Day 3: On Formulas and Models

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Design-Based Regression Inference
Fall 2024

Outline

1. Formula Treatments/Instruments
2. Nonlinear/Structural Models

Beyond Simple Treatments/Instruments

- We've seen how a regression of y_i on x_i and w_i identifies a convex average of treatment effects when $E[x_i \mid y_i(\cdot), w_i] = E[x_i \mid w_i] = w_i'\gamma$
  - IV version: $E[z_i \mid y_i(\cdot), w_i] = w_i'\gamma$ and exclusion/monotonicity hold

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- Let's build up to these slowly...

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  - Can use $s_i g_i$ as an IV controlling for s_i, given exclusion/monotonicity

Interacted Treatments (Cont.)

- Now suppose $g_i \mid y(\cdot), s, q \overset{iid}{\sim} G(q_i)$: e.g., a stratified RCT
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- Key point: the design of exogenous shocks g_i + knowledge of the "formula" $s_i g_i$ tells us what controls are needed for identification

Shift-Share Instruments

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- Cool new twist: we can use design to "translate" shocks from one level (e.g. industries) to estimate effects at another (e.g. regions)!

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  - I.e. need to control for the share-weighted average of shock-level confounders, $w_i = \sum_k s_{ik} q_k$
  - In Autor et al. (2014), this means controlling for the sum-of-shares interacted with period FE

Illustration: Autor et al. (2014) China Shock

ADH study the effects of rising Chinese import competition on US commuting zones, 1991-2000 and 2000-2007

- Treatment x_{it}: local growth of Chinese imports in \$1,000/worker
- Main outcome y_{it}: local change in manufacturing employment share

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To address endogeneity, ADH use a SSIV $z_{it} = \sum_n s_{int} g_{nt}$

- n: 397 SIC4 manufacturing industries $\times$ two periods
- g_{nt}: growth of Chinese imports in non-US economies per US worker
- s_{int}: lagged share of manufacturing industry n in *total* employment of location i; hence $\sum_n s_{int}$ is i's manufacturing employment share

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Design-based justification: random industry productivity shocks in China, jointly affecting imports in the U.S. and elsewhere

- Observed g_{nt} proxy for latent as-if-randomly assigned productivity shocks; need other drivers to be unrelated to local U.S. conditions

ADH Balance Tests

Balance variable	Coef.		SE
Panel A: Industry-level balance			
Production workers' share of employment, 1991	−0.011		(0.012)
Ratio of capital to value-added, 1991	−0.007		(0.019)
Log real wage (2007 USD), 1991	−0.005		(0.022)
Computer investment as share of total, 1990	0.750		(0.465)
High-tech equipment as share of total investment, 1990	0.532		(0.296)
No. of industry-periods		794	
Panel B: Regional balance			
Start-of-period % of college-educated population	0.915		(1.196)
Start-of-period % of foreign-born population	2.920		(0.952)
Start-of-period % of employment among women	−0.159		(0.521)
Start-of-period % of employment in routine occupations	−0.302		(0.272)
Start-of-period average offshorability index of occupations	0.087		(0.075)
Manufacturing employment growth, 1970s	0.543		(0.227)
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No. of region-periods		1,444	

- Panel A regresses industry characteristics on the g_{nt} shocks, controlling for period FE
- Panel B regresses location characteristics on the z_{it} instrument, controlling for manufacturing employment share $\times$ period FE

ADH Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient	−0.596	−0.489	−0.267	−0.314	−0.310	−0.290	−0.432
	(0.114)	(0.100)	(0.099)	(0.107)	(0.134)	(0.129)	(0.205)
Regional controls							
Autor *et al.* (2013) controls	✓	✓	✓		✓	✓	✓
Start-of-period mfg. share	✓						
Lagged mfg. share		✓	✓	✓	✓	✓	✓
Period-specific lagged mfg. share			✓	✓	✓	✓	✓
Lagged 10-sector shares					✓		✓
Local Acemoglu *et al.* (2016) controls						✓	
Lagged industry shares							✓
SSIV first stage F-stat.	185.6	166.7	123.6	272.4	64.6	63.3	27.6
No. of region-periods	1,444	1,444	1,444	1,444	1,444	1,444	1,444
No. of industry-periods	796	794	794	794	794	794	794

