

# The Role of Models in “Reduced Form” Empirics

Peter Hull

Brown University  
Spring 2024

# Model-Free Empirics?

- The so-called “Credibility Revolution” (Angrist & Pischke 2010) has forever changed empirical practice in microeconomics
- In the '80s, economists would carefully/parametrically model an individuals' decision to select into a treatment (e.g. job training) and use this structure to fight selection bias (Heckman 1974, 1976, 1979)
  - Sometimes the resulting estimates were “preposterously large or outlandishly negative” (Lewis 1986), and not always taken seriously
- Angrist, Ashenfelter, Card, Krueger, Pischke, and others changed the game by thinking hard about *variation*, instead of model structure
  - E.g. find a series of as-good-as-random shocks (e.g. draft lottery #s) or a “clean” policy change (e.g. NJ minimum wage increase)
- Sometimes this approach is termed “reduced form” and viewed as “model-free”.... but is it really?

# Outline for Today

1. OLS & IV Review
2. Models for Specification
3. Models for Extrapolation
4. Models for Identification

# Causal Models

- Let  $X_i$  be a “treatment” of interest and  $Y_i$  be an “outcome.” Suppose:

$$Y_i = \beta X_i + \varepsilon_i \quad (1)$$

specifies how  $Y_i$  is generated from  $X_i$  across units (e.g. individuals)  $i$ .  
Of interest: the *model parameter*  $\beta$

- Ex. 1** (*Potential outcomes*) Let  $D_i \in \{0, 1\}$  indicate treatment receipt and let  $Y_i(0)$  and  $Y_i(1)$  be  $i$ 's outcome when  $D_i = 0$  or  $D_i = 1$ . Then:

$$Y_i = Y_i(0)(1 - D_i) + Y_i(1)D_i = (Y_i(1) - Y_i(0))D_i + Y_i(0)$$

so (1) follows when effects  $Y_i(1) - Y_i(0) = \beta_i$  are constant across  $i$

- Q:** What is a way the general potential outcomes model is restrictive?

## Causal Models (Cont.)

- **Ex. 2** (*Production function*) Let  $Y_i$  measure firm  $i$ 's output,  $L_i$  measure labor,  $K_i$  measure capital, and suppose:

$$Y_i = Q_i K_i^\alpha L_i^\beta$$

for unobserved TFP shocks  $Q_i$ . Suppose  $\alpha = 1 - \beta$  to get to (1):

$$\ln(Y_i/K_i) = \beta \ln(L_i/K_i) + \ln Q_i$$

- **Q:** Suppose we have data on  $(Y_i, K_i, L_i)$  across firms. What would we need to be true in order to estimate  $\beta$ ?

# Natural Experiments

- Given our model,  $Y_i = \beta X_i + \varepsilon_i$ , suppose we know (or assume) that  $X_i$  is drawn at random conditional on  $\varepsilon_i$ 
  - E.g. we flip a fair coin for each  $i$ , regardless of  $\varepsilon_i$ , to determine  $X_i$

- Regression slope coefficient:

$$\frac{\text{Cov}(X_i, Y_i)}{\text{Var}(X_i)} = \frac{\text{Cov}(X_i, \beta X_i + \varepsilon_i)}{\text{Var}(X_i)} = \beta + \frac{\text{Cov}(X_i, \varepsilon_i)}{\text{Var}(X_i)} = \beta$$

I.e. the regression of  $Y_i$  on  $X_i$  *identifies* the model parameter  $\beta$

- More generally, if  $E[\varepsilon_i | X_i, W_i] = E[\varepsilon_i | W_i]$  for some vector  $W_i$ , a regression of  $Y_i$  on  $X_i$  that flexibly controls for  $W_i$  identifies  $\beta$ 
  - How much does this result rely on our constant-fx model  $Y_i = \beta X_i + \varepsilon_i$ ?

# The Robustness of Regression

- Angrist (1998) first showed that a regression on a binary treatment  $X_i$  and strata dummies  $W_i$  identifies a convex average of treatment effects when  $X_i$  is as-good-as-randomly assigned within strata
- More generally, suppose  $Y_i = \beta_i X_i + \varepsilon_i$  with  $X_i \perp (\beta_i, \varepsilon_i) \mid W_i$ . The regression of  $Y_i$  on  $X_i$  w/controls spanning  $\mu_i = E[X_i \mid W_i]$  identifies:

$$\beta^{OLS} = E[\sigma_i^2 \beta_i] / E[\sigma_i^2]$$

where  $\sigma_i^2 = \text{Var}(X_i \mid W_i)$  (see, e.g., Borusyak and Hull 2024)

