Y_{g,t}(d)$ same as that affecting $Y_{g,t}(d')$, or equivalently that $Y_{g,t}(d) - Y_{g,t}(d')$ constant over time

Example (continued): Gentzkov et al. (AER, 2011)

Table: Estimates of the effect of one additional newspaper on turnout

	Estimate	Standard error	N
$\widehat{\beta}_{fd}$	0.0026	0.0009	15,627
$\widehat{\beta}_{fe}$	-0.0011	0.0011	16,872
$DIDM$	0.0043	0.0014	16,872
$DIDM$ Placebo	-0.0009	0.0016	13,221

$\Rightarrow$ $DIDM$ is 66% larger and significantly different from $\widehat{\beta}_{fd}$ at the 10% level (t-stat=1.77) and has an opposite sign to $\widehat{\beta}_{fe}$

Extension to Continuous Treatments (de Chaisemartin et al., 2024)

- $DIDM$ compares outcome evolution of switchers and of stayers with the same baseline treatment
- Two challenges when extending this simple idea to continuous treatments:
  1. There may not be stayers
     E.g., Deschênes and Greenstone (2007) use US-county level data and TWFE regs to estimate effect of temperatures on agricultural yields.
     No stayer: no US county experiences exact same temperatures in two consecutive years
  2. Switchers cannot be matched to stayers with same baseline treatment
     E.g.: Fajgelbaum et al. (2020), impact of 2018-2019 "Trump tariffs".
     Only changed tariffs for minority of varieties, so many stayers.
     However, tariffs $\simeq$ continuous, so many varieties targeted by Trump cannot be matched to non-targeted variety with same tariffs before 2018

Notation and // Trends

- We drop the g subscript: what follows holds for any group in the sample
- Group observed at two periods (generalization to more periods easy)
- Let D_1 and D_2 denote group's treatments at periods 1 and 2
- For any $d \in \mathcal{D}_1 \cup \mathcal{D}_2$, let $Y_1(d)$ and $Y_2(d)$ denote group's potential outcomes at periods 1 and 2 with treatment d
- Let Y_1 and Y_2 denote observed outcomes
- Let $S = 1\{D_2 \neq D_1\}$ be indicator equal to 1 if the group's treatment changes from period one to two, i.e. if group is a switcher
- // trends with continuous treatment

$$\forall d_1 \in \mathcal{D}_1,\ E(Y_2(d_1) - Y_1(d_1)|D_1 = d_1, D_2) = E(Y_2(d_1) - Y_1(d_1)|D_1 = d_1)$$

Building-block Identification Result

- Under // trends,

$$
\begin{aligned}
TE(d_1, d_2|d_1, d_2) := E\left(\frac{Y_2(d_2) - Y_2(d_1)}{d_2 - d_1} \mid D_1 = d_1, D_2 = d_2\right) \\
= E\left(\frac{\Delta Y_2 - E(\Delta Y|D_1 = d_1, S = 0)}{d_2 - d_1} \mid D_1 = d_1, D_2 = d_2\right)
\end{aligned}
$$

- In a canonical DID design: $\mathcal{D}_1 = 0$ and $\mathcal{D}_2 \in \{0, 1\}$

$\Rightarrow$ $(d_1, d_2) = (0, 1)$ and so $TE(0, 1|0, 1) =$ATT

Building-block Identification Result: Proof

$$
\begin{aligned}
& E(Y_2(d_2) - Y_2(d_1) \mid D_1 = d_1, D_2 = d_2) \\
=& E(\Delta Y \mid D_1 = d_1, D_2 = d_2) - E(\Delta Y(d_1) \mid D_1 = d_1, D_2 = d_2) \\
=& E(\Delta Y \mid D_1 = d_1, D_2 = d_2) - E(\Delta Y(d_1) \mid D_1 = d_1, D_2 = d_1) \\
=& E(\Delta Y \mid D_1 = d_1, D_2 = d_2) - E(\Delta Y(d_1) \mid D_1 = d_1, S = 0) \\
=& E(\Delta Y - E(\Delta Y | D_1 = d_1, S = 0) \mid D_1 = d_1, D_2 = d_2)
\end{aligned}
$$

$\Rightarrow$ The counterfactual outcome evolution switchers would have experienced if their treatment had not changed is identified by the outcome evolution of stayers with the same period-one treatment

