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Difference in Differences: foundations and new methodology

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Difference-in-Difference

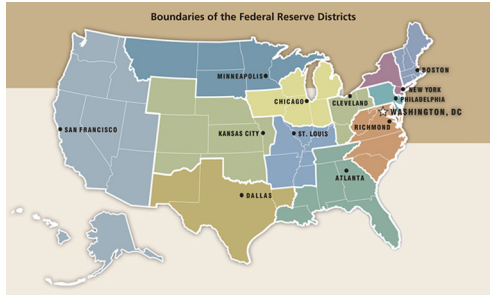
- DD (DiD) (Diff-Diff) is one of the most widely applied methods for policy analysis
  - Intuitive and applicable in many-setting.
- Let's start with an example

Example: Bank Run

- During the period right before the great-depression, in TN a a bank run occurred
- A bank run happens when lots of people massively withdraw their assets from the bank. Leaving the banks with no liquidity (cash). Which basically means they can't operate and the banks collapsed.
- Years ago, during the great recession, we went through something similar of having several banks "collapsing".
  - The main government agency that can respond to bank actions is the federal reserve bank.
  - They have two options on how to respond to situations like this:
    - Option A: let the banks deal with it
    - Option B Give the banks some cash so they don't collapse.

One Mississippi, Two Mississippi

- There are 12 federal reserve banks.
- Each of them manage banks from a particular geographical area (called districts).



One Mississippi, Two Mississippi

- The state of Mississippi, is covered by the 6th and the 8th district.
  - The 8th district fell under the jurisdiction of the St. Louis Fed
  - The 6th district fell under the jurisdiction of the Atlanta Fed.
- During the 1930s, a bank-run started in Nashville, TN in the bank of Nashville and quickly spread out the neighboring areas.

One Mississippi, Two Mississippi

- Each Fed-Director had to make a choice about what policy-environment they would follow: No intervention or some.
- The director of the St. Louis Fed believed in not-intervening (i.e. let the banks fail).
- The director of the Atlanta Fed believed in intervening (i.e. we should help this banks out).

Monetary Policy in action

- When the bank run was happening both Feds reacted very differently.
  - One gave cash to the banks (ATL Fed) and other one didn't do anything (St. Louis).
- In this set-up, we have one state, composed by two "districts". One district will receive a treatment, the other will not.
- Q: What is the advantage of this, versus comparing all banks in the 6th district vs all banks in the 8th district?
  - Banks location could be a reflection of economic condition.
  - Different states, etc.

Monetary Policy in action

- One could argue that for the case of Mississippi there is some "randomness" in the fact that some banks are treated and some banks will not.
  - It's hard to construct a story about a bank locating somewhere because they thought they would receive a treatment.
- However, we can come up with stories about banks locating in south/north region of the Mississippi, because of things like rent, projects, employment levels.

Bank run: Two paths

- About 1931 (sept?) bank-runs started to happen along MS and other states.
- Within 4 weeks of the collapse:
  - The ATL Fed *increased* lending by about 40% (did something) while the St. Louis Fed *decreased* its lending by 3% (did nothing)
- What happened to the banks on each district? Which district fare better? the ones from the ATL or STL?

A naive approach

- One way to approach to figure out to compare the # of banks that remain after the bank run
  - In the 6th district (ATL), there were 121 banks left after the bank run
  - In the 8th district (STL), there were 132 banks left after the bank run
  - What's the conclusion from this comparison?
- "After the collapse, the 8th district that 11 more banks than the 6th district"
- "After the collapse, the 8th district had 9 percent more banks than the 6th district"
- What's the issue with this comparison?

A better approach

- We can do better by comparing the *change in the number of banks* rather than the "post" number.
  - i.e. How many banks where lost, not how many remained
- In ATL, in the pre-period (1930) they were 135 banks and in the post period (1931) they were 121 banks. So ATL lost 14 banks.
- What's the issue with this comparison?
- Can we do better?

Pre-Post Analysis

	1930 (Pre)	1931 (Post)	Diff
8th District STL	165	132	-33
6th District ATL	135	121	-14

- Specifically ATL appears to have saved 19 banks (more than 10% of those operating in Mississippi (19/135))

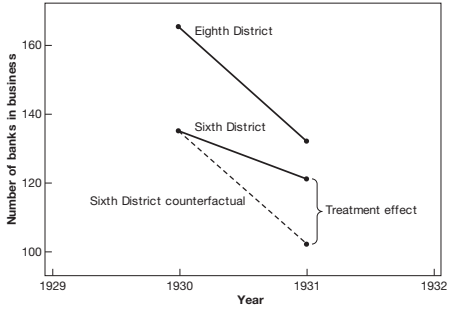
DD - Bank run

	1930 (Pre)	1931 (Post)	Diff
8th District STL	165	132	-33
6th District ATL	135	121	-14
Diff	-30	-11	19

- The Difference estimates suggest that lending to troubled banks kept many of them opened
- 19 is what we call the "Difference-in-Difference Estimate".

DD - Counterfactual

FIGURE 5.1
Bank failures in the Sixth and Eighth Federal Reserve Districts



Notes: This figure shows the number of banks in operation in Mississippi in the Sixth and Eighth Federal Reserve Districts in 1930 and 1931. The dashed line depicts the counterfactual evolution of the number of banks in the Sixth District if the same number of banks had failed in that district in this period as did in the Eighth.

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- We are using the 8th district (no treatment) as the counterfactual of what would have happened to the 6th district had they not implemented any policy

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Difference-in-Difference

- Esta es una versión simplificada de cómo aplicar la diferencia en diferencia.
  - Buscamos instancias donde tenemos una unidad "tratada" y una unidad "control".
  - Luego comparamos sus resultados antes y después de la intervención.
- Si esto es el estimador DD, ese estimado, es el efecto causal?
  - Lo sera, si es que se cumplen algunos supuestos. Comenzemos a pensar en lo que esta haciendo el DD.

