ECON 8000/9000 Empirical Energy Econ

Topic 03: Review of Panel Data Regressions: FE Regressions + DID

Christy Zhou

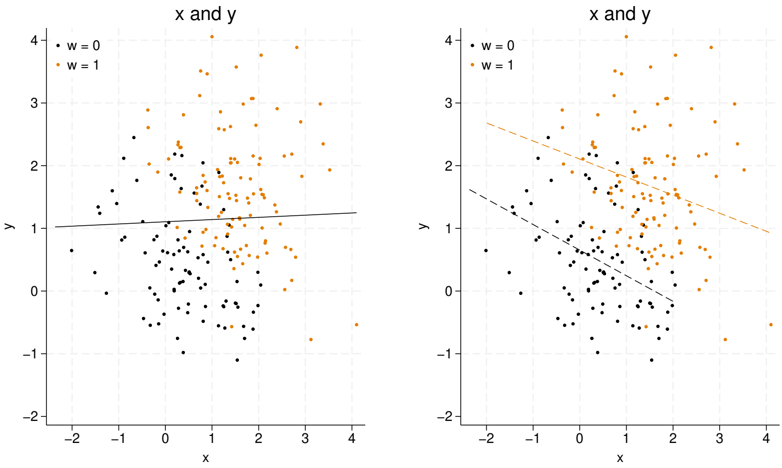
January 21, 2026

Outline

- Review of Panel Data Regressions: FEs + DID
- TWFE Example 1 Scott (2022)
- TWFE Example 2 Severen & van Benthem (2022)
- LP-DID by Dube et al. (2025) + An Example Fabra et al. (2024)

1. Review the Functionality of Adding Controls

Partialing-out by adding w_i: Consider $y_i = \beta_0 + \beta_1 x_i + \gamma w_i + \varepsilon_i$



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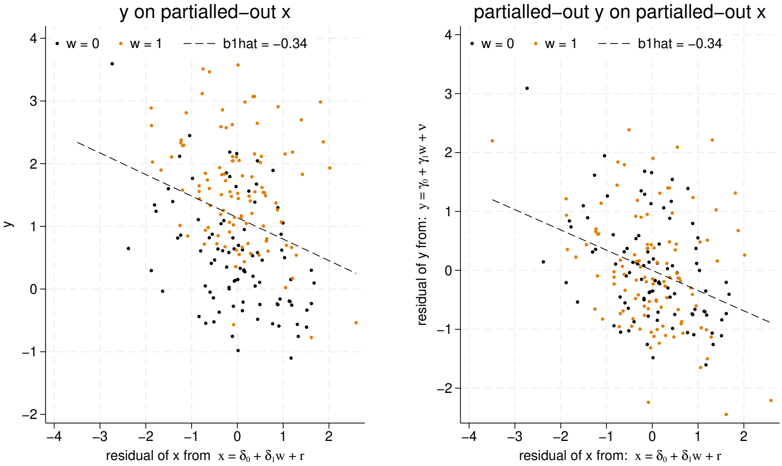
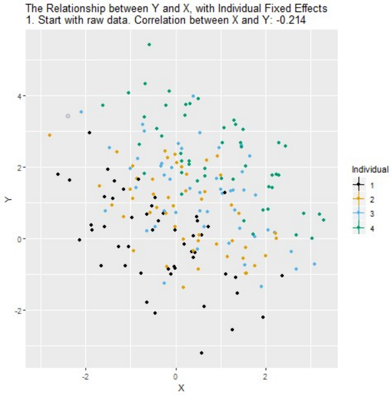


2. Review the Functionality of FE

Extract Within-Effect for Both Cross-Sectional & Panel Data Settings



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- $y_i = \beta_0 + \beta_1 x_i + \phi_i + \varepsilon_i$
- ϕ_i: A group FE such as income group, racial group, etc
- Partialling-out with an FE works like demeaning
- Effectively demeaning x_i for groups ϕ_i
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- Aka how variation in x_i within a group affect y_i

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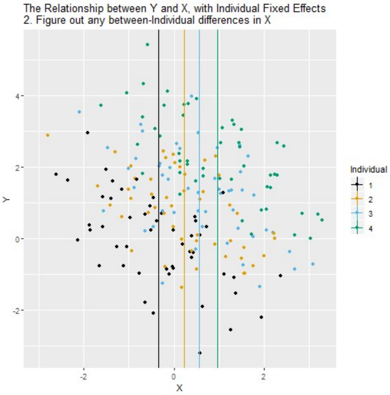

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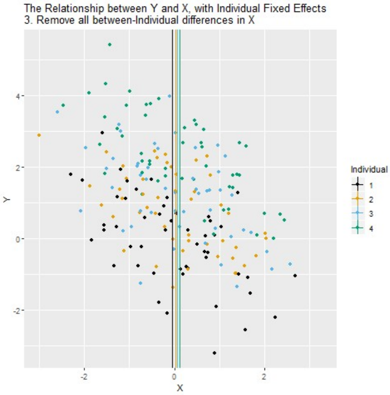

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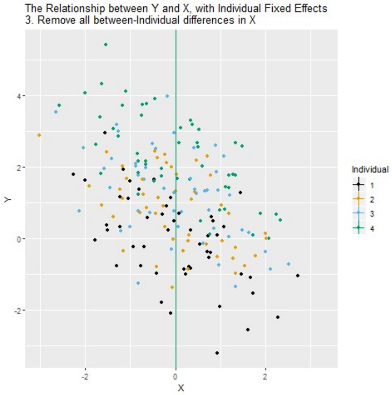

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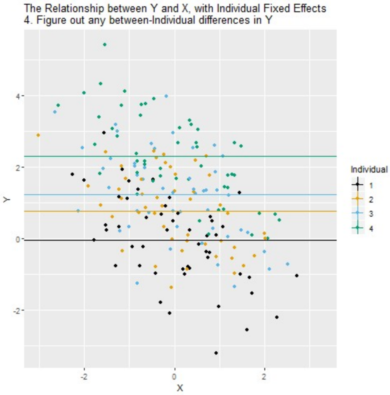

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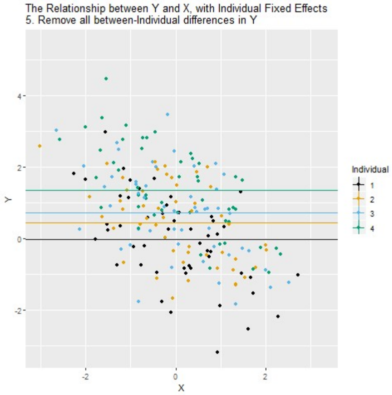

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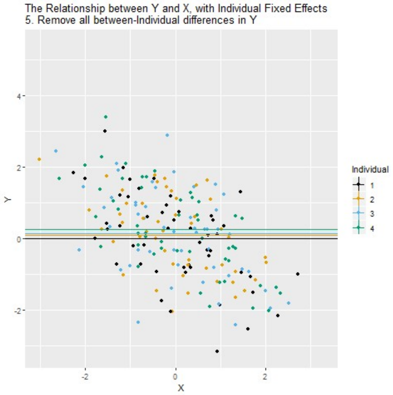

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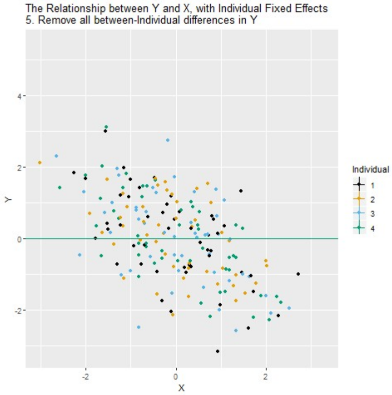

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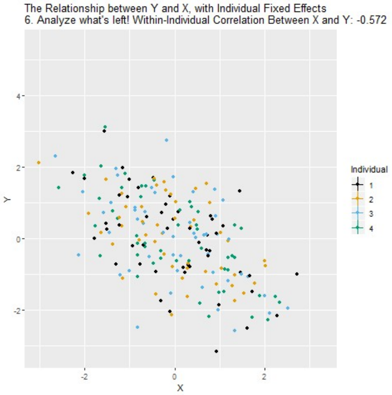