- E.g. If a unit's treatment changes from two to five, we can recover its counterfactual outcome evolution if its treatment had not changed, by using the average outcome evolution of all stayers with a baseline treatment of two

Target Parameter: the ASOS

- δ_1: Average Slope of Switchers: ASOS

$$\delta_1 := E\left(\frac{Y_2(D_2) - Y_2(D_1)}{D_2 - D_1}\middle| S = 1\right)$$

- Average effect across switchers of moving their D from period-one to period-two value, scaled by difference between these two values
- Local effect
  - Applies to switchers
  - Measures effect of moving their treatment from its period-one to period-two value, not of other manipulations of their treatment
- But ASOS can be used to identify (resp. bound) effect of other treatment changes if potential outcomes linear (resp. concave/convex)

Support Condition for ASOS Identification

- Standard support condition for matching estimators: no value of the period-one treatment such that only switchers have this value

$$0 < P(S = 1), \text{ and almost surely, } P(S = 1|D_1) < 1$$

- Implies $P(S = 0) > 0$: while we assume D_1 and D_2 continuous, we also assume that treatment persistent

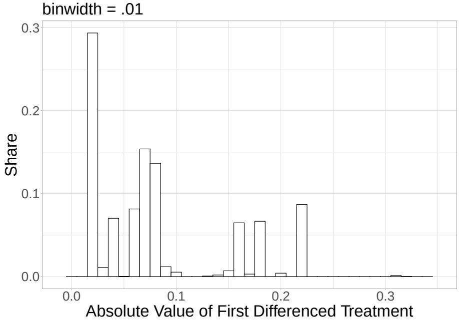
$\Rightarrow$ $D_2 - D_1$ has a mixed distribution with mass point at zero

No Quasi-stayers

- Switchers' treatment changes by at least c in absolute value

$$\exists c > 0 : P(|D_2 - D_1| > c|S = 1) = 1$$

⇒ Holds in Fajgelbaum et al. (2020): tariffs increases decided by Trump administration ≥ 1.5pp:



ASOS Identification w/o Quasi-stayers

- Switchers' treatment effects identified by comparing their outcome evolution to that of stayers with same period-one treatment

$$\delta_1 = E\left(\frac{Y_2 - Y_1 - E(Y_2 - Y_1|D_1, S=0)}{D_2 - D_1}\middle| S = 1\right)$$

ASOS estimation w/o quasi-stayers

- With iid sample $(Y_{g,1}, Y_{g,2}, D_{g,1}, D_{g,2})_{1\leq g\leq G}$, $E\left(\frac{\Delta Y - E(\Delta Y|D_1,S=0)}{\Delta D}\middle|S=1\right)$ can be estimated in three steps:

  1. Estimate non-parametric regression of ΔY_g on $D_{g,1}$ among stayers
  2. Compute $\widehat{E}(\Delta Y|D_{g,1}, S=0)$, predicted outcome evolution given baseline treatment according to non-parametric regression, for all switchers
  3. Finally,

$$\widehat{\delta}_1 := \frac{1}{G_s}\sum_{g:|\Delta D_g|>0}\frac{\Delta Y_g - \widehat{E}(\Delta Y|D_{g,1}, S=0)}{\Delta D_g}.$$

  4. One can show that $\widehat{\delta}_1$ is $\sqrt{G}-$ consistent, and $\sqrt{G}(\widehat{\delta}_1 - \delta_1)$ converges towards normal distribution whose variance can be consistently estimated

Bobba, Flabbi and Levy (IER, 2022)

Labor Market Search, Informality, and Schooling Investments

- An equilibrium search model where:
  1. Search frictions generate mobility between formal and informal jobs
  2. Match-specific productivity and bargaining generate overlapping wage distributions
  3. Both ingredients generates a mix of formal and informal jobs in equilibrium
- One important long-term "cost of informality": Under-investment in education
  - Same features that create informality may also distort returns to schooling
  - Hold-up ex-ante investments in human capital

Labor Markets in Latin America

- More than half of the labor force is in the informal sector
    - Workers not contributing to and not covered by the social security system
    - Informal employees and (most of the) self-employed
- Patterns in the data are not consistent with either a segmented or a competitive view of the labor market
    1. Individuals transit back and forth between formal and informal jobs
    2. Wage/productivity distributions overlap
    3. Mix of formality status within the same firm
- Informal workers have started to gain access to non-contributory social security programs