Difference-in-Difference: Formalizacion

- El componente de tiempo captará los "cambios a lo largo del tiempo" que podrían afectar a todos.
  - Que pasaria si solo comparamos antes y después en el grupo de tratamiento?
  - Que pasari si solo comparamos el grupo de control y tratamiento, pero solo despues del tratamiento?
- Por lo tanto, lo que el grupo de control está permitiendo hacer es "controlar" todas las demás cosas que podrían contribuir lo que está sucediendo en esos pasar de el tiempo.
- Eso quiere decir que escoger la comparacion y escoger bien el grupo de control es una de los componentes vitales de DD

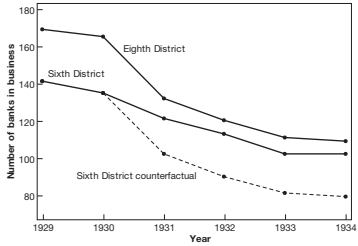
Como elegimos a un grupo de control

- Pero ¿Como sabemos que estamos escogiendo a un buen grupo de control?
  - La teoria (el conocimiento que trae el investigador al contexto)
  - Ejercicios empiricos:
    - Si las tendendias del grupo de control y tratamiento han sido paralelas por un tiempo antes de el tratamiento, esto nos proporciana evidencia que, si el tratamiento no hubiera pasaso, hubieran seguido las tendendencias pasadas.
    - Esta es la idea del "Parallel Trends"

Supuestos

- Esta logica, nos brinda la intuicion de el primer supuesto principal de DiD

FIGURE 5.3
Trends in bank failures in the Sixth and Eighth Federal Reserve Districts, and the Sixth District's DD counterfactual



Notes: This figure adds DD counterfactual outcomes to the banking data plotted in Figure 5.2. The dashed line depicts the counterfactual evolution of the number of banks in the Sixth District if the same number of banks had failed in that district after 1930 as did in the Eighth.

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- Que "El grupo de tratamiento hubiera evoluciando como el grupo de control, si es que el grupo de tratamiento no hubiera recibido el tratamiento."

Supuestos

- Este supuesto - que en si es sencillo - tiene muchas implicaciones.
- Estas implicaciones se han ido entendiendo mejor al pasar de los años.
- Por ejemplo este supuesto implica que:
  - No hay otras reformas al mismo tiempo en el grupo de control.
  - No haya un efecto anticipatorio de la reforma
- En si, para ser mas claros y no caer en juegos semanticos, se empezara a mencionar los supuestos y sus implicaciones por separado.

Supuestos

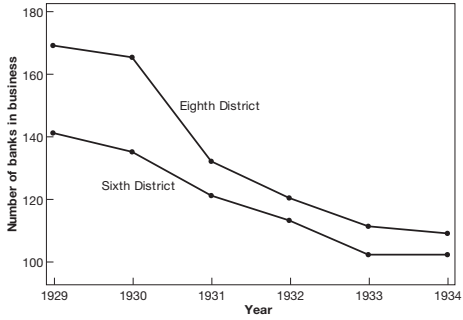
- Nótese que este supuesto no impone que el tratamiento y el control sean "exactamente iguales".
  - No importaba que los dos distritos tuvieran diferentes puntos de partida,
  - No solo eso, podrian tener diferentes características observables y no observables.
- La clave es entender si están siguiendo patrones y tendencias similares.
- Esto es algo muy poderoso del DiD y una de las razones por las que se ha hecho bien popular.

Parallel Trends Assumption

- Y como sabemos que el supuesto es valido?
  - No se puede "testear", solo dar evidencia a favor o en contra
  - La idea de parallel trends es demostrar como los grupos "track" el uno con el otro antes de la politica.
  - En si, aunque los grupos sean diferentes, si se estan tracking en caracteristicas observables, tambien podrian estar tracking en caracteristicas no observables.

DD- Parallel Trends

FIGURE 5.2
Trends in bank failures in the Sixth and Eighth Federal Reserve Districts

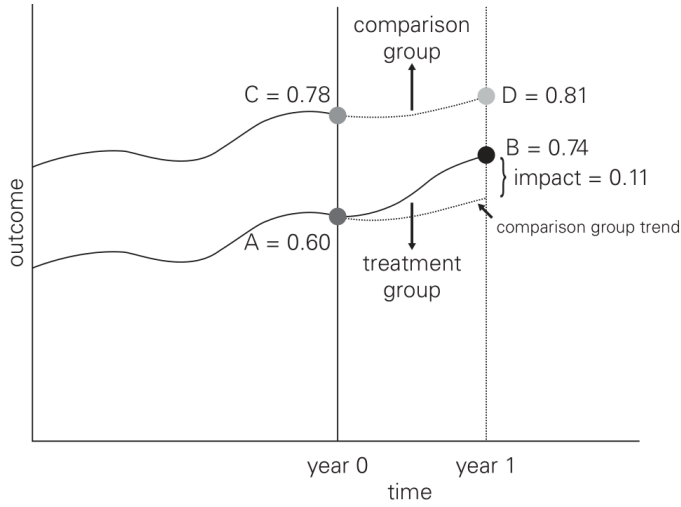


Note: This figure shows the number of banks in operation in Mississippi in the Sixth and Eighth Federal Reserve Districts between 1929 and 1934.

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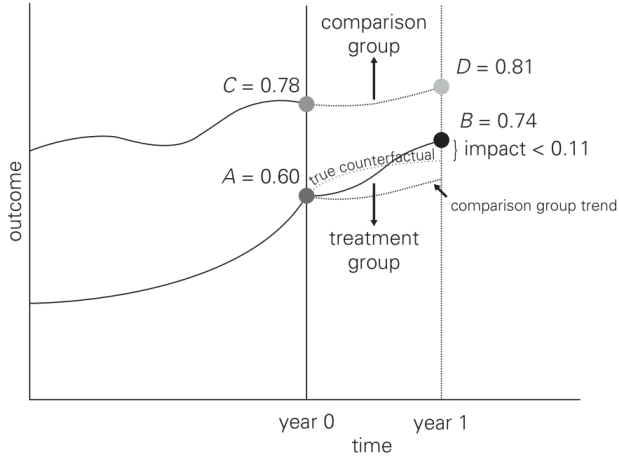
- Una implicación de las tendencias paralelas es que cuantos más datos tengamos antes de la reforma, mejor porque podemos ver cómo evolucionan durante largos períodos de tiempo.

Parallel Trends assumptions



Source: Authors.

Parallel Trends assumption



Source: Authors.