Note: columns 3-7 control for mfg. employment share $\times$ period FE

Contrast: Outome Modeling

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  - Condition on the shocks, then assume the shares are exogenous in the sense of $E[\varepsilon_i \mid s_{i1}, \ldots, s_{iK}] = 0$
  - When things are in first-differences, this is effectively parallel trends: e.g. regions with different exposure shares would have had the same outcome trends if not for shocks to treatment

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- Reasonable enough, but not design-based
    - Need an *ex ante* stand on the right form for y_i (e.g. logs v levels)
    - Any transformation of $(s_{i1}, \ldots, s_{iK})$ is a valid instrument, including the individual shares $\rightarrow$ overidentified, potentially massively so
    - Can't have any unobserved shocks which transmit to y_i through the same or correlated shares (e.g. can't have $\varepsilon_i = \sum_k s_{ik} \nu_k + \eta_i$, even if the ν_k are totally independent of the g_k)

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  - Could exploit other symmetries/discontinuities in the shocks (e.g. RD)

Formula Instruments: General Case

- We have a $z_i = f_i(g, s)$ for known $f_i(\cdot)$ and observed g and s
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  3. Compute expected instrument as $\mu_i = \frac{1}{L} \sum_\ell z_i^{(\ell)}$

Example: Borusyak and Hull (2023)

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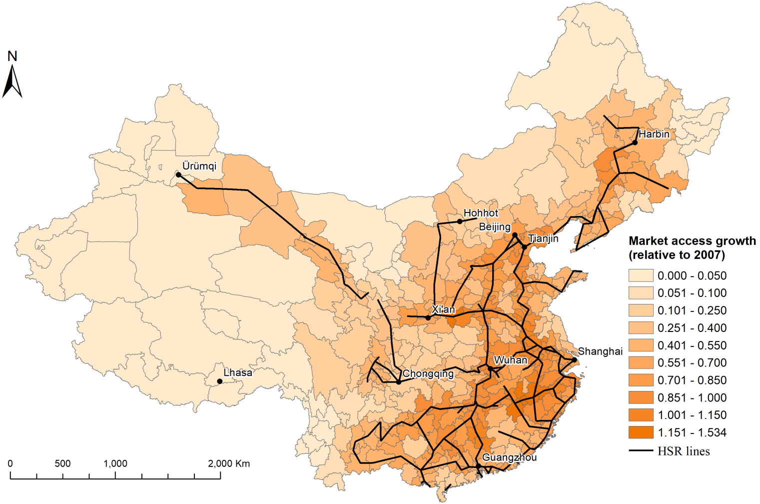
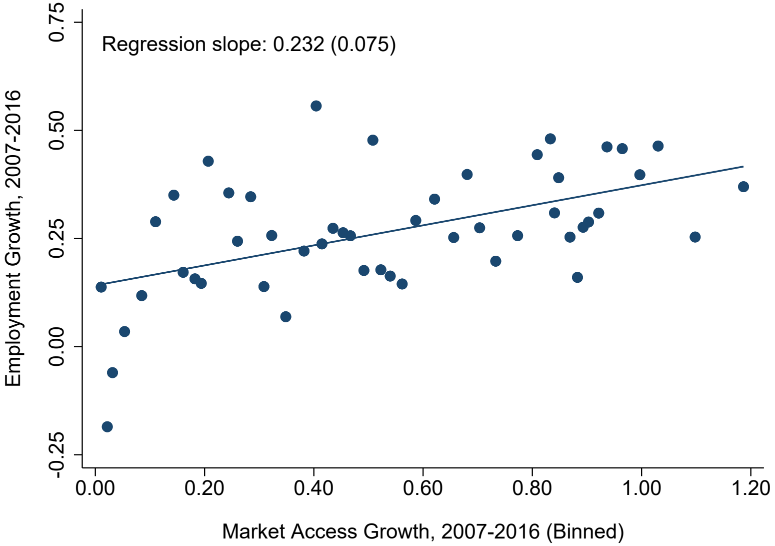
  - Then either control for or recenter by expected market access growth