The Costs of Informality

- Informality may well be an optimal choice to a given institutional setting
  - De facto flexibility for firms and workers to cope with adverse shocks
- Still, its pervasive diffusion may generate short- and long-term costs:
  - Hinders fiscal capacity and the provision of social insurance
  - Subsidy for smaller and often less productive firms
  - Worsen hold-up problems in investment decisions of firms and workers

The Model Environment

- Timing
  1. Schooling decision
  2. Searching status decision
  3. Labor market dynamics
- Labor Market States
  1. Unemployed
  2. Self-employed
  3. Informal Employee
  4. Formal Employee

Schooling Decision

- Irrevocable decision about schooling level h
- Discrete choice: $h \in \{0, 1\}$
- Individual-specific heterogeneity
  - costs $\kappa \sim T(\kappa)$
  - opportunity cost - PDV of participating in LMK as $h = 0$
- Schooling decision has reservation value rule: only agents with $\kappa < \kappa^\star(y)$ will acquire $h = 1$
- All labor market parameters are allowed to be schooling-specific

Searching-status Decision

- Irrevocable decision about searching as:
  - Self-employed
  - Unemployed
- Individuals search for an employee job in both states but receive offers at different rates: $\gamma_h < \lambda_h$
- Self-employment income $y \sim R(y|h)$
- Searching status decision has (again) reservation rule property: only agents with $y \geq y^\star(h)$ search for an employee job while also working as self-employed

Labor Market Dynamics

State	PDV	Shock	Flow Utility
Workers:			
Unemployed	$U(h)$	λ_h	$\xi_h + \beta_{0,h} B_0$
Self-Employed	$S(y,h)$	γ_h	$y + \beta_{0,h} B_0$
Informal Employee	$E_0[w,y,h]$	η_h, χ_h	$w_0(x;y,h) + \beta_{0,h} B_0$
Formal Employee	$E_1[w,y,h]$	η_h, χ_h	$w_1(x;y,h) + \beta_{1,h} B_1[w_1(x;y,h)]$
Firms:			
Vacancy	$V[h]$	ζ_h	ν_h
Filled Informal Job	$F_0[x,y,h]$	η_h, χ_h	$x - w_0(x;y,h)$
Filled Formal Job	$F_1[x,y,h]$	η_h, χ_h	$x - (1+t) w_1(x;y,h)$

- Match-specific productivity: $x \sim G(x|h)$
- One-shot penalty for firms hiring illegally: $c_h w_0(x;y,h)$
- Matching function determines $\{\lambda_h, \gamma_h, \zeta_h\}$: $m_h = (u_h + \psi_h s_h)^{\iota_h} (v_h)^{1-\iota_h}$

Labor Market Institutions and Wage Determination

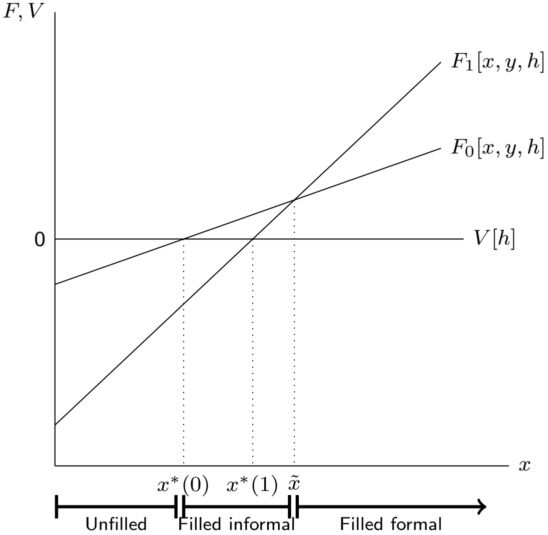
- Non-wage workers' flow value:
  - formal employee $= \beta_{1,h} B_1[w_1(x;y,h)] = \beta_{1,h}[\tau t w_1(x;y,h) + b_1]$
  - informal employee $= \beta_{0,h} B_0$
  - Notice: the endogenous b_1 introduces redistribution within and between schooling levels.