Formalizacion usando Potential Outcomes Framework

- **$Y_{i,t}(0,0)$ es el outcome de la unidad i en el periodo t, si la unidad i no hubiera recibio tratamiento (0) en t=1, ni en t=2.**
- **$Y_{i,t}(0,1)$ es el outcome de la unidad i en el periodo t, si la unidad i no recibio tratamiento (0) en t=1, y no en t=2. (1)**
- $Y_{i,t}(1,0)$ es el outcome de la unidad i en el periodo t, si la unidad i recibio tratamiento (1) en t=1, y no en t=2 (0)
- $Y_{i,t}(1,1)$ es el outcome de la unidad i en el periodo t, si la unidad i si recibio tratamiento (1) en t=1, y si en t=2. (1)

Notacion

- En si el problema de causalidad nos dice que no podemos observar dos potential outcomes al mismo tiempo. Solo uno:
- Es decir solo podemos observar o $Y_{i,t}(0,0)$ o $Y_{i,t}(0,1)$.
- El otro termino importante de la notacion es $D_i = 1$ y $D_i = 0$
- Eso simplemente indica en que grupo pertenece i, un grupo de tratamiento o grupo de control.
- $D_i = 1, \{i = 1,2,3\}$, $D_i = 0, \{i = 4,5,6\}$, mutually exclusive groups.

Notacion

- Que es $Y_{i,t}(0,0) \mid D_i = 0$? Se puede observar?
- Que es $Y_{i,t}(0,0) \mid D_i = 1$? Se puede observar?
- Que es $Y_{i,t}(0,1) \mid D_i = 0$? Se puede observar?
- Que es $Y_{i,t}(0,1) \mid D_i = 1$? Se puede observar?

El parametro causal

- En si quisieramos saber:

$$\tau_2 | D_i = 1 = \tau = \mathbb{E}[Y_{i,2}(0,1) - Y_{i,2}(0,0) \mid D_i = 1] \quad (1)$$

- Tambien estariamos interesados en

$$\tau_2 | D_i = 0 = \tau = \mathbb{E}[Y_{i,2}(0,1) - Y_{i,2}(0,0) \mid D_i = 0] \quad (2)$$

- *Pero nos enfocamos en el primero*, porque queremos saber que paso el grupo que recibio el tratamiento, esto se le llama el Average Treatement Effect on the Treated (ATT) (in period t=1).
- A.0 Un primer supuesto aca es que la unidad i, solo tiene un potential outcome, y no depende de el tratamiento de otros (SUTVA - Stable Unit Treatment Value Assumption)

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Lo que observamos

- En si quisieramos saber:

$$\tau_2 | D_i = 1 = \tau = \mathbb{E}[Y_{i,2}(0,1) - Y_{i,2}(0,0) \mid D_i = 1] \tag{3}$$

- Lo que observamos es:

$$\mathbb{E}[Y_{i,2}(0,1) - Y_{i,1}(0,1) | D_i = 1] - \mathbb{E}[Y_{i,2}(0,0) - Y_{i,1}(0,0) | D_i = 0] \tag{4}$$

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Lo que observamos

$$\mathbb{E}[Y_{i,2}(0,1) - Y_{i,1}(0,1)|D_i = 1] - \mathbb{E}[Y_{i,2}(0,0) - Y_{i,1}(0,0)|D_i = 0] + E[Y_{i,2}(0,0) - Y_{i,2}(0,0)|D_i = 1]$$

$$\mathbb{E}[Y_{i,2}(0,1) - Y_{i,2}(0,0) \mid D_i = 1] + \mathbb{E}[Y_{i,2}(0,0) - Y_{i,1}(0,1)|D_i = 1] - \mathbb{E}[Y_{i,2}(0,0) - Y_{i,1}(0,0)|D_i = 0]$$

$$A.1 \quad \mathbb{E}(Y_{i,1}(0,0)) = \mathbb{E}(Y_{i,1}(0,1)) \ \forall \ D_i = 1 \quad (Anticipation)$$

$$\tau_2 + \mathbb{E}[Y_{1,2}(0,0) - Y_{i,1}(0,1)|D_i = 1] - \mathbb{E}[Y_{i,2}(0,0) - Y_{i,1}(0,0)|D_i = 0]$$

Lo que observamos

$$\tau_2 + \mathbb{E}[Y_{1,2}(0,0) - Y_{i,1}(0,1)|D_i = 1] - \mathbb{E}[Y_{i,2}(0,0) - Y_{i,1}(0,0)|D_i = 0]$$

$$A.2 \quad E[Y_{1,2}(0,0) - Y_{i,1}(0,1)|D_i = 1] = \mathbb{E}[Y_{i,2}(0,0) - Y_{i,1}(0,0)|D_i = 0] \quad (PT)$$

$$\tau_2$$

Para obtener el τ_2, de una comparacion que podemos hacer, necesitamos supuesto A.0, A.1 y A.2.

Supuestos

- A.0 SUTVA, presente en todos los research design incluidos RCTs. Construyendo el grupo de control ayuda a mantener este supuesto.
- A.1 No Anticipacion, en si es algo que se podra observar con la data (habria un efecto antes de la intervencion).
- A.2 Parallel Trends, este es el supuesto mas dificil de evaluar, y por ende se vuelve el mas importante.
  - Cualquier cosa que puede violar a este supuesto se esta asumiendo, por ejemplo tratamientos al grupo de control"
  - Queremos un tiempo largo despues de el tratamiento o corto?

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Aplicando DiD

- Una vez que se obtienen estos supuestos, uno puede obtener el parametro τ simplement obteniendo:
  - La diferencia entre pre and post para el grupo de T y C group (Diff 1)
  - La diferencia de las dos Diff1s (Diff 2).

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DD in a regression format

- Si bien se puede aplicar DiD con tan solo una comparacion de promedios, tambien podriamos utilizar OLS. Cuales son las ventajas?
  - Standard Errors
  - Complicaciones
- Como se estima una DiD simple con OLS?