HSR Lines and Market Access



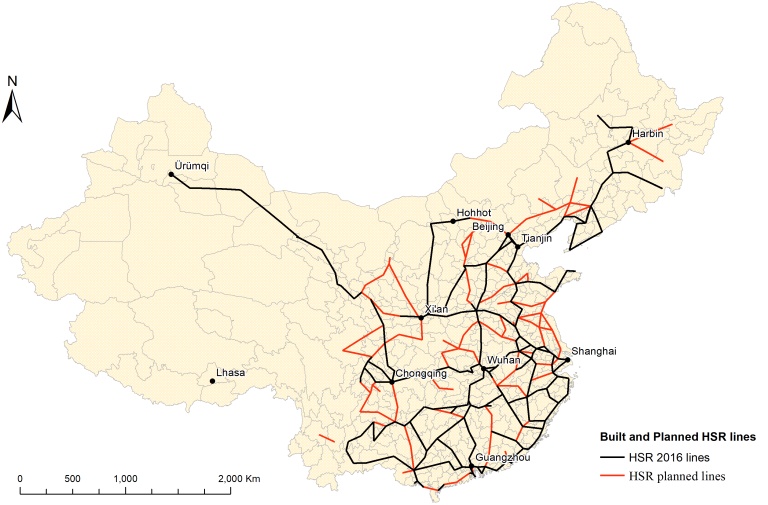
Naive OLS compares dark ("treatment") vs light ("control") regions

Naive OLS Suggests a Big Market Access Effect...



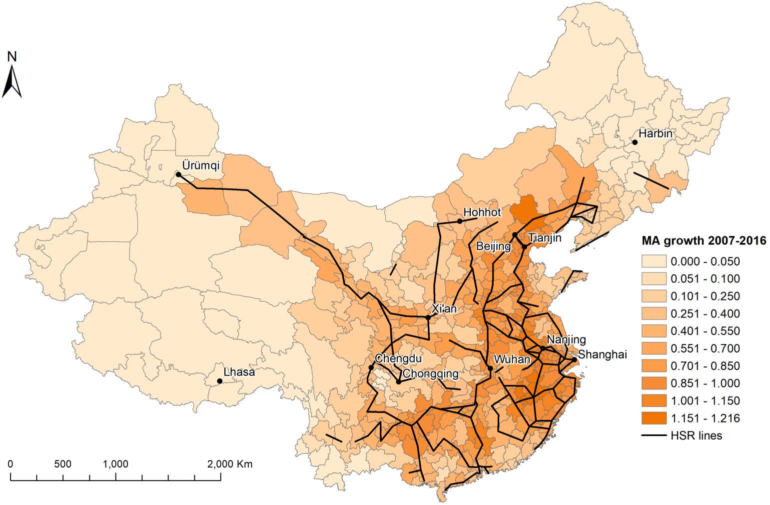
... but we probably shouldn't believe it

HSR Lines and Counterfactuals



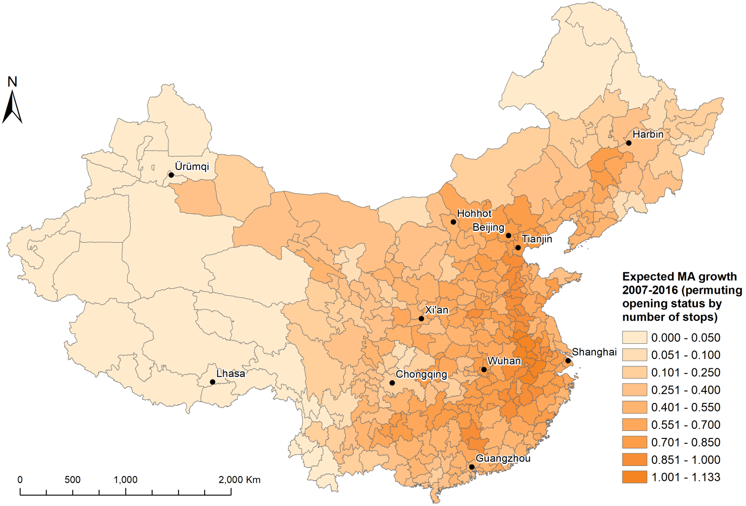
Counterfactuals permute which lines opened by 2016, conditional on length