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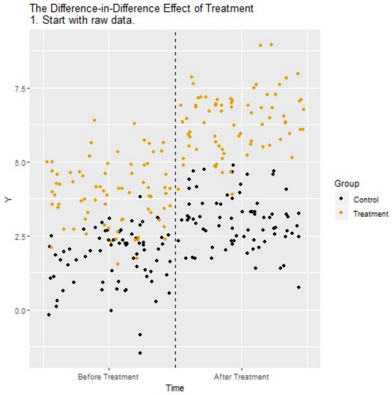
Use DID Single Treatment to Demo for Panel Data FE Regressions



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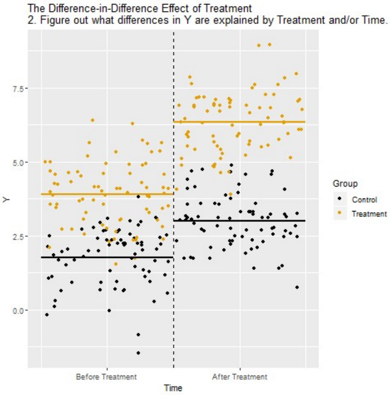

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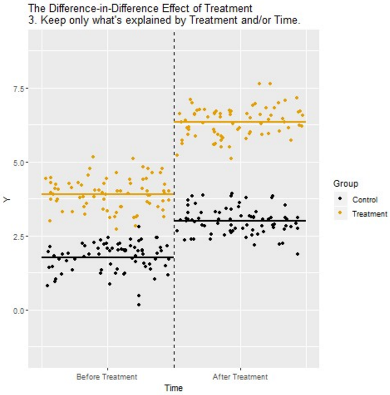

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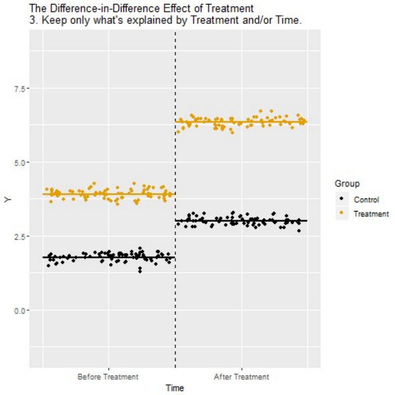

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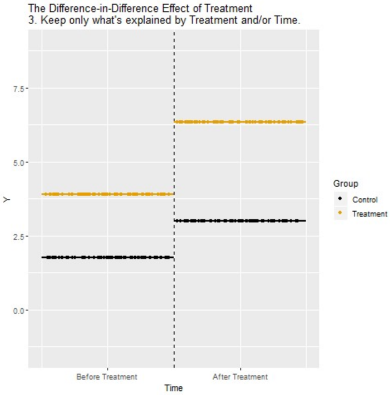

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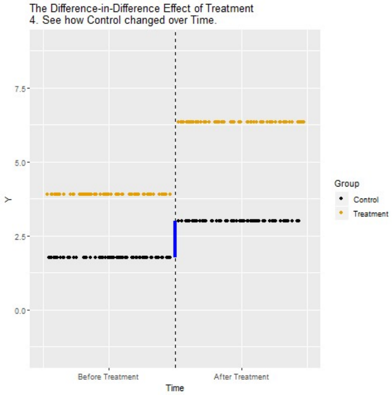

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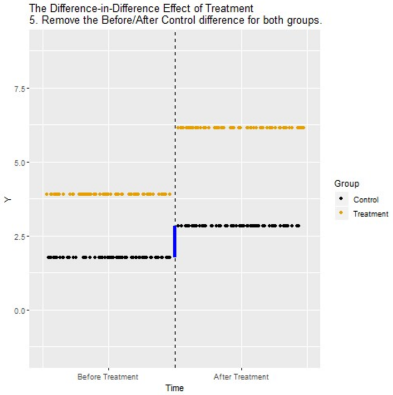

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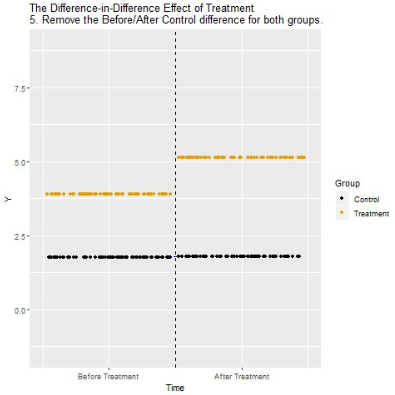

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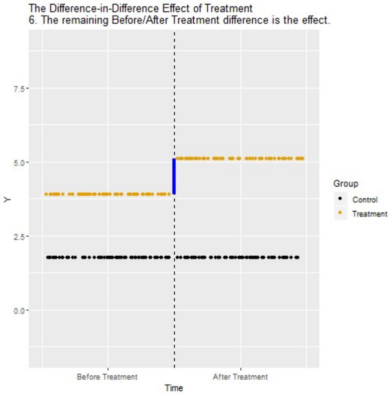

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Panel Data Regression with FEs

Typical Estimation

Typically, we would estimate

$$Y_{it} = \beta X_{it} + \gamma \mathbf{Z}_{it} + \phi_i + \phi_t + \varepsilon_{it} \tag{1}$$

- Key var of interest: X_{it} can be a dummy or a continuous variable
- Here we have observables $\mathbf{Z}_{it}$ and FEs ϕs
- To know what β picks up, it is important to know how X_{it} varies (i) across is, (ii) across ts, and (iii) across is over time t
- Reality check: X_{it} may not be exogenous, and you may need to look for IV

Expansion:

- Stacked FD: $\Delta Y_{it} = \beta \Delta X_{it} + \gamma \mathbf{Z}_{it} + \phi_t + \varepsilon_{it}$
- Long(er) FD: $\Delta Y_{i,t-1,t+h} = \beta \Delta X_{it} + \gamma \mathbf{Z}_{it} + \phi_t + \varepsilon_{it}$

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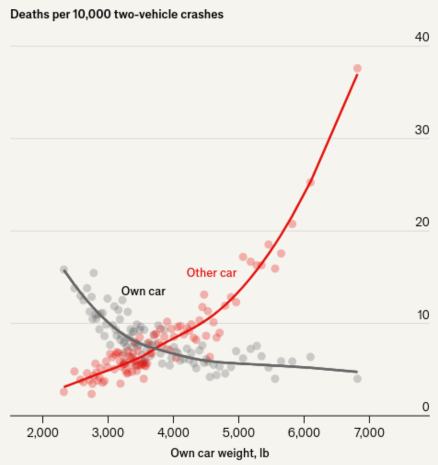
Scott (2022) J of Law & Econ

"Preference for Safety/Larger Vehicles"



Source: Economist "Too Much of a Good Thing" Link

Scott (2022) J of Law & Econ

"Preference for Safety/Larger Vehicles"

Scott (2022) studies how having a next-door neighbor who experienced a fatal car crash affects consumer choice of vehicle weight.

- Main yvar: Vehicle weight
- Main xvar: Fatal motor vehicle accident of next-door neighbor (Quasi-random!)
- Why do consumers desire larger vehicles?

  ⇀ Private benefits: (i) preference for safety, (ii) sport-feature, etc.
- Why is it important?
  - Potential external costs and source of inefficiency: (i) safety consequences on other on-road vehicles, pedestrians, and public property, (ii) emissions from larger vehicles, (iii) intensifying pre-distorted undervaluation of fuel efficiency, (iv) social spillover effect, etc.
  - If there are external costs, then, there is a cost not priced to the consumer
- Why is the parameter that the author is after difficult to estimate in other contexts?

Scott (2022) J of Law & Econ

Q1. Why is this Q difficult to answer?

Entertain two typical routes to answer

- 1. Find a survey dataset that asks consumers' stated preferences for vehicle safety
  - Then regress the weight on such stated preference

Scott (2022) J of Law & Econ

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- 2. Use transactional data, regress weight on safety ratings?
  - Same flaws as above: Measurement issue + confounders
- 3. Use product level data, run a demand estimation such as
  $\ln s_{jt} - \ln s_{j0} = \beta_1 price_{jt} + \beta_2 weight_{jt} + \text{other controls} + \text{FEs} + \eta_{jt}$
  - Then $\hat{\beta}_2/\hat{\beta}_1$ implies WTP for weight
  - Flaw:
    - a. Implication: WTP for weight $\neq$ WTP weight out of safety concern
    - b. Preferences for weight correlate with the preference for unobservable quality that is not listed as an attribute

Scott (2022) J of Law & Econ

Q1. why is this Q difficult to answer?

What about RCT then?