Ejercicio: Linear Reg and DD

- Configuracion principal

$$Y_{dt} = \alpha + \beta\, Treat_d + \gamma Post_t + \delta_{DD}(Treat_d \times Post_t) + \varepsilon_{dt}$$

- In the bank run example:

	1930 (Pre)	1931 (Post)	Diff
8th District STL	165	132	-33
6th District ATL	135	121	-14
Diff	-30	-11	19

- Worksheet #1

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Practice Question

For each of the four panels below, indicate the values of β, γ, δ

$$Y_{dt} = \alpha + \beta\, Treat_d + \gamma Post_t + \delta_{DD}(Treat_d \times Post_t) + \varepsilon_{dt}$$

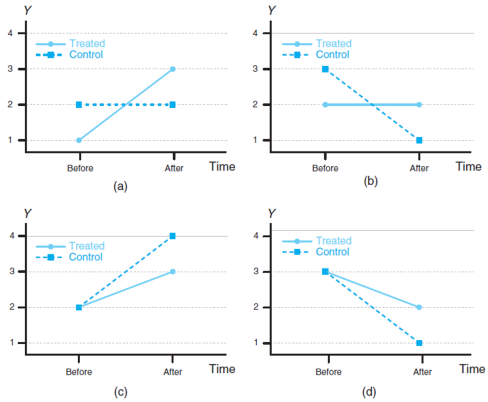


FIGURE 8.6: More Difference-in-Difference Examples (for Review Question)

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Como se codifica en STATA?

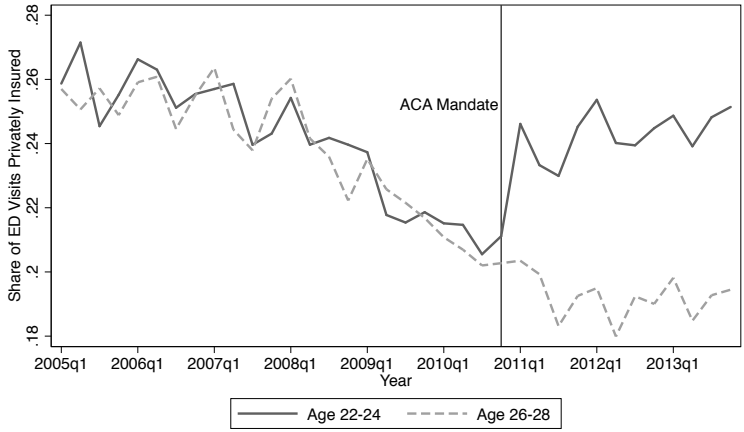
- `reg outcome treat post treat_X_post`
- `reg outcome treat post i.treat#i.post`
- `reg outcome i.treat##i.post`

Event Study

- Como damos evidencia al supuesto de parallel trends?
- Podemos ver un grafico, y evaluar las tendencias pasadas.

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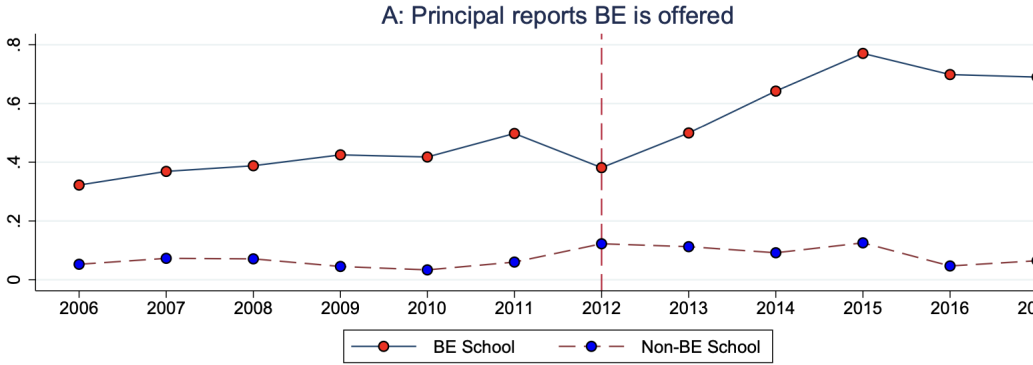
Ejemplos



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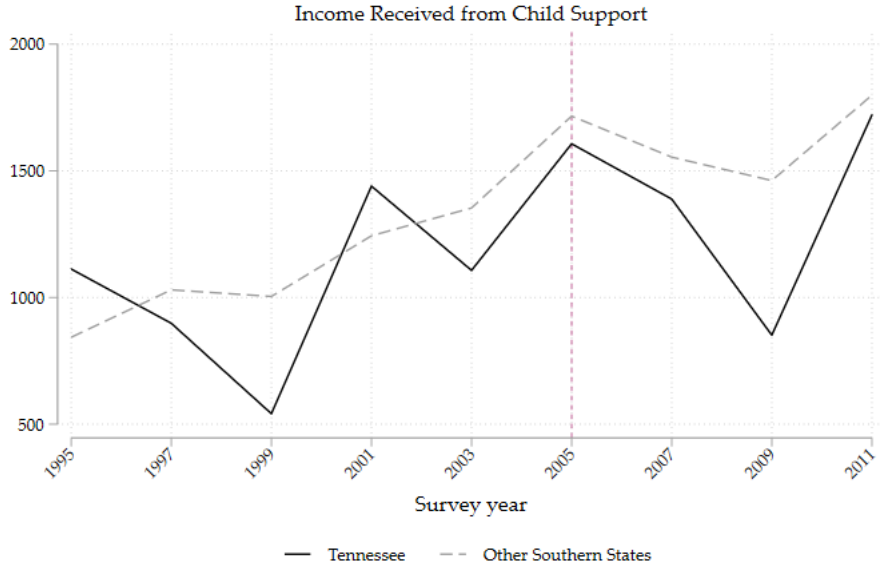
Ejemplos

Figure 2: Bilingual education inputs



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Ejemplos



Event Study

- No solo queremos "confiar" en nuestros ojos. Por eso queremos evaluarlo a traves de OLS.
- Haciendo esta evaluacion con OLS, es un "event study"
- Los Event studies son una forma de realizar una prueba empírica de las tendencias paralelas. Comencemos con el marco principal:

$$Y_{dt} = \alpha + \beta\, Treat_d + \gamma Post_t + \delta_{DD}(\boldsymbol{Treat_d \times Post_t}) + X_{dt} + \varepsilon_{dt}$$