- Thus, regression identifies a sensible convex average of causal effects even when the constant-effect model motivating it is misspecified

## What About IV?

- Back to the model  $Y_i = \beta X_i + \varepsilon_i$ . Suppose we don't think  $X_i$  is as-good-as-randomly assigned but we do think another  $Z_i$  is
  - We also think  $Z_i$  only affects  $Y_i$  through  $X_i$ , making  $\text{Cov}(Z_i, \varepsilon_i) = 0$
- IV slope coefficient:

$$\frac{\text{Cov}(Z_i, Y_i)}{\text{Cov}(Z_i, X_i)} = \frac{\text{Cov}(Z_i, \beta X_i + \varepsilon_i)}{\text{Cov}(Z_i, X_i)} = \beta + \frac{\text{Cov}(Z_i, \varepsilon_i)}{\text{Cov}(Z_i, X_i)} = \beta$$

I.e. the IV regression of  $Y_i$  on  $X_i$  *identifies* the model parameter  $\beta$

- More generally, if  $E[\varepsilon_i | Z_i, W_i] = E[\varepsilon_i | W_i]$ , an IV regression of  $Y_i$  on  $X_i$  that flexibly controls for  $W_i$  and instruments with  $Z_i$  identifies  $\beta$ 
  - How much does this result rely on our constant-fx model  $Y_i = \beta X_i + \varepsilon_i$ ?



# The Robustness of IV

- Imbens and Angrist (1994) first showed that an IV regression with binary  $X_i$  and binary  $Z_i$  identifies a *local average treatment effect* (LATE): the avg effect for those who comply with the instrument
  - Requires a monotonicity condition: nobody defies the instrument
- More generally, suppose  $Y_i = \beta_i X_i + \varepsilon_i$  with  $Z_i \perp (\beta_i, \varepsilon_i) \mid W_i$ . The regression of  $Y_i$  on  $X_i$  w/controls spanning  $\mu_i = E[Z_i \mid W_i]$  identifies:

$$\beta^{IV} = E[\sigma_i^2 \pi_i \beta_i] / E[\sigma_i^2 \pi_i]$$

where  $\sigma_i^2 = \text{Var}(Z_i \mid W_i)$  and  $\pi_i = \frac{\text{Cov}(Z_i, X_i \mid W_i, \beta_i)}{\text{Var}(Z_i \mid W_i, \beta_i)}$  (see again BH '24)

- Thus, as long as  $\pi_i \geq 0$  (monotonicity), IV regression is robust to misspecification of the constant-effects model motivating it

# Models for Specification

- Even so far, we've seen a role for models in “reduced-form” analyses: specifying the target parameter
  - OLS is remarkably robust to unspecified heterogeneity so long as the treatment is as-good-as-randomly assigned and the controls are flexible
  - IV too, as long as exclusion + monotonicity hold
- But as-good-as-random with respect to what? The model is also needed for saying what  $\varepsilon_i$  is / what you should be concerned about
  - E.g. unobserved TFP shocks in example 2
- Models might also point to other ways basic OLS/IV specs fall short
  - E.g. spillovers, violating even the general potential outcomes model

# Modeling Spillovers

- Suppose a set of new federal subsidies/tariffs  $g_k$  are as-good-as-randomly assigned across industries  $k$ 
  - They shift labor demand; we want to use them to estimate labor supply
  - Why wouldn't we want to just regress industry wages on industry employment, instrumenting with  $g_k$ ?
- General equilibrium models tell us worker mobility causes spillovers:  $g_k$  affects wages/employment of *all* industries within local markets  $i$
- Suppose these models specify regional supply curves  $Y_i = \beta_i X_i + \varepsilon_i$  for wages  $Y_i$ , employment  $X_i$ , and unobserved supply shocks  $\varepsilon_i$ 
  - Can we still use the national  $g_k$  shocks to say something about  $\beta_i$ ?

# Shift-Share Instruments

- Consider  $Z_i = \sum_k s_{ik} g_k$  for some “exposure shares”  $s_{ik}$  with  $\sum_k s_{ik} = 1$ 
  - E.g. baseline industry employment shares, prior to subsidies  $g_k$
- Borusyak et al. (2022) show how  $Z_i$  can be a valid instrument for  $X_i$  in the location-level IV regression of  $Y_i$ , even with endogenous  $s_{ik}$ 
  - Intuition: imagine full industry specialization, i.e.  $s_{ik} \in \{0,1\}$ . Then it's like we have a “clustered” RCT with industry-level shocks
  - More generally, need  $g_k$  to be as-good-as-random with respect to a share-weighted average of the  $\varepsilon_i$
- The shift-share  $Z_i$  acts as a “translation device,” bringing the exogenous industry-level shocks to the specified location-level model
  - See Borusyak and Hull (2023) for a generalization of this approach, for general “formula instruments”  $Z_i = f_i(s, g)$