- Wage are determined by bargaining, conditioning on formality status endogenously posted by firms. Wage schedules (under free-entry of firms) are:

$$
\begin{aligned}
w_0(x;y,h) &= \frac{\alpha_h}{1+\chi_h c_h} x + (1-\alpha_h)[\rho Q(y,h) - \beta_{0,h} B_0] \\
w_1(x;y,h) &= \frac{\alpha_h}{1+t} x + \frac{(1-\alpha_h)}{1+\beta_{1,h}\tau t}[\rho Q(y,h) - \beta_{1,h} b_1]
\end{aligned}
$$

where: $Q(y,h) \equiv \max\{S(y,h), U(h)\}$

Equilibrium Representation



Empirical Implications

- Main stylized facts of informal labor markets are replicated in equilibrium:
  1. A mixture of formal and informal jobs is realized
  2. Formal employees have on average higher wages than informal employees. But their accepted wage distributions overlap
  3. Informal employees and self-employed have markedly different labor market dynamics
  4. Some firms hire formal or informal workers at different points in time just as workers transit over time between different formality status

Data Sources

1. Mexico's Labor Force Survey (ENOE): Year 2005
   - Nonagricultural, full-time, male, private-sector, secondary-school workers between the ages of 25 and 55 who reside in urban areas
   - $w \equiv$ Hourly wages as employee, main job after labor contributions
   - $y \equiv$ Hourly labor income as self-employed, without paid employees
   - $f = 1$ if employee is contributing to the social-security fund; $= 0$ otherwise
   - $h = 1$ if Upper secondary completed $= 0$ if Lower secondary completed
2. Aggregate labor shares for Mexico in 2005
   - Total compensations per employee as percentage of GDP
3. Vacancy rates for 2005
   - Good coverage of vacancy posting in urban areas
   - Detailed information on the schooling level required for the job

Identification: Informality Parameters

- Different transition rates out of formal jobs and informal jobs identify χ_h
- The identification of β_1 and c_h is derived from the location and extent of the *overlap* between formal and informal accepted wage distributions
  - While movement in β_1 and c_h can achieve the same extent of the overlap, they do so by moving its location in different directions
  - This generates different shape in the accepted wage distribution of formal and informal employees
- Repeating the same argument over the y distribution generates useful variation to separately identify the parameters

Identification: Informality Parameters (cont'd)

- The identification of β_0 requires the use of additional information
  - We exploit the roll-out of the *Seguro Popular* (SP) program in 2005-2006
  - In terms of our model, it can be seen as an exogenous increase in B_0
- Variation in B_0 across individuals identify β_0
  - As long as this variation is not correlated with changes in the primitive parameters of the model
  - Labor market outcomes pre-policy seem balanced

SP Roll-out and Labor Market Characteristics in 2001

	Hourly Wages (log)			Labor Market Proportions			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Formal	Informal	Self	Formal	Informal	Self	Unempl.
SP in 2005 (1=yes)	-0.041	0.048	-0.035	-0.034	0.035	-0.004	0.003
	(0.036)	(0.055)	(0.062)	(0.026)	(0.019)	(0.014)	(0.006)
Complete Sec. (1=yes)	0.218	0.288	0.092	0.061	-0.036	-0.029	0.003
	(0.017)	(0.032)	(0.033)	(0.011)	(0.008)	(0.008)	(0.003)
Observations	7865	5474	2777	16458	16458	16458	16458

NOTE: OLS estimates. Standard errors clustered at the municipality level are reported in parenthesis. Data is drawn from the Mexican labor market survey (ENE, 2001) and matched at the municipality-level with the roll-out of the *Seguro Popular* program. State dummies and municipality-level controls (log(population), log(population^2), and poverty index) are included in all specifications.