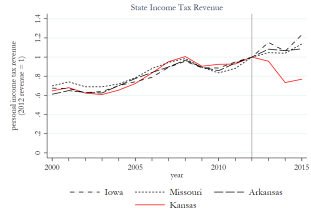
- El cambio principal es que cambiamos la interacción, para que sea una interacción con cada año (o medida de tiempo) de los datos:

$$Y_{dt} = \alpha + \beta\, Treat_d + \gamma Post_t + X_{dt} + \delta_1\, Treat \times Year_1 + \delta_2\, Treat \times Year_2 \ldots + \delta_T\, Treat \times Year_T + \varepsilon_{dt}$$

$$Y_{dt} = \alpha_0 + \beta\, Treat_d + \gamma Post_t + X_{dt} + \sum_{i=1}^{t} \delta_i\, Treat \times Year_i + \varepsilon_{dt}$$

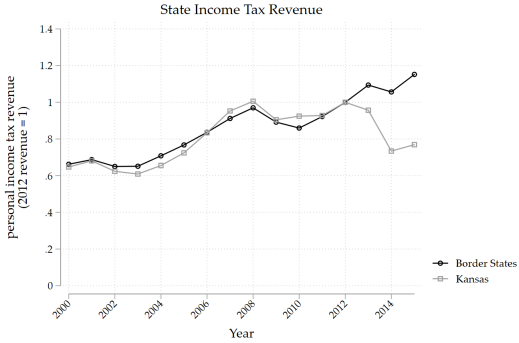
Event Study

- Hacer ejercicio
- Pregunta: Que estan capturando estas dummies?
- Estan capturando la diferencia de pendientes entre el tratamiento y el grupo de control en un punto del tiempo, relativo a un la diferencia en un tiempo base.
- Veamos como funciona esto con un ejemplo



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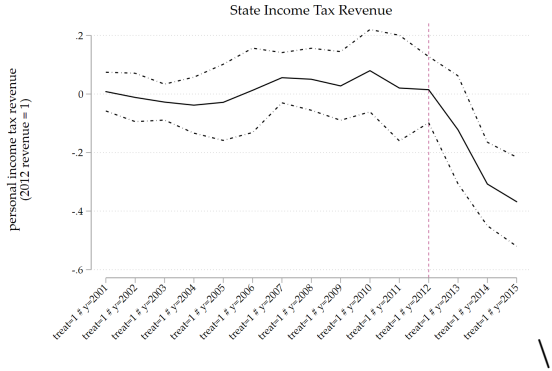
Event Study



- Crear un event study, con 2012 como año base

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Event Study



En Stata

- `fvset base 2011 year`
- `reg outcome treat post i.treat#i.year, cl(fips)`

treat#year						
1 2000	-.0204675	.0548879	-0.37	0.734	-.1951453	.1542103
1 2001	-.0121733	.0629509	-0.19	0.859	-.2125113	.1881646
1 2002	-.0320695	.0534688	-0.60	0.591	-.2022312	.1380921
1 2003	-.0480236	.0519496	-0.92	0.423	-.2133503	.1173031
1 2004	-.0585066	.0352607	-1.66	0.196	-.170722	.0537087
1 2005	-.0488789	.0413734	-1.18	0.323	-.1805475	.0827897
1 2006	-.0077361	.06085	-0.13	0.907	-.2013878	.1859156
1 2007	.0353768	.0489586	0.72	0.522	-.1204315	.1911851
1 2008	.0300801	.03773	0.80	0.484	-.0899935	.1501537
1 2009	.007196	.0281234	0.26	0.815	-.0823052	.0966973
1 2010	.0593568	.0122813	4.83	0.017	.0202721	.0984414
1 2012	-.0056218	.0272642	-0.21	0.850	-.0923886	.0811449
1 2013	-.1427703	.0229417	-6.22	0.008	-.2157809	-.0697596
1 2014	-.3280348	.0176562	-18.58	0.000	-.3842248	-.2718448
1 2015	-.3885654	.054395	-7.14	0.006	-.5616746	-.2154561
_cons	.9223457	.0272642	33.83	0.000	.835579	1.009113

Event study

- En si, los event study son -ahora- una manera estandar de presentar los resultados de tu DiD.
- Los event-study tienen las siguientes ventajas:
  - Muestran una evaluacion de PT
  - Muestran el efecto (visualmente), se puede entender el economic significance (magnitud).
  - Muestran como el efecto evoluciona a traves de el tiempo (el parametro δ es un promedio de estos puntos)
  - Muestra visualmente los confidence intervals (statistical significance).
- En la practica, solo corro ES, y solo presento eso. Las tablas, son para hablar de el efecto, y en el apendice.

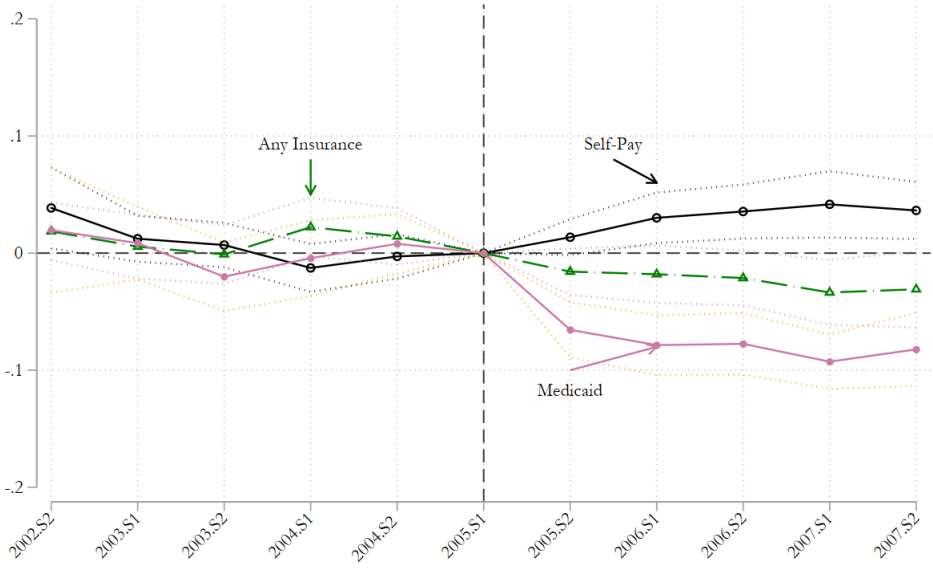
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Como Graficar un Event Study?