- 1. Randomize consumers' preferences for safety?
  - Ethics aside, can we randomize what people think in their brains, for real?
  - Ergo, not feasible
- 2. Alternatively, randomize a shock that shifts consumers' preferences?
  - This may be possible, but we cannot pass ethics and IRB
  - Ergo, not feasible
  - Here is where the natural experiment idea comes in

Scott (2022) J of Law & Econ

Data

- Texas Police Report
  - Author got this as a grad student in grad school
  - Key var: crash location address, involved individuals' address
- Fatality Analysis Reporting System (FARS) from NHTSA (Website)
  - Key vars: vehicle model, crash location
  - Key flaw: zipcode of crash location only, older FARS doesn't have VIN
- Texas DMV Vehicle Registration 2004 - 2010
  - Key vars: VIN, hh address
- Main data-linking effort of this study:
  - Extract $Weight_{it}$, the key yvar, using VIN and then apply a VIN-decoder
  - Extract $NeighborDied_{it}$ and $CrashWeight$ of i's neighbor, by
    - 1st, match FARS's crash location (zip) & model $\leftrightarrow$ police report
    - 2nd, use the police report to get victim/survivor's identity and address
    - 3rd, match victim/survivor address to DMV's address to identify (i) whose neighbor got a car crash and (ii) the weight of the crashed car using VIN from DMV

Scott (2022) J of Law & Econ

Main Estimation Equation

$$Weight_{it} = \beta NeighborDied_{it} + \gamma PostCrash_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \qquad \text{(Eq(4) of Scott (2022))}$$

- $Weight_{it}$: Total weight of all cars registered by i in month t
- $NeighborDied_{it}$: Neighor has died in car crash i in month t
- Control group: Same street (or 100-m radius) as the fatal crash, but the neighbor survived
- Q2: Why use cumulative weight?
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Scott (2022) J of Law & Econ

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Scott (2022) J of Law & Econ

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- Q5: Quasi-randomness comes from?
  - Temporal randomness: The timing t of the accident is as good as random (a better claim for weekly data rather than monthly)
  - Cross-sectional randomness: Whether the neighbor survived in an accident that has at least 1 casualty is as good as random

Scott (2022) J of Law & Econ

Balance Test + Pre-Trends

Table 2
Household Characteristics for Two Nearest Neighbors

	Treatment	Control	Difference
Demographics:			
Median income	41.79	44.35	−2.56
	(17.80)	(19.66)	(.31)
Median age	33.70	33.16	.54
	(5.17)	(5.04)	(.08)
Percentage white	72.71	72.57	.14
	(20.37)	(20.21)	(.33)
Percentage black	11.44	10.99	.45
	(17.50)	(16.70)	(.28)
Precrash vehicle characteristics:			
Curb weight	3,680.39	3,675.45	4.94
	(857.12)	(857.31)	(14.007)
Passenger car[a]	.49	.49	.00
	(.46)	(.45)	(.007)
Airbag	.77	.79	−.02
	(.37)	(.37)	(.006)
Miles per gallon	19.04	19.03	.01
	(4.16)	(4.05)	(.068)
Vehicle purchases	.73	.72	.01
	(.78)	(.77)	(.013)
Households	8,827	6,506	2,321

Note. Treated group households have a neighbor who died in an accident; control group households have a neighbor who survived an accident. Values are sample means, with standard deviations in parentheses. Demographic data are at the census-tract level.

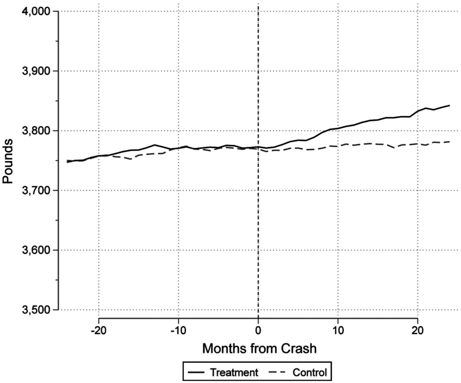


Figure 2. Effect of neighbor's fatality on household vehicle size

Scott (2022) J of Law & Econ

Main Results

Table 3

Effect of Neighbor's Fatality on Household Vehicle Size

	(1)	(2)	(3)	(4)	(5)
Postcrash	53.40**	56.5**	5.27	3.12	2.41
	(16.23)	(16.03)	(3.10)	(2.86)	(2.77)
Neighbor Died	32.16*	33.48*	34.50*	19.95*	16.37*
	(14.73)	(14.56)	(14.52)	(8.37)	(8.32)
Controls	No	Yes	Yes	No	No
Month-year fixed effects	No	No	Yes	Yes	Yes
Household fixed effects	No	No	No	Yes	Yes
County time trend	No	No	No	No	Yes

Note. Standard errors, clustered at the street level, are in parentheses. The sample includes the nearest two neighbors of a crash victim. All specifications control for the Postcrash indicator; those not controlling for household fixed effects control for a treatment-group indicator. $N = 964{,}824$.

* $p < .05$.
** $p < .01$.

Scott (2022) J of Law & Econ

Placebo

- $Weight_{it} = \beta NeighborDied_{it} + \gamma PostCrash_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \cdots$ Eq(4) of Scott (2022)
- Re-draw $NeighborDied_{it}$ with 1,000 reps
- When do people do this kind of placebo? What does this accomplish?
- What are typical ways to do the placebo?

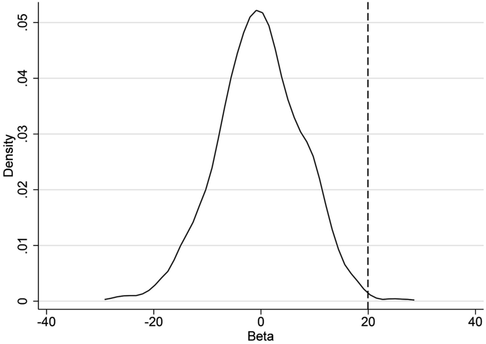


Figure 3. Distribution of placebo estimates on household vehicle weight

Scott (2022) J of Law & Econ

Spillover

$Weight_{it} = \beta \Delta CrashWeight_i \cdot PostCrash_{it} + \gamma PostCrash_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \cdots$ Eq(5) in Scott (2022)

- $CrashWeight_i$ relative weight: neighbor's car's weight - the other crashed car's weight
- Q6: Source of quasi-randomness for $\Delta CrashWeight_i \cdot PostCrash_{it}$?
- Temporal randomness: The timing t of the accident is as good as random (but monthly)
- Cross-sectional randomness: The exact relative weight is as good as random

Table 7
Spillover Effects of Vehicle Weights

	Neighbor Died		Household Vehicle Weight				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Weight difference (1,000 pounds)	−.0982**	−.0968**	−14.83*	−15.97**	−15.58**	−11.42**	−11.27**
	(.00505)	(.00512)	(6.059)	(6.007)	(5.993)	(3.774)	(3.747)
Controls	No	Yes	No	Yes	Yes	No	No
Month-year fixed effects	No	No	No	No	Yes	Yes	Yes
Household fixed effects	No	No	No	No	No	Yes	Yes
County time trend	No	No	No	No	No	No	Yes
N	6,626	6,626	414,817	414,817	414,817	414,817	414,817

Note. Standard errors are in parentheses. In columns 1 and 2, standard errors are clustered at the level of the county of residence, and the results are from the first-stage regression of the effect of the neighbor's and the opposing vehicle's weight difference on the probability that the neighbor dies. The sample includes all two-car collisions. In columns 3–7, standard errors are clustered at the street level. The sample is the two nearest neighbors. All specifications control for the Postcrash indicator.
* $p < .05$.
** $p < .01$.

Outline

- Review of Panel Data Regressions: FEs + DID ✓
- TWFE Example 1 Scott (2022) ✓
- TWFE Example 2 Severen & van Benthem (2022)
- LP-DID by Dube et al. (2025) + An Example Fabra et al. (2024)

Severen & van Benthem (2022) AEJ:AE

"Formative Experiences and the Price of Gasoline"

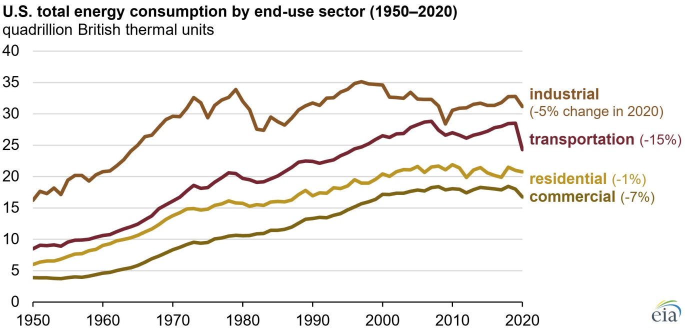
Research Question: How does the early-life exposure to gas price shocks affect future-life habits/demand of energy-use in transportation, measured by the extensive margins of (i) driving to work, (ii) taking public transportation to work, (iii) vehicle ownership, etc?