An Example Counterfactual HSR Network



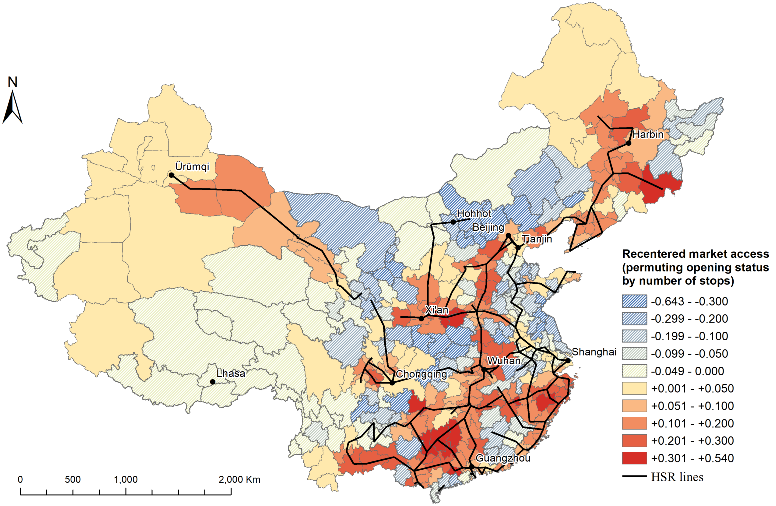
Seems ok...

Expected Market Access Across Counterfactuals



Darker regions see more MA growth regardless of which lines are built first

Recentered Market Access



Recentered IV compares region that saw more MA growth than expected (in red) to those that saw less MA growth than expected (in blue)

Balance Tests

	Unadjusted	Recentered		
	(1)	(2)	(3)	(4)
Distance to Beijing	−0.292	0.069		0.089
	(0.063)	(0.040)		(0.045)
Latitude/100	−3.323	−0.325		−0.156
	(0.648)	(0.277)		(0.320)
Longitude/100	1.329	0.473		0.425
	(0.460)	(0.239)		(0.242)
Expected Market Access Growth			0.027	0.056
			(0.056)	(0.066)
Constant	0.536	0.014	0.014	0.014
	(0.030)	(0.018)	(0.020)	(0.018)
Joint RI p-value		0.489	0.807	0.536
R^2	0.823	0.079	0.007	0.082
Prefectures	274	274	274	274

Recentered MA growth can't be reliably predicted from geography

BH Estimates

	Unadjusted OLS (1)	Recentered IV (2)	Controlled OLS (3)
Panel A. No Controls			
Market Access Growth	0.232	0.081	0.069
	(0.075)	(0.098)	(0.094)
		[-0.315, 0.328]	[-0.209, 0.331]
Expected Market Access Growth			0.318
			(0.095)
Panel B. With Geography Controls			
Market Access Growth	0.132	0.055	0.045
	(0.064)	(0.089)	(0.092)
		[-0.144, 0.278]	[-0.154, 0.281]
Expected Market Access Growth			0.213
			(0.073)
Recentered	No	Yes	Yes
Prefectures	274	274	274

Large effect for naive OLS goes away with recentering/controlling

- Once adjusting for μ_i, auxilliary controls don't matter ($\implies$ balance)

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- Ex: Medicaid eligibility is a treatment combining statewide policy shocks and individual exposure (income, family structure, etc)
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- In an application to ACA Medicaid expansions, we find this helps a lot
  - Design assumption: can swap expansion decisions of states conditional on whether the governor is a Republican or Democrat