## Example: Market Access

- Models of economic geography tell us transportation upgrades affect local outcomes (e.g. land value) by increasing market access (MA):

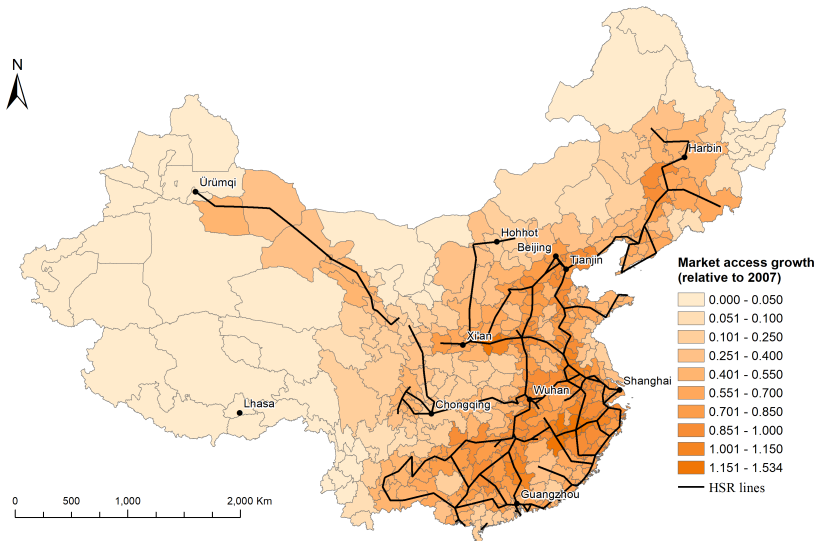
$$\Delta \log V_i = \beta \Delta \log MA_i + \varepsilon_i, \quad (2)$$

$$\text{where } MA_{it} = \sum_j \tau(g_t, loc_i, loc_j)^{-1} pop_j, \quad (3)$$

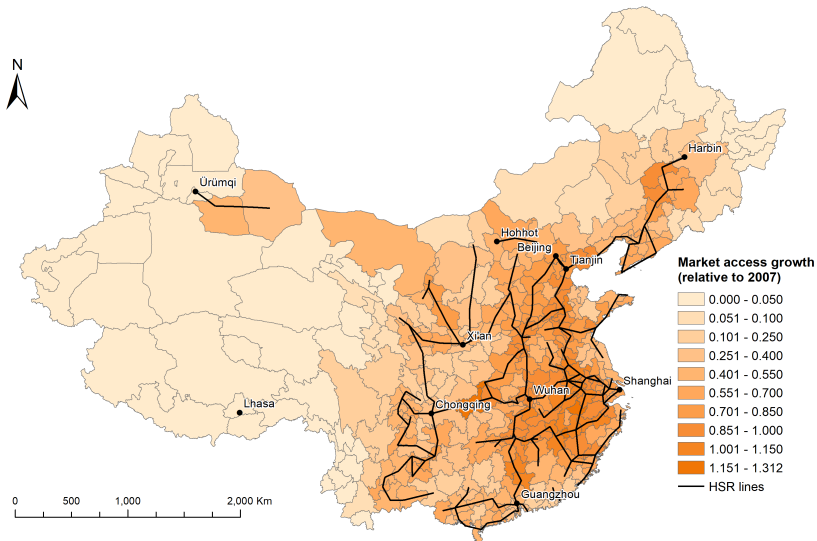
for railroad network  $g_t$  in periods  $t = 1, 2$ , region locations  $loc_j$  (co-determining travel cost  $\tau$ ), and regional population  $pop_j$

- Suppose we have a natural experiment in  $g_t$ : e.g. timing of new line construction is as-good-as-random conditional on plans
  - Further assume these shocks don't directly affect local "amenities"  $\varepsilon_i$
- Could use some shift-share  $z_i = \sum_k w_{ik} \mathbf{1}[\text{line } k \text{ opens}]$  for some  $w_{ik}$ 
  - Or could use the model more directly:  $z_i$  as predicted MA growth from predetermined variables + opening shocks
  - BH '23 show how you can "recenter" these predictions to just use the exogenous shocks for identification....