- Worksheet #2

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Ejemplos



Generalizacion DD

- Un último punto sobre la ecuación DD es que podemos hacer que el modelo sea "más flexible" usando efectos fijos más flexibles

$$Y_{dt} = \alpha + \beta\, Treat_d + \gamma Post_t + \delta_{DD}(\boldsymbol{Treat_d} \times \boldsymbol{Post_t}) + X_{dt} + \varepsilon_{dt}$$

- Cambiaremos las variables Post y Treat por efectos fijos de grupo (puede ser cualquier cosa) y efectos fijos de tiempo.

Cambiando *Post*

$$Y_{dt} = \alpha + \beta\, Treat_d + \gamma \boldsymbol{Post_t} + \delta_{DD}(Treat_d \times Post_t) + X_{dt} + \varepsilon_{dt}$$

- El "Post" representó el cambio en el grupo de control después del período de tiempo en que se implementó la política. El espíritu de esto era controlar otras cosas que están sucediendo en el período posterior que están afectando al grupo de tratamiento y control.
  - Podemos ser más flexibles capturando eventos que pueden estar ocurriendo en un período de tiempo específico que están afectando a todos los grupos de tratamiento y control de la misma manera
    - Time fixed effects: e.g. Year Fixed Effects

$$Y_{dt} = \alpha + \beta\, Treat_d + YearFE_t + \delta_{DD}(Treat_d \times Post_t) + X_{dt} + \varepsilon_{dt}$$

Cambiando *Treat*

- Que es lo que "treat" estaba capturando?

$$Y_{dt} = \alpha + \beta\, Treat_d + YearFE_t + \delta_{DD}(Treat_d \times Post_t) + X_{dt} + \varepsilon_{dt}$$

- Lo que estaba capturando "treat" es la diferencia de "nivel" inherente entre los grupos de tratamiento y control.
- Podemos ser más flexibles al tener un nivel de referencia para cada grupo: efectos fijos grupales: group fixed-effects
  - Por Ejemplo: State fixed effects

$$Y_{dt} = \alpha + StateFE_d + YearFE_t + \delta_{DD}(Treat_d \times Post_t) + X_{dt} + \varepsilon_{dt}$$

$$Y_{dt} = \alpha + \delta_{DD}(Treat_d \times Post_t) + StateFE_d + YearFE_t + X_{dt} + \varepsilon_{dt}$$

  - Tenga en cuenta que no solo tiene que ser estado, puede ser FE de "unidades", donde "unidades" representa unidades que reciben el tratamiento.

Event study

- Finalmente nuestra regresion se podra escribir asi

$$Y_{dt} = \alpha + \delta_{DD}(Treat_d \times Post_t) + GroupFE_d + TimeFE_t + X_{dt} + \varepsilon_{dt}$$

- El event study ser vera asi:

$$Y_{dt} = \alpha_0 + GroupFE + TimeFE + X_{dt} + \sum_{i=1}^{t} \delta_i Treat \times Time_i + \varepsilon_{dt}$$

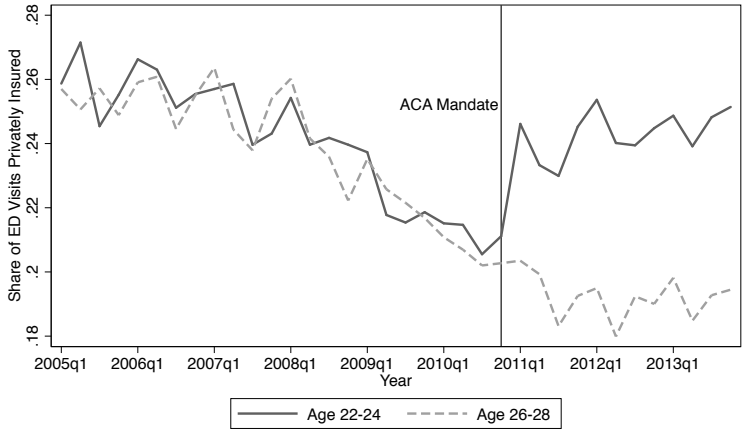
- Notese que aun estamos comparando unidades que fueron tratadas en el mismo tiempo. Aun no hemos "variado" en el tiempo.
- Por ende, todos los supuestos originales, son los necesarios para estimar DiD con TWFE. (Two way fixed effects).

Como escoger un grupo de control?

- Respuesta general:
  - Busque formas inteligentes de construir un grupo de control.
- Comparaciones regionales:
  - Estados fronterizos, regiones censales
- Comparaciones demográficas:
  - Grupos de edad, ser padre, etc.
- Por lo general, desea confiar en más de una unidad de control. Queremos construir un promedio de otras unidades.

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Ejemplo: Grupos de Edad



Preguntas

- ¿Qué más estamos capturando?
  - ¿Hay otras políticas cambiando al mismo tiempo en el grupo de tratamiento o control? (Difícil cuando el análisis incluye varios estados)
  - ¿Vemos tendencias previas que conduzcan a la política? (Alguna evidencia de anticipacion)
- Prueba de placebos:
  - (Placebo Outcome). ¿La reforma estara afectando esto a un resultado que no debería verse afectado?
  - (Placebo Treatment group) Ejecutar el DiD en una muestra en la que no debería haber un efecto.
- Randomization inference
  - Esto puede ser usado como una robustez y una manera de hacer inferencia

California Paid Leave

- The United States is the only advanced industrialized country without a national law providing new mothers (and often fathers) with entitlements to paid family leave (PFL).
- However, three states have implemented paid leave programs, the first of these being California, where PFL took effect in 2004.
- Let's say we want to learn from this policy, we could study what happened in California after the implementation of this policy.
- What outcomes would be interesting to study?

California Paid Leave

- A number of states extend the FMLA by providing rights to unpaid leave to additional employees or for longer periods of time. (Depends on employer size and work-history).
- None of them (back then) offered paid leave.
- California's first in the nation explicit PFL program, took effect in July 2004.
- California PFL offers six weeks of partially paid leave to bond with a newborn or a recently placed foster or adoptive child, or for other reasons (such as to care for seriously ill relatives).
  - Wage replacement is 55 percent up to a ceiling based on the state's average weekly wage.

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California Paid Leave

- What's the naive comparison?
- How would you set it up in a DD framework?

$$Y_{dt} = \alpha + \beta\, Treat_d + \gamma Post_t + \delta_{DD}(Treat_d \times Post_t) + \varepsilon_{dt}$$

- Possible comparison groups:
  - Mothers vs. Non-mothers in CA, before and after 2004. Any mothers?
  - Mothers in CA vs. other states, before and after 2004.