Severen & van Benthem (2022) AEJ:AE

Why these Yvars: Why do we care about the transportation sector?

The transportation sector is the top 2 sector in terms of energy use

- And there is some shifts across sectors over time



Source: U.S. Energy Information Administration, *Monthly Energy Review*
Note: End-use sector consumption includes primary energy consumption plus the electricity retail sales and associated electrical system energy losses from the electric power sector.

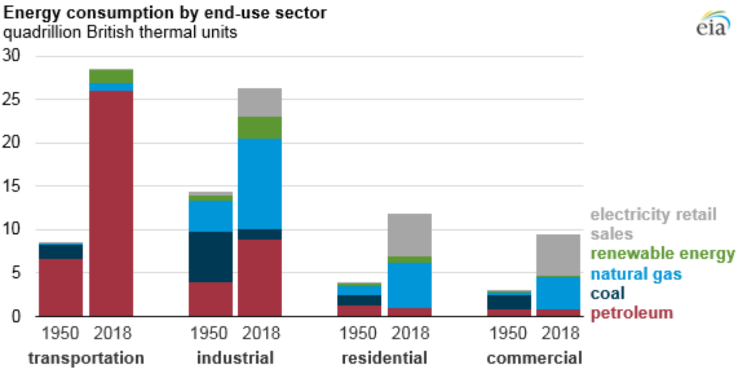
Source: EIA Link

Severen & van Benthem (2022) AEJ:AE

Why these Yvars: Why do we care about the transportation sector?

The transportation sector is the top 2 sector in terms of energy use, esp for gas/petrol

- And there are some shifts across sectors over time



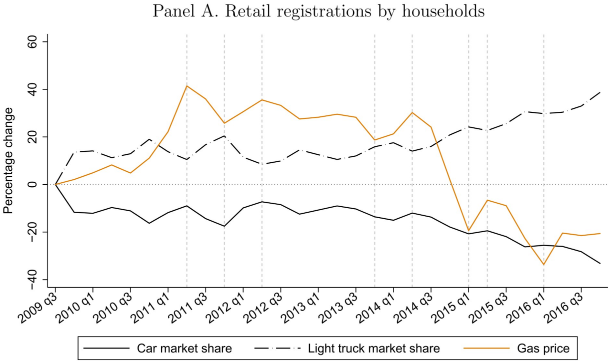
Source: EIA Link

Severen & van Benthem (2022) AEJ:AE

Why these Xvars: Why do we care about gas price shocks? Elasticities & substitution

Because we care about elasticities and substitution patterns:

- E.g., extensive margin of new vehicle demand



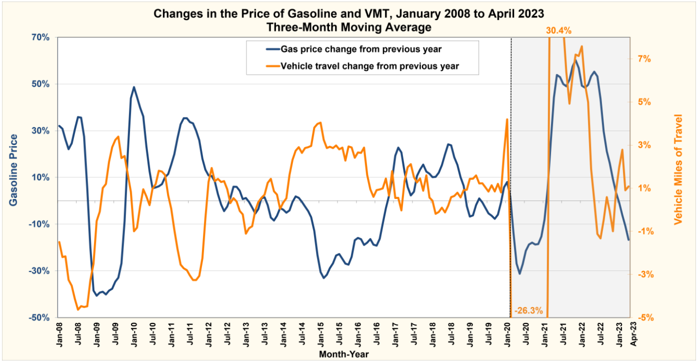
Source: Leard et al. (2019) JIndE

Severen & van Benthem (2022) AEJ:AE

Why these Xvars: Why do we care about gas price shocks?

Because we care about elasticities and substitution patterns:

- E.g., intensive margin of vehicle miles traveled (VMT) aka driving intensity



Source: DOE Link

Severen & van Benthem (2022) AEJ:AE

Data

- Main data: Decennial Census and ACS (Can use the PUMS version!)
  - Key yvar: (i) main mode of commute for workers (ii) public transit usage for workers, (iii) vehicle ownership for household
  - Key info for xvar: (i) birth year, (ii) birth state
  - Can define treatment using whether i happens to be able to experience oil crisis during initial driving age (if age ≤ 15 in 1980)
  - Can refine treatment using across-state variation in legal driving age during the oil crisis
- Historical gas price at state level
  - Energy Information Administration (EIA) for gas price post 1983
  - Federal Highway Administration (FHWA) for 1966-1982
- FHWA: Historical legal driving age requirement at state-year level
  - This will refine the xvar
- National Household Travel Survey (NHTS) from NHTSA
  - This will provide VMT as an extra yvar
  - Unlike one-yr ACS, these are in multiple waves

Severen & van Benthem (2022) AEJ:AE

Key Cross-sectional Variation from Temporal Variation: By Birth Year

Consider a future year t, say $t = 2000$:

- Depending on birth year c, a fraction of individuals will be young enough (e.g., 15 or younger by 1980) to fully experience oil crisis

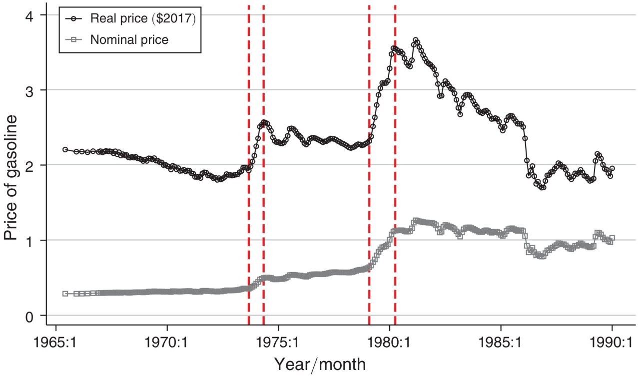


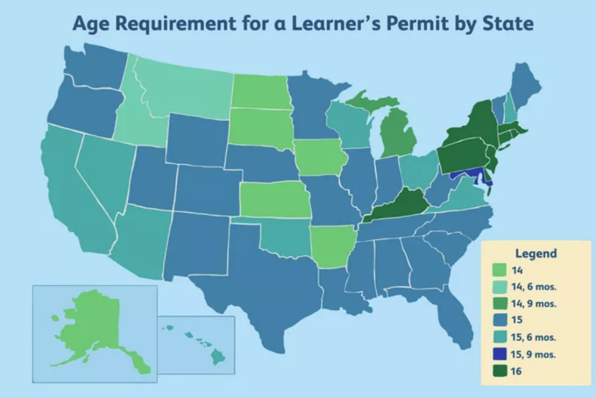
FIGURE 1. GASOLINE PRICES SPIKES IN THE UNITED STATES (1965–1990)

Severen & van Benthem (2022) AEJ:AE

Additional Cross-sectional Variation: By Birth State

Consider a future year t, say $t = 2000$:

- Depending on birth year y and birth state, a fraction of individuals will be young enough (e.g., 15 or younger by 1980) to fully experience the oil crisis



Severen & van Benthem (2022) AEJ:AE

Construct Xvar: Continuous Treatment

For each individual from birth cohort (birht year) c and state s, define treatment T_{cs} in four ways:

$$\text{(i)} \quad P_{cs}^{\Delta a,(a-h)} = \frac{P_{cs}^{a} - P_{cs}^{a-h}}{P_{cs}^{a-h}}, \qquad \text{(ii)} \quad P_{cs}^{a},$$

$$\text{(iii)} \quad P_{cs}^{\Delta(m_{cs}+j),(m_{cs}-k)} = \frac{P_{cs}^{m_{cs}+j} - P_{cs}^{m_{cs}-k}}{P_{cs}^{m_{cs}-k}}, \qquad \text{(iv)} \quad P_{cs}^{m_{cs}},$$

- Definition 1: Gas price changes from age 15 to 17 relative to 15 (baseline),
  Gas price changes from age 16 to 17 relative to 16
- Definition 2: Gas price for age 16 (base)
- Definition 3: Suppose legal driving age = 17, then construct
  Gas price changes from age 16 to 18 relative to 16 (baseline), or
  Gas price changes from age 15 to 19 relative to 15
- Definition 4: Suppose legal driving age = 17, then construct
  Gas price at age 17
- Definition 5: 1{Age 15 or younger by 1980} when analyzing 2000 sample (appendix)

Severen & van Benthem (2022) AEJ:AE

Estimation Equation

For individual i in cohort (birth year) c born in state s in sample year t:

$$(2) \qquad Y_{icst} = \theta T_{cs} + \kappa_s + \delta_t + \eta_{t-c} + X_{it}'\lambda + \varepsilon_{icst},$$

- T_{cs}: A continuous treatment (4 versions)
  You can think of this as a continuous-treatment DID
- X_{it}: Individual or hh characteristics
- FEs: κ_s: State FE
  δ_t: Sample year FE
  η_{t-c}: Age-at-time-of-sample FE
- Q1: What are we effectively comparing given the construction of Xvar + the FEs?
- Q2: The as-good-as random part in T_{cs} is from?
- Q3: Could you use this design for other questions?