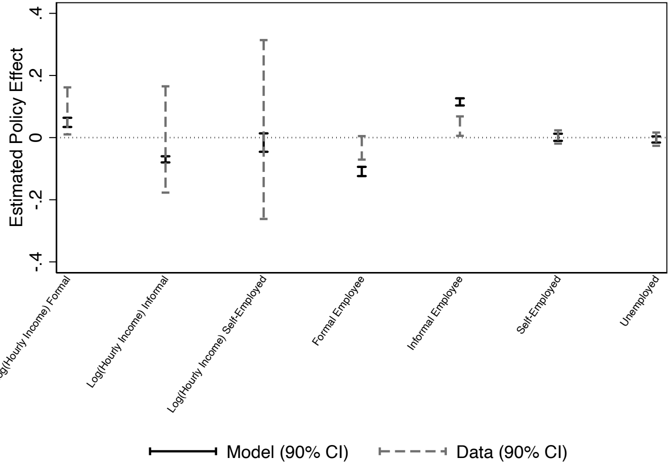
Estimation

- For each schooling and treatment group, we match the following moments
  - Proportions of individuals in each labor market state
  - Accepted wage distributions of formal and informal employees
    - Mean and SD: overall and by quintiles
    - Overlap, as measured by proportion of formal employees for each quintile of the informal accepted wage distribution
  - Accepted earnings distributions of self-employed
    - Mean and SD
  - Transitions between LMK states (yearly)
  - Hazard rates out of unemployment (at 3 and 6 months)
  - Labor Shares
- Back-out demand-side parameters using vacancy rates

Returns to Schooling

Ability:	Low	High
	$k=1$	$k=2$
PDV of Labor Market Search:		
$\int_y Q(y,h)dR(y\|h)$	0.309	0.278
Average Accepted Wages:		
F: $E_h\left[w_1 \mid \tilde{x}(y,h) \leq x\right]$	0.479	0.435
I: $E_h\left[w_0 \mid x_0^*(y,h) \leq x < \tilde{x}(y,h)\right]$	0.281	0.296
Average Offered Wages:		
F: $E_h\left[w_1 \mid y < y^*(h)\right]$	0.213	0.210
F: $E_h\left[w_1 \mid y \geq y^*(h)\right]$	0.213	0.204
I: $E_h\left[w_0 \mid y < y^*(h)\right]$	0.133	0.134
I: $E_h\left[w_0 \mid y \geq y^*(h)\right]$	0.142	0.136

Out-of-Sample Model Validation

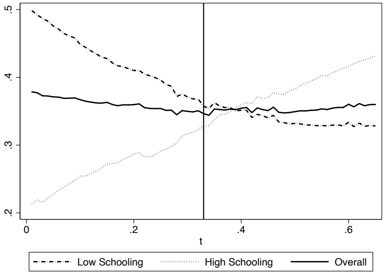


Counterfactual: The Equilibrium Effects of Informality

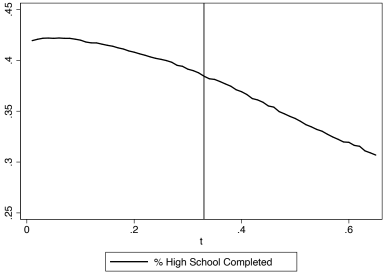
Model:	Firms can only offer a formal contract			
Specifications:	Baseline Model	Exogenous Schooling	Exogenous Contact Rates	Hosios-like Condition ($\alpha = \iota$)
Flow Welfare:				
Total	-0.0596	-0.0750	-0.0020	0.0478
Workers	-0.0460	-0.0599	0.0166	0.0570
Firms	-0.2821	-0.3219	-0.3055	-0.1589
Labor Market Proportions:				
Unemployed	0.0213	0.0636	0.0019	-0.0459
Self-employed	0.3353	0.3526	0.3625	0.2329
Formal Employees	0.0275	-0.0146	-0.0376	0.0076
Schooling Outcomes:				
% HS Completed	0.1029	–	0.0781	0.1501
% High Ability in HS	0.0538	–	0.0569	0.0628

NOTE: Relative changes wrt the benchmark model. Hosios increases α from 0.56 to 0.73.

Counterfactual: Changes in Payroll Tax Rate (t)



(a) Informality



(b) Schooling

Main Take-ways from Estimated Model

1. Returns to schooling are substantial
2. Informality is welfare improving but:
   - Significantly more so for firms than workers
   - Reduces human capital accumulation (hold-up problem)
3. Payroll tax rate has a non-intuitive impact on equilibrium outcomes
   - Informality rate not a good indicator for policy
   - Redistributive components within the formal system are often ignored in the policy debate

Job Search Models and Diff-in-Diffs

- Relevant institutional features are included in the model in a tractable way
- But these extra parameters are hard to separately identify using standard labor market survey data
- The staggered roll-out of the welfare policy provides additional variation outside the model environment
  1. Identify the (average) valuation of non-contributory benefits
  2. Validate the model on a different time period by simulating one-step ahead