Estimates of Private Insurance Crowdout Effects

	Has Medicaid		Has Private Insurance		Has Employer-Sponsored Insurance	
	Simulated IV (1)	Recentered IV (2)	Simulated IV (3)	Recentered IV (4)	Simulated IV (5)	Recentered IV (6)
Panel A. Eligibility Effects						
Eligibility	0.132	0.072	−0.048	−0.023	0.009	−0.009
	(0.028)	(0.010)	(0.023)	(0.007)	(0.014)	(0.005)
	[0.080,0.216]	[0.051,0.093]	[-0.110,0.009]	[-0.040,-0.007]	[-0.034,0.052]	[-0.021,0.004]
Panel B. Enrollment Effects						
Has Medicaid			−0.361	−0.321	0.068	−0.125
			(0.165)	(0.092)	(0.111)	(0.061)
			[-0.813,0.082]	[-0.566,-0.108]	[-0.232,0.421]	[-0.263,0.070]
P-value: SIV=RIV			0.719		0.104	
Exposed Sample	N	Y	N	Y	N	Y
States	43	43	43	43	43	43
Individuals	2,397,313	421,042	2,397,313	421,042	2,397,313	421,042

Inference Can Be Tricky with Formulas

- Common exposure to the exogenous shocks g make $z_i = f_i(g, s)$ correlated across i, potentially in complicated ways
  - E.g. for shift-share $z_i = \sum_k s_{ik} g_k$, if unit i and j are far apart in space but close in terms of $(s_{ik})_{k=1}^{K}$ and $(s_{jk})_{k=1}^{K}$ then $Cov(z_i, z_j) > 0$

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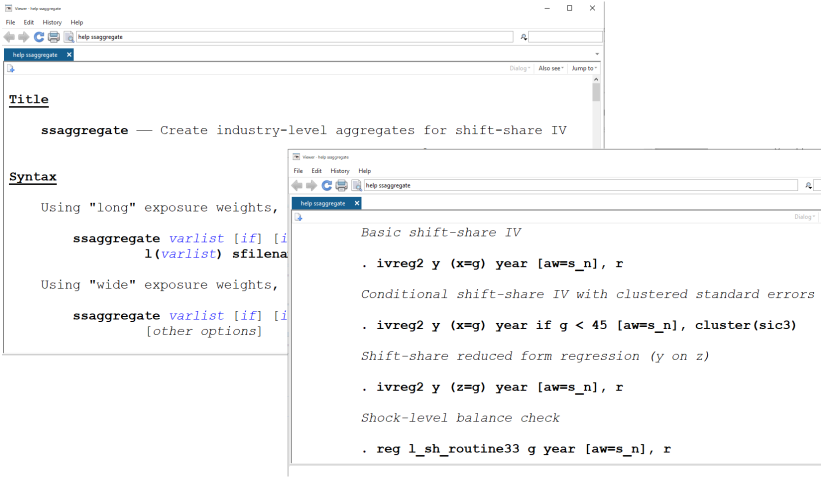
- For other $f_i(\cdot)$, Borusyak and Hull '23 propose randomization inference
  - Use the counterfactual g to simulate the distribution of test statistics under the null and check if the actual test is in the tails

ssaggregate in Stata



This is what I used to get SEs in the previous balance / IV tables

Outline

1. Formula Treatments/Instruments✓

2. Nonlinear/Structural Models

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  - In fact, we've already seen this: our Day-1 constant-effect model of $y_i = \beta x_i + \varepsilon_i$ can be understood as extrapolating simply across all i
  - Other (nonlinear) procedures can sometimes be seen as imposing different extrapolations to the same underlying (design-based) variation

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- Suppose you run an RCT with a binary treatment x_i. A regression of y_i on x_i identifies $E[y_i \mid x_i = 1] - E[y_i \mid x_i = 0] = E[y_i(1) - y_i(0)]$
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$$=$$

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&= \underbrace{\Phi\left(\frac{\alpha}{\sigma}\right)}_{\text{constant}} + \underbrace{\left\{\Phi\left(\frac{\alpha+\beta}{\sigma}\right) - \Phi\left(\frac{\alpha}{\sigma}\right)\right\}}_{\text{slope coefficient}} x_i
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  - Different extrapolation of missing CATE: a feature or a bug?