Following Rossin-Slater, Ruhm, Waldfogel

The Effects of California's Paid Family Leave Program on Mothers' Leave-Taking and Subsequent Labor Market Outcomes

Maya Rossin-Slater
Christopher J. Ruhm
Jane Waldfogel

Rossin-Slater, Ruhm, Waldfogel

APPROACH

We employ a DD design comparing changes in the outcomes for eligible California mothers with infants ($N = 2{,}482$), surveyed before and after the implementation of PFL, to corresponding differences for comparison groups unlikely to be affected by the law.[15] To illustrate, consider specifications of the form:

$$Y_{it} = \beta_0 + \beta_1 \times TREAT_i + \beta_2 \times POST_t \times TREAT_i + \gamma' X_{it} + \delta_t + \varepsilon_{it} \quad (1)$$

for individual i surveyed in year t. Y_{it} is the outcome of interest (e.g., use of maternity leave). $TREAT_i$ is a dummy variable set to 1 for California mothers with children less than 1 year old at the survey date. $POST_t$ is an indicator equal to 1 if the individual was surveyed in 2005 or later, and 0 otherwise.[16] X_{it} is the vector of individual characteristics. δ_t is a vector of general year effects and ε_{it} is an individual-specific error term. The key coefficient, $\hat{\beta}_2$, measures the DD estimate of the effect of PFL on the treatment group.[17]

Comparison

- The have four comparison groups:
  - Main comparison group: Women in California with a youngest child aged 5 to 17 years at the survey date (N = 18,593).
  - Alternative 1: Women in California with no children (N = 33,790)
  - Alternative 2: Mothers with infants residing in the next three largest states—Florida, New York, and Texas—(N = 4,000)
  - Alternative 3: Mothers with infants residing in all states other than California (N = 28,605).
- *"Although each of these comparisons is informative, we focus on specifications using comparison group 1 because the labor market behavior of California mothers with older children is likely to be more similar to that of mothers of infants than those of women with no children (comparison group 2)."*

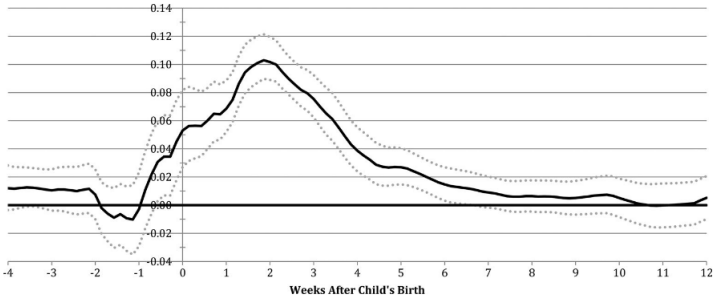
Results

	Maternity Leave	Family Leave	Other Leave	Any Leave
Treat x Post (PFL)	0.0357***	0.0362***	−0.0048	0.0308**
	(0.0074)	(0.0070)	(0.0055)	(0.0092)
	On Leave	With job and at work	Not employed	
Treat x Post (PFL)	0.0548**	−0.0373	−0.0217	
	(0.0151)	(0.0326)	(0.0291)	

Pre-program maternity leave-taking for the treatment group averages 5.4 percent.

When?

Figure 3. Marginal Effects of PFL on Leave for Mothers with 95 Percent Confidence Intervals.



Notes: The figure shows DD estimates (coefficients on CA × POST interactions). The weekly marginal effects (the bold line) are seven-day averages centered on the starting day of the indicated week; weekly standard errors are averaged over the same seven-day period. The 95 percent confidence intervals (the light lines) are calculated by adding and subtracting 2.01 times the averaged standard error to and from the averaged marginal effect.

Figure 4. Marginal Effects of PFL on Leave for Fathers with 95 Percent Confidence Intervals.

Results

- *Our study of California's first in the nation PFL program provides evidence that the overall use of maternity leave increased by an average of three weeks after the program was enacted.*
- *The point estimates also suggest that the growth may have been especially large for black, non-college educated, unmarried, and Hispanic mothers. These groups used only an average of around one to two weeks of leave prior to the enactment of PFL, compared to between three and five weeks for their advantaged counterparts. After rights to paid leave were provided, however, high school educated, unmarried, Hispanic, and black mothers were predicted to take four, five, five, and six weeks of leave, respectively, approaching the six to seven weeks estimated for their college-educated, married, or non-Hispanic white peers.*

Assumption?

- What is the identifying assumption?
- The key identification assumption is that changes in (but not levels of) the outcomes would have been the same for the treatment and comparison groups in the absence of PFL
- What are things that could break the assumption? Let's go over some examples:
  - An event in California in 2010?
  - An event in the control group before the treatment?
  - A federal change in leave? (Like a new mandate?)

Examples

- What happens to voter turnout when you move to another place with higher voter turnout?
- What does subsidizing legal counsel in poor communities do?
- Does reducing taxes on small business actually pay for itself?

Legal Counsel

CHANGES IN FAMILY STRUCTURE AND WELFARE PARTICIPATION SINCE THE 1960S: THE ROLE OF LEGAL SERVICES*

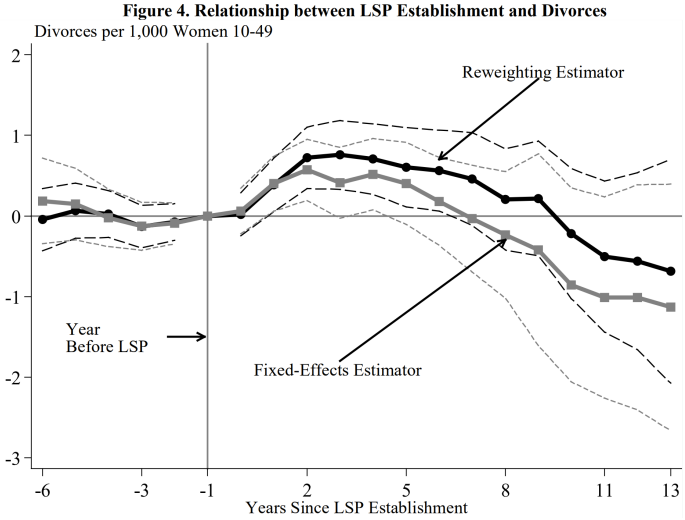
Jamein Cunningham
Andrew Goodman-Bacon

July 15, 2020

Abstract

This paper evaluates the effects of the War on Poverty's Legal Services Program (LSP) on family structure and welfare participation. LSPs provided subsidized legal assistance to poor communities, focusing on divorce and welfare access. We use a difference-in-differences research design based on the rollout of the program to 251 counties from 1965 to 1975. We find temporary increases in divorce and persistent increases in welfare participation and nonmarital birth rates. Nonmarital births rose because marriage rates fell, not because birth rates rose. Expanded access to legal institutions thus contributed, directly and indirectly, to changes in family structure in the 1960s.