Severen & van Benthem (2022) AEJ:AE

Estimation Equation

For individual i in cohort (birth year) c born in state s in sample year t:

$$Y_{icst} = \theta T_{cs} + \kappa_s + \delta_t + \eta_{t-c} + X'_{it}\lambda + \varepsilon_{icst}, \tag{2}$$

- T_{cs}: A continuous treatment (4 versions)
  You can think of this as a continuous-treatment DID
- X_{it}: Individual or hh characteristics
- FEs: κ_s: State FE
  δ_t: Sample year FE
  η_{t-c}: Age-at-time-of-sample FE
- Q1: What are we effectively comparing given the construction of Xvar + the FEs?
- Q2: The as-good-as random part in T_{cs} is from?
- Q3: Could you use this design for other questions? Hint: Cohort-specific shocks
  - E.g., Isen, Rossin-Slater, and Walker (2017) "Relationship between Season of Birth, Temperature Exposure, and Later Life Wellbeing" PNAS 114 (51) 13447-13452

Severen & van Benthem (2022) AEJ:AE

Results: A Cross-Sectional Regression for Census 2000 Sample

Table A.3: Event study in turning 15 after 1979 on commuting behavior in 2000.

Model	Poly. order	Bandwidth (years) 2	3	4	5	6	7	8	9	10
Panel A: Effect on driving, no controls										
	1	-0.0050* (0.0022)	-0.0029+ (0.0016)	-0.0026+ (0.0014)	-0.0032** (0.0012)	-0.0026* (0.0011)	-0.0027** (0.0010)	-0.0032** (0.0009)	-0.0032** (0.0009)	-0.0029** (0.0008)
	2				-0.0033 (0.0022)	-0.0039* (0.0019)	-0.0032+ (0.0016)	-0.0021 (0.0015)	-0.0027+ (0.0014)	-0.0032* (0.0013)

Table A.4: Event study in turning 15 after 1979 on transit usage and vehicle access in 2000.

Poly. order	Bandwidth (years) 2	3	4	5	6	7	8	9	10
Panel A: Transit usage									
1	0.0036* (0.0015)	0.0027* (0.0011)	0.0027** (0.0009)	0.0023** (0.0008)	0.0017* (0.0007)	0.0016* (0.0007)	0.0016** (0.0006)	0.0015** (0.0006)	0.0018** (0.0005)
2				0.0038** (0.0014)	0.0037** (0.0012)	0.0030** (0.0011)	0.0023* (0.0010)	0.0024** (0.0009)	0.0018* (0.0009)
N	545k	811k	1075k	1343k	1614k	1888k	2148k	2398k	2642k

Severen & van Benthem (2022) AEJ:AE

Main Results: Driving for Work for Workers

TABLE 1—THE EFFECT OF FORMATIVE GASOLINE PRICES ON DRIVING TO WORK

	1[drive] (1)	**1**[drive] (2)	**1**[drive] (3)	**1**[drive] (4)	**1**[drive] (5)	**1**[drive] (6)	**1**[drive] (7)
Exposure defined by age							
$P_{cs}^{\Delta 17,15}$	−0.0038	−0.0028	−0.0031	−0.0037	−0.0039	−0.0039	−0.0043
	(0.0010)	(0.0008)	(0.0009)	(0.0010)	(0.0010)	(0.0010)	(0.0009)
P_{cs}^{16}	−0.0007	0.0012	−0.0029	−0.0009	−0.0011	−0.0011	−0.0011
	(0.0010)	(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0008)	(0.0008)
Exposure defined by minimum driver license age							
$P_{cs}^{\Delta(m_{cs}+1, m_{cs}-1)}$	−0.0041	−0.0038	−0.0040	−0.0040	−0.0040	−0.0042	−0.0045
	(0.0010)	(0.0008)	(0.0008)	(0.0011)	(0.0010)	(0.0011)	(0.0010)
$P_{cs}^{m_{cs}}$	−0.0012	0.0006	−0.0012	−0.0013	−0.0015	−0.0015	−0.0015
	(0.0010)	(0.0006)	(0.0010)	(0.0009)	(0.0009)	(0.0008)	(0.0008)
Census year fixed effects	Yes	Yes	Yes	Yes	Yes	—	—
State of birth fixed effects	Yes	Yes	Yes	Yes	Yes	—	—
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	—	—	—	Yes	Yes	Yes	Yes
ln HH income	—	—	—	—	Yes	Yes	Yes
State × year fixed effects	—	—	—	—	—	Yes	Yes
Quad. birth year	—	—	—	—	—	—	Yes
Price in state of	Birth	Birth	Res	Birth	Birth	Birth	Birth
Sample	Stay	All	All	Stay	Stay	Stay	Stay

Severen & van Benthem (2022) AEJ:AE

Main Results: Pub Transit for Work for Workers + Vehicle Ownership for HH

TABLE 2—THE EFFECT OF FORMATIVE GASOLINE PRICES ON OTHER CENSUS OUTCOMES

	Transit usage		Vehicle available			
	1[transit] (1)	**1**[transit] (2)	**1**[vehicle] (3)	**1**[vehicle] (4)	**1**[vehicle] (5)	**1**[vehicle] (6)
Exposure defined by age						
$P_{cs}^{\Delta 17,15}$	0.0029	0.0024	−0.0014	−0.0009	−0.0019	−0.0018
	(0.0007)	(0.0009)	(0.0008)	(0.0006)	(0.0009)	(0.0006)
P_{cs}^{16}	0.0001	0.0004	0.0004	0.0007	−0.0007	−0.0001
	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0009)	(0.0007)
Exposure defined by minimum driver license age						
$P_{cs}^{\Delta(m_{cs}+1,m_{cs})}$	0.0027	0.0025	−0.0020	−0.0017	−0.0022	−0.0021
	(0.0008)	(0.0009)	(0.0011)	(0.0009)	(0.0012)	(0.0010)
$P_{cs}^{m_{cs}}$	0.0006	0.0008	0.0001	0.0003	−0.0008	−0.0005
	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0008)	(0.0006)
Census year fixed effects	Yes	—	Yes	—	Yes	—
State of birth fixed effects	Yes	—	Yes	—	Yes	—
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	—	Yes	—	Yes	—	Yes
ln HH income	—	Yes	—	Yes	—	Yes
State × year fixed effects	—	Yes	—	Yes	—	Yes
Quad. birth year	—	Yes	—	Yes	—	Yes
Sample	Empl	Empl	Empl	Empl	All	All

Severen & van Benthem (2022) AEJ:AE

Main Results: VMT for HH (Smaller Sample from NHTS)

TABLE 3—THE EFFECT OF FORMATIVE GASOLINE PRICES ON LOG MILES TRAVELED (USING NHTS)

	ln(VMT) (1)	ln(VMT) (2)	ln(VMT) (3)	ln(VMT) (4)	ln(VMT) (5)
Exposure defined by age					
$P_{cs}^{\Delta 17,15}$	−0.0776	−0.0812	−0.0759	−0.0763	−0.0613
	(0.0267)	(0.0264)	(0.0263)	(0.0260)	(0.0256)
P_{cs}^{16}	0.0216	0.0206	0.0192	0.0199	0.0034
	(0.0108)	(0.0110)	(0.0109)	(0.0110)	(0.0096)
Exposure defined by minimum driver license age					
$P_{cs}^{\Delta(m_{cs}+1, m_{cs}-1)}$	−0.0483	−0.0546	−0.0451	−0.0460	−0.0326
	(0.0194)	(0.0198)	(0.0201)	(0.0203)	(0.0197)
$P_{cs}^{m_{cs}}$	0.0152	0.0133	0.0114	0.0112	−0.0023
	(0.0119)	(0.0120)	(0.0116)	(0.0117)	(0.0106)
Sample year fixed effects	Yes	Yes	Yes	—	—
State fixed effects	Yes	Yes	Yes	—	—
Age fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	—	Yes	Yes	Yes	Yes
Income-by-year bin fixed effects	—	—	Yes	Yes	Yes
State × year fixed effects	—	—	—	Yes	Yes
Quad. birth year	—	—	—	—	Yes