ExtrapoLATEing

- Consider the simplest design-based IV story: binary x_i, binary z_i, no controls. IV identifies LATE:

$$\beta^{IV} = E[y_i(1) - y_i(0) \mid x_i(1) > x_i(0)]$$

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- Without restrictions on $(y_i(1), y_i(0), x_i(1), x_i(0))$, can't say anything more: IV only reveals effects among i whose x_i is shifted by z_i
  - Actually not quite true: can identify avg. $y_i(1)$ of always-takers (w/ $x_i(1) = x_i(0) = 1$), avg. $y_i(0)$ of never-takers (w/ $x_i(1) = x_i(0) = 0$), as well as avg. $y_i(1)$ & $y_i(0)$ separately for compliers
  - By adding a (semi-)parametric model of selection, we can extrapolate these objects to identify other parameters, e.g., ATE $E[y_i(1) - y_i(0)]$

Adding Structure to IV

- Suppose we have a z_i which is as-if-randomly assigned + excludable
  - Assume a distribution for $(y_i(1), y_i(0), v_i)$ where $x_i = \mathbf{1}[\mu + \pi z_i > v_i]$

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- Key point: the model allows us to extrapolate "local" IV variation to estimate more "policy relevant" parameters
  - When z_i has limited support, the model is doing more "work"
  - With full support, we have "identification at infinity" (w/o a model)

Linking Back to LATE

- Kline and Walters (2019) formalize this extrapolation logic in the familiar Imbens and Angrist (1994) setup
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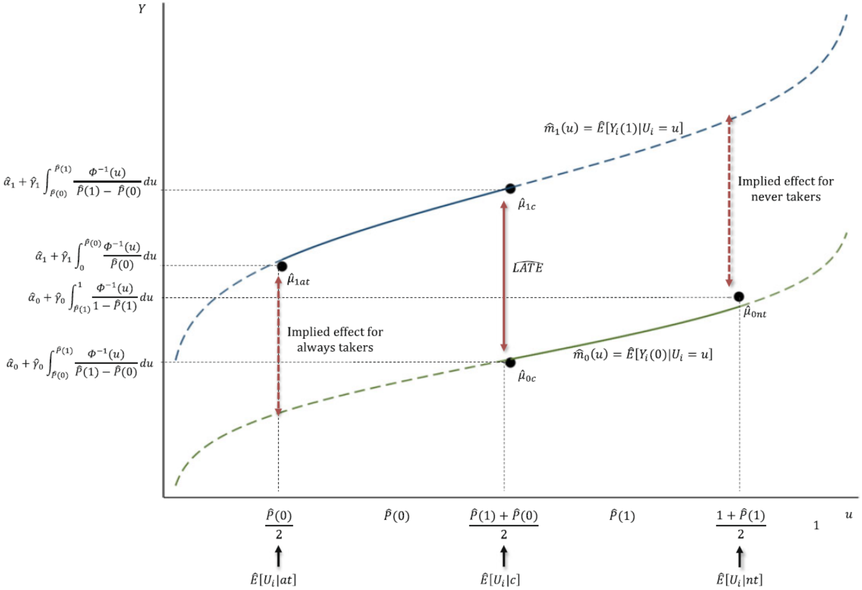
- This can all be extended to conditional as-if-random assignment
  - Design knowledge gives you reduced-form estimands; then plug these into a model to get more!

Heckit Extrapolation of IV Moments



"Heckit" model: $E[Y_i(d)|U_i] = \alpha_d + \gamma_d \Phi^{-1}(U_i)$

Summing Up

- You can do a lot with a solid design-based identification strategy
  - Give a clear *ex ante* rationalization for controls in a linear regression/IV
  - Have confidence in the level of standard error clustering
  - Avoid concerns over “negative weights” / explore alternative weightings
  - Access a large class of formula treatments/IVs
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Thanks for a Great Class!