Legal Counsel



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Example: Voting

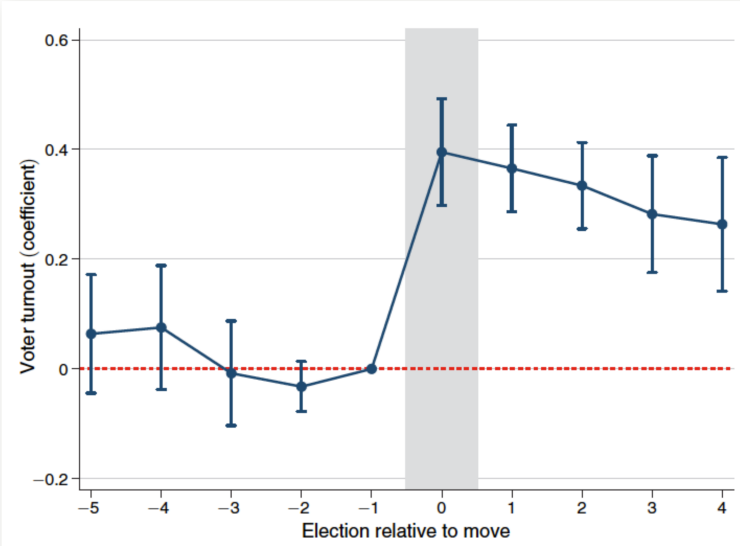
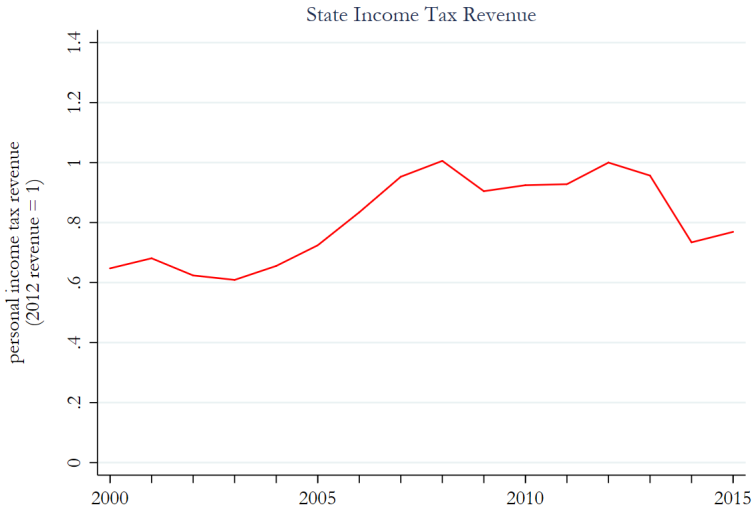


Figure 3 from Cantoni and Pons (2022)

Summary Stats: Kansas no-tax/less tax on small business

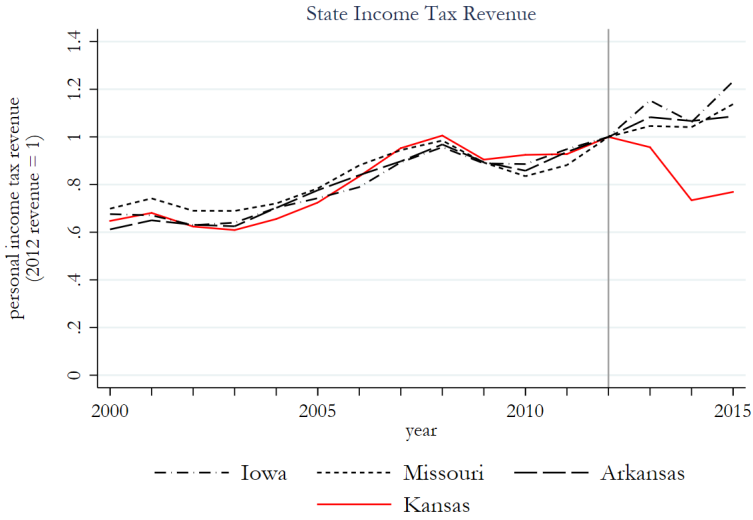


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Kansas Reform on 2012



Simple DD


State Income Tax Revenue
personal income tax revenue (2012 revenue = 1)
0
.2
.4
.6
.8
1
1.2
1.4
2000
2005
2010
2015
year
Iowa
Missouri
Arkansas
Kansas

Example

Effective Policy for Reducing Inequality? The Earned Income Tax Credit and the Distribution of Income
Hilary W. Hoynes and Ankur J. Patel
NBER Working Paper No. 21340
July 2015
JEL No. H2,I38,J2

ABSTRACT

In this paper, we examine the effect of the EITC on the employment and income of single mothers with children. We provide the first comprehensive estimates of this central safety net policy on the full distribution of after-tax and transfer income. We use a quasi-experiment approach, using variation in generosity due to policy expansions across tax years and family sizes. Our results show that a policy-induced $1000 increase in the EITC leads to a 7.3 percentage point increase in employment and a 9.4 percentage point reduction in the share of families with after-tax and transfer income below 100% poverty. Event study estimates show no evidence of differential pre-trends, providing strong evidence in support of our research design. We find that the income increasing effects of the EITC are concentrated between 75% and 150% of income-to-poverty with little effect at the lowest income levels (50% poverty and below) and at levels of 250% of poverty and higher. By capturing the indirect effects of the credit on earnings, our results show that static calculations of the anti-poverty effects of the EITC (such as those released based on the Supplemental Poverty Measure, Short 2014) may be underestimated by as much as 50 percent.