Severen & van Benthem (2022) AEJ:AE

Estimation Equation: Investigate Formative Window

For individual i in cohort (birth year) c born in state s in sample year t.

$$(2) \qquad Y_{icst} = \theta T_{cs} + \kappa_s + \delta_t + \eta_{t-c} + X'_{it}\lambda + \varepsilon_{icst},$$

$$(3) \qquad Y_{icst} = \sum_{a=13}^{20} \theta_a P_{cs}^{\Delta a,a-1} + \kappa_s + \delta_t + \eta_{t-c} + X'_{it}\lambda + \varepsilon_{icst}.$$

- Eq(3) resembles running an "Event Study" for Eq(2) in TWFE format
  - Suppose event year s is the year when a person is age 17
  - Using dummies of event year $s = -4, -3, -2, -1, 0, 1, 2, 3, 4$
  - Except that here we don't have event year dummies
  - Here we use continuous treatment instead
- The main RHS vars are a whole host of $P_{cs}^{\Delta a,a-1}$:
  Gas price changes from age 12 to 13 relative to 12,
  Gas price changes from age 13 to 14 relative to 13,
  Gas price changes from age 14 to 15 relative to 14, ... ,
  Gas price changes from age 19 to 20 relative to 19.

Severen & van Benthem (2022) AEJ:AE

Main Results: The Age/stage-of-life Matters the Most

TABLE 4—THE EFFECT OF GASOLINE PRICE CHANGES AT DIFFERENT AGES

	Extensive margin		Intensive margin	
	1[drive] (1)	**1**[drive] (2)	ln(VMT) (3)	ln(VMT) (4)
$P_{cs}^{\Delta 13,12}$		−0.0007		−0.0616
		(0.0018)		(0.0585)
$P_{cs}^{\Delta 14,13}$	−0.0002	−0.0002	0.0009	0.0063
	(0.0015)	(0.0016)	(0.0336)	(0.0413)
$P_{cs}^{\Delta 15,14}$	−0.0002	−0.0003	0.0158	−0.0010
	(0.0019)	(0.0022)	(0.0430)	(0.0447)
$P_{cs}^{\Delta 16,15}$	−0.0057	−0.0057	−0.1024	−0.0930
	(0.0019)	(0.0021)	(0.0483)	(0.0524)
$P_{cs}^{\Delta 17,16}$	−0.0027	−0.0026	−0.0799	−0.0963
	(0.0015)	(0.0017)	(0.0417)	(0.0415)
$P_{cs}^{\Delta 18,17}$	−0.0024	−0.0023	−0.0839	−0.0646
	(0.0017)	(0.0019)	(0.0392)	(0.0388)
$P_{cs}^{\Delta 19,18}$	−0.0013	−0.0013	−0.0500	−0.0673
	(0.0017)	(0.0018)	(0.0498)	(0.0466)
$P_{cs}^{\Delta 20,19}$		−0.0006		−0.0117
		(0.0019)		(0.0463)
Sample year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes

Severen & van Benthem (2022) AEJ:AE

Some Questions from the Class on Perusall

- Q. Will we have same result with data of countries where people are more accustomed to public transport than private transport. What will be the effect of high gas prices?
- Q. While the minimum age is below 18, I know a lot of people who waited until they were 18 or sometimes even after 18. I feel that the authors here are assuming that people start driving in each state as soon as possible. While I concede that this is likely the case, it would have been nice to see a survey here to back this claim up.
- Q. For Table 1: I would have expected the income fixed effects to have a greater impact on the final results, 1) because it seems like an individual would be more likely to own a car with higher income levels and 2) because they would have faced less hardship during the oil crises of this time. Table 2 accounts for some of this, but I would expect a greater impact in our primary regression.
  - A. To answer, think about what robustness/stable coefficients imply?
- Q. For Table 4: I am confused how to implement an exposure function and how it works.
  - A. One can define a cumulative exposure measure two ways (i) NPV of individual treatments using an assumed discounting factor, and (ii) a NPV function of individual treatments with a discount/formation parameter to estimate
  - A. Think when (to what end) would you benefit from doing so?
  - A. Are you going after a habit formation parameter?

Outline

- Review of Panel Data Regressions: FEs + DID ✓
- TWFE Example 1 Scott (2022) ✓
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LP-DID

History and Development

History and development of LP-DID

- Local projections (LP): Macro and time-series researchers like to estimate impulse-response (R)functions (Jorda 2005 AER; Jorda and Taylor 2025 JEL)
- Applied micro (panel data) researchers like to run DID and event studies
- **Methodology and Best Practice**: Dube, Girardi, Jorda, and Taylor (2025) "A Local Projections Approach to DID Event Studies" J of Applied Econometrics
- **An Example**: Fabra, Gutierrez, Lacuesta, and Ramos (2024) "Do Renewable Energy Investments Create Local Jobs?" JPubE
- Let's cover Dube et al. (2025) first as a 101, then back to Fabra et al. (2024)
- Goal of LP-DID
  - We'd like an "event-study-style" plot that resembles what macro ppl use IR or LP for
  - We don't want to assume any structure, as in IR
  - We don't want to assume any auto-regressive (AR) process
  - We want to run it in panel data

Dube et al. (2025)

LP-DID Set-up

$$\Delta Y_{i,t-1,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_t + \varepsilon_{it}, \text{ with event time } h \quad (1)$$

- $\Delta Y_{i,t-1,t+h}$ is defined as long-difference $Y_{i,t+h} - Y_{i,t-1}$
- For evennt time $h = -Q, \cdots, -2, -1, 0, 1, 2, \cdots H$
  - Suppose $Q = 5$, $H = 5$
  - Suppose researchers choose base year $h = -1$
  - Suppose each event time is a year
  - Then run Eq(1) 10 times in 10 separate regressions when $h =$ -5, -4, -3, -2, 0, 1, 2, 3, 4, 5 and obtain $\hat{\beta}_h$ for 10 of them
  - Plot LP-DID event study: Plot 10 of $\hat{\beta}_h$ together with CI, set the base year as zero

Dube et al. (2025)

LP-DID Set-up

$$\Delta Y_{i,t-1,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_t + \varepsilon_{it}, \text{ with event time } h \tag{1}$$

- $\Delta Y_{i,t-1,t+h}$ is defined as long-difference $Y_{i,t+h} - Y_{i,t-1}$
- For evennt time $h = -Q, \cdots, -2, -1, 0, 1, 2, \cdots H$
  - Suppose $Q = 5$, $H = 5$
  - Suppose researchers choose base year $h = -1$
  - Suppose each event time is a year
  - Then run Eq(1) 10 times in 10 separate regressions when h = -5, -4, -3, -2, 0, 1, 2, 3, 4, 5 and obtain $\hat{\beta}_h$ for 10 of them
  - Plot LP-DID event study: Plot 10 of $\hat{\beta}_h$ together with CI, set the base year as zero
- FEs
  - Individual FE ϕ_i: Do not throw this in, as this is already FD
  - Time FE ϕ_t: Only throw this in if you have multiple-treatment timing

Dube et al. (2025)

LP-DID Set-up

$$\Delta Y_{i,t-1,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_t + \varepsilon_{it}, \text{ with event time } h \quad (1)$$

- $\Delta Y_{i,t-1,t+h}$ is defined as long-difference $Y_{i,t+h} - Y_{i,t-1}$
- For evennt time $h = -Q, \cdots, -2, -1, 0, 1, 2, \cdots H$
  - Suppose $Q = 5$, $H = 5$
  - Suppose researchers choose base year $h = -1$
  - Suppose each event time is a year
  - Then run Eq(1) 10 times in 10 separate regressions when $h =$ -5, -4, -3, -2, 0, 1, 2, 3, 4, 5 and obtain $\hat{\beta}_h$ for 10 of them
  - Plot LP-DID event study: Plot 10 of $\hat{\beta}_h$ together with CI, set the base year as zero
- FEs
  - Individual FE ϕ_i: Do not throw this in, as this is already FD
  - Time FE ϕ_t: Only throw this in if you have multiple-treatment timing
- Optional: Outcome lags such as $\sum_j \gamma_j Y_{i,t-j}$ for $j = 1, 2, \cdots J$
  - This is like a distributed-lag model
  - Suppose $J = 2$, then you throw in $Y_{i,t-1}$ and $Y_{i,t-2}$, aka 1-yr and 2-yr lags in Yvar relative to event year t

LP-DID

Panel Data Version vs FD Version

Either version is fine:

$$\Delta Y_{i,t-1,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_t + \varepsilon_{it} \quad \text{, with event time } h \tag{1}$$

$$Y_{i,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_i + \phi_t + \varepsilon_{it} \quad \text{, with event time } h \tag{2}$$

- Eq(1) FD: Dube at al. (2025) $\Delta Y_{i,t-1,t+h}$ is defined as $Y_{i,t+h} - Y_{i,t-1}$
- Eq(2) PD: Fabra at al. (2024) $Y_{i,t+h}$
- Regard Eq(1) as a FD of Eq(1), and ergo no more ϕ_i
- Usually people run Eq(1) but Eq(2) is not unheard of
- I abuse the notation of **Z** but both can throw-in *it*-specific control
- Arrange data in a staggered fashion when you have multiple-treatment timing

LP-DID

Key: Clean Control Condition

$$\Delta Y_{i,t-1,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_t + \varepsilon_{it} \quad \text{, with event time } h \tag{1}$$

$$Y_{i,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_i + \phi_t + \varepsilon_{it} \quad \text{, with event time } h \tag{2}$$

But we need to add a clean control condition for each regression for event time h. We shall restrict the estimation sample to

$$\begin{cases} \text{newly treated,} & \Delta D_{it} = 1 \\ \text{or clean control,} & D_{i,t+h} = 0 \end{cases}$$

- Clean control includes (i) never treated and (ii) not yet treated by $t + h$

LP-DID

Key: Clean Control Condition

$$\Delta Y_{i,t-1,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_t + \varepsilon_{it} \quad \text{, with event time } h \tag{1}$$

$$Y_{i,t+h} = \beta_h \Delta D_{it} + \gamma \mathbf{Z}_{it} + \phi_i + \phi_t + \varepsilon_{it} \quad \text{, with event time } h \tag{2}$$

But we need to add a clean control condition for each regression for event time h. We shall restrict the estimation sample to

$$\begin{cases} \text{newly treated,} & \Delta D_{it} = 1 \\ \text{or clean control,} & D_{i,t+h} = 0 \end{cases}$$

- Clean control includes (i) never treated and (ii) not yet treated by $t + h$
- Given the clean control condition, you should expect SE to get larger and CI to get wider as h increases to H
- One can also simply use strict clean control that includes never-treated units only

LP-DID

Typical Results will be an Event-study-style Plot: E.g., Dube et al. (2025)

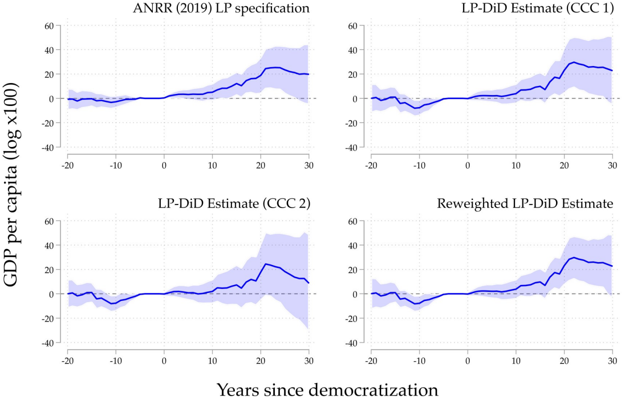


FIGURE 4 | Effect of democracy on growth: LP-DiD estimates. *Note:* LP-DiD estimates for the effect of democracy on GDP per capita, using the specification of Equation (16). Top left panel ("ANRR (2019) LP specification") replicates results in Section IV of ANRR, which set $L = 1$ year in the clean control condition. The other three panels set $L = 20$ years. CCC 1 is a clean control condition that defines treated units as countries that democratize in year t and experienced no transition between $t - 20$ and $t - 1$ and clean controls as countries that are continually nondemocracies between $t - 20$ and t. CCC 2 defines treated units in the same way, but clean controls are continually nondemocracies between $t - 20$ and $t + h$. Right bottom panel uses reweighting to obtain an equally weighted effect, using CCC 1. See main text for more details.

Fabra, Gutierrez, Lacuesta, and Ramos (2024) JPubE

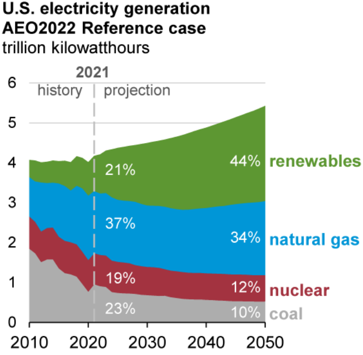
"Do Renewable Energy Investments Create Local Jobs?"

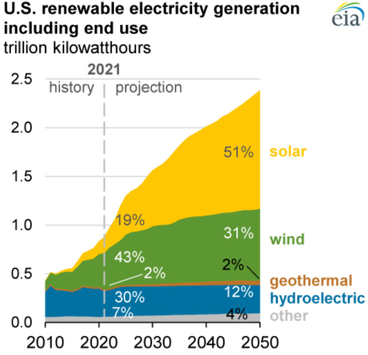
Research Question: What are the local labor market effects for solar and wind projects for municipalities in Spain?

FGLR (2024) JPubE

Why this xvar? Why do we care about renewable energies?

- Energy transition: a shift in energy production by source
- From coal-fired plants to renewables



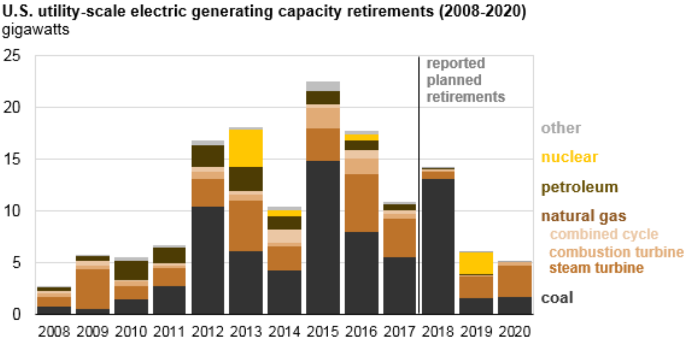


Source: EIA Link

FGLR (2024) JPubE

Why this xvar? Why do we care about renewable energies?

- The main player retiring out of the market: coal-fired energy
- But... many communities (workers and businesses) are dependent on fossil-fuel energy
- While renewable offers lower MC (aka more efficient) power generation, does it soften the blow of fossil-fuel energy phasing out?



Source: EIA Link

FGLR (2024) JPubE

Main Data and Key Cross-sectional/Spatial Variation

- Unit of obs: Municipal by month
- 3,000 municipalities in Spain over 2017-2021 (→US-equivalent is ACS or other PUMS)
- Location & timing of solar and wind energy projects (→US-equivalent is EIA Form-860)

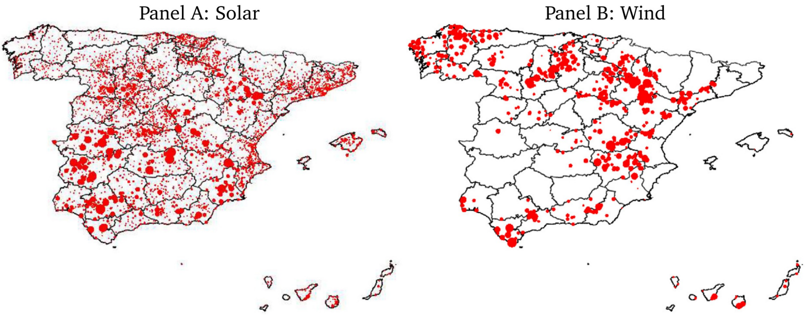
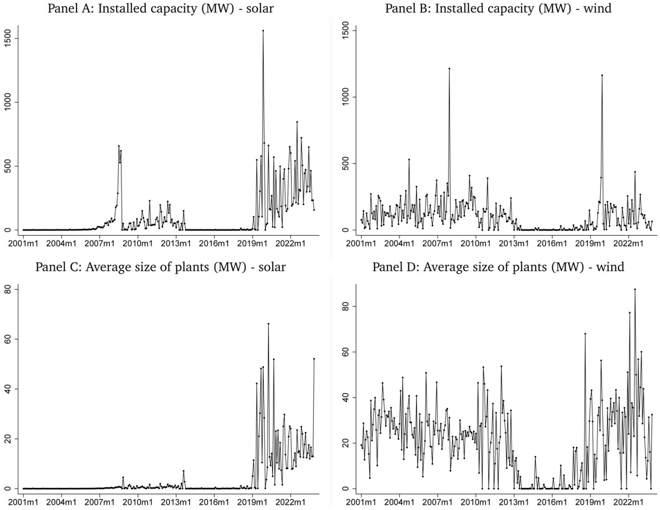


Fig. 2. Spatial distribution of investments in wind and solar energy.

FGLR (2024) JPubE

Main Data and Key Temporal Variation



FGLR (2024) JPubE

Main Estimation Equation

For municipal i in year-by-month t

$$y_{i,t+h} = \beta^{s}_{\tau+h}\Delta k^{s}_{i,t} + \beta^{w}_{\tau+h}\Delta k^{w}_{i,t} + \gamma_h X_{i,t} + \alpha_{h,i} + \lambda_{h,t} + \epsilon_{i,t+h} \tag{1}$$

- y_{it}: local employment, unemployment, etc.
- Δk^{s}_{it}: Solar energy treatment dummy, or megawatt-capacity change
- Δk^{w}_{it}: Wind energy treatment dummy, or megawatt-capacity change
- τ: Event-starting month
- h: ranges from -36 to 12 months
- FEs: $\alpha_{h,i}$: Municipal-by-month FE
  $\lambda_{h,t}$: Year-by-month FE
- Run Eq(1) 48+1 times (no base year)

FGLR (2024) JPubE

Main Estimation Equation and Clean Control Condition, $h = -15$

For municipal i in year-by-month t

$$y_{i,t+h} = \beta^{s}_{\tau+h}\Delta k^{s}_{i,t} + \beta^{w}_{\tau+h}\Delta k^{w}_{i,t} + \gamma_h X_{i,t} + \alpha_{h,i} + \lambda_{h,t} + \epsilon_{i,t+h} \tag{1}$$

Clean control condition

$$\text{Treatment} \quad \begin{cases} \Delta k_{i,t} > 0; \quad k_{i,t-1} = 0 & \text{if} \quad h < -12 \\ \Delta k_{i,t} > 0; \quad k_{i,t-1} = 0; \quad k_{i,t} = k_{i,t+12+h} & \text{if} \quad h \geq -12 \end{cases}$$

$$\text{Clean control} \quad \begin{cases} k_{i,t} = 0 & \text{if} \quad h < -12 \\ k_{i,t} = k_{i,t+12+h} = 0 & \text{if} \quad h \geq -12 \end{cases} \tag{10}$$

- Choose to use event time as -12 months to set up clean control
- If running Eq(1) for $h < -12$
- E.g., for h=-15
- Include 1: never-treated units
- Include 2: newly/eventually treated units

FGLR (2024) JPubE

Main Estimation Equation and Clean Control Condition, $h = 0$

For municipal i in year-by-month t

$$y_{i,t+h} = \beta^{s}_{\tau+h}\Delta k^{s}_{i,t} + \beta^{w}_{\tau+h}\Delta k^{w}_{i,t} + \gamma_h X_{i,t} + \alpha_{h,i} + \lambda_{h,t} + \epsilon_{i,t+h} \tag{1}$$

Clean control condition

$$\begin{aligned}
&\text{Treatment} \quad \begin{cases} \Delta k_{i,t} > 0; \quad k_{i,t-1} = 0 & \text{if} \quad h < -12 \\ \Delta k_{i,t} > 0; \quad k_{i,t-1} = 0; \quad k_{i,t} = k_{i,t+12+h} & \text{if} \quad h \geq -12 \end{cases} \\
&\text{Clean control} \quad \begin{cases} k_{i,t} = 0 & \text{if} \quad h < -12 \\ k_{i,t} = k_{i,t+12+h} = 0 & \text{if} \quad h \geq -12 \end{cases}
\end{aligned} \tag{10}$$

- Choose to use event time as -12 months to set up clean control
- If running Eq(1) for $h \geq -12$
- E.g., for h=0
- Include 1: never-treated units
- Include 2: newly/eventually treated units
- Include 3: units that are not-treated within the next 12+0=12 months

FGLR (2024) JPubE

Main Estimation Equation and Clean Control Condition, $h = 6$

For municipal i in year-by-month t

$$y_{i,t+h} = \beta^{s}_{\tau+h}\Delta k^{s}_{i,t} + \beta^{w}_{\tau+h}\Delta k^{w}_{i,t} + \gamma_h X_{i,t} + \alpha_{h,i} + \lambda_{h,t} + \epsilon_{i,t+h} \tag{1}$$

Clean control condition

$$\begin{aligned}
&\text{Treatment} && \begin{cases} \Delta k_{i,t} > 0; \quad k_{i,t-1} = 0 & \text{if} \quad h < -12 \\ \Delta k_{i,t} > 0; \quad k_{i,t-1} = 0; \quad k_{i,t} = k_{i,t+12+h} & \text{if} \quad h \geq -12 \end{cases} \\
&\text{Clean control} && \begin{cases} k_{i,t} = 0 & \text{if} \quad h < -12 \\ k_{i,t} = k_{i,t+12+h} = 0 & \text{if} \quad h \geq -12 \end{cases}
\end{aligned} \tag{10}$$

- Choose to use event time as -12 months to set up clean control
- If running Eq(1) for $h \geq -12$
- E.g., for h=6
- Include 1: never-treated units
- Include 2: newly/eventually treated units
- Include 3: units that are not-treated within the next 12+6=18 months

FGLR (2024) JPubE

LP-DID Results

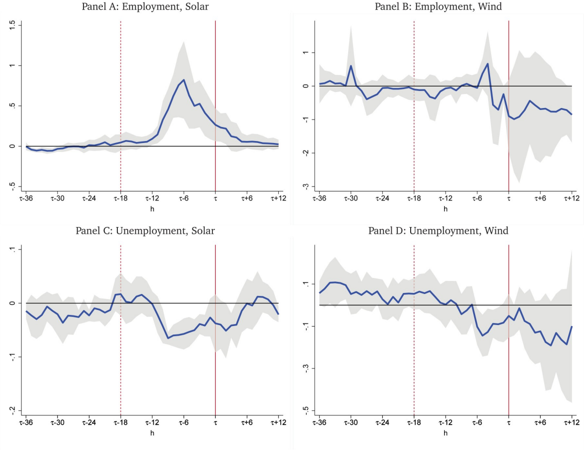


Fig. 13. Local employment and unemployment effects - clean control.

Notes: These figures show the effects of investing 1 MW in solar or wind plants on employment and unemployment in the period January 2017-November 2021, *h* months before or after the start-up date. The vertical solid red line marks the start-up date, while the dashed red line marks 18 months prior to that when the construction is likely to start. Treatment and control observations are restricted to account for the staggered adoption. Panels A and B show the employment results for solar and wind investments, and panels C and D the unemployment results. Error bands depict the 95% confidence interval. Standard errors are clustered at the municipality level.

FGLR (2024) JPubE

Other LP-DID Examples + Some Questions from the Class on Perusall

Other LP-DID Example

- Gilbert, Hoen, Gagarin (2025) "Distributional Equity in Employment and Wage Impact of Energy Transition", JAERE, Vol 11, Number S1, November 2024

FAQ

- Some questions regarding employment data
  - Basically, their employee data is from firm-level data, kind of like the US situation for LEHD and county business pattern (CBP)
  - Very-small firms (for temporary gigs) do not need to report the number of employees
  - Similar in the US, very small entities do not need to report employees, payroll, etc.
- Some questions regarding the unemployment data
  - Basically, the data is from the local gov't that needs to issue UIs
  - This is mostly an European data thing.
  - It will exclude individuals who register using their family/friend/lawyer/other address