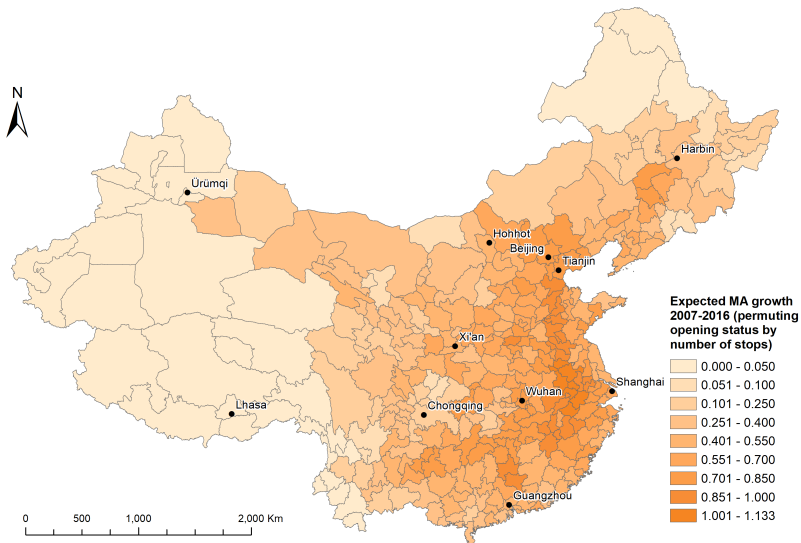
# BH '23: Market Access Growth from HSR Construction



# BH '23: Counterfactual HSR Line Openings + MA Growth

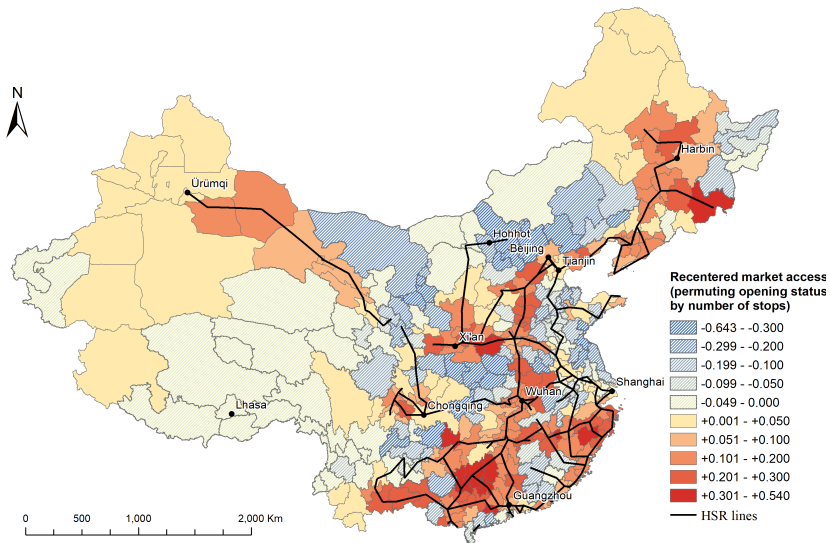


# BH '23: Expected MA Growth Over Counterfactuals





# BH '23: "Recentered" (Realized - Expected) MA Growth



## BH '23: Estimated Market Access Effects

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

Regressions of log employment growth on log market access growth in 2007–2016. Spatial-clustered standard errors in parentheses; permutation-based 95% CI in brackets

# Models for Extrapolation

- Consider the simplest version of Imbens and Angrist (1994): binary  $D_i$ , binary  $Z_i$ , no controls. IV identifies:

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

where  $D_i(z)$  denotes potential treatment when  $Z_i = z$

- LATE: average treatment effect ( $Y_i(1) - Y_i(0)$ ) among compliers (those with  $D_i(1) = 1, D_i(0) = 0$ )
- Without restrictions on  $(Y_i(1), Y_i(0), D_i(1), D_i(0))$ , can't say anything more: IV only reveals effects among  $i$  whose  $D_i$  is shifted by  $Z_i$ 
  - Actually not quite true: can identify avg.  $Y_i(1)$  of always-takers (w/  $D_i(1) = D_i(0) = 1$ ), avg.  $Y_i(0)$  of never-takers (w/  $D_i(1) = D_i(0) = 0$ ), 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-good-as-randomly assigned + excludable

- Assume a distribution for  $(Y_i(1), Y_i(0), v_i)$  where  $D_i = \mathbf{1}[\mu + \pi Z_i > v_i]$
- Then we have parametric models for

$$E[Y_i \mid D_i = 1, Z_i = z] = E[Y_i(1) \mid \mu + \pi z > v_i] \equiv f_1(z; \theta)$$

$$E[Y_i \mid D_i = 0, Z_i = z] = E[Y_i(0) \mid \mu + \pi z < v_i] \equiv f_0(z; \theta)$$

as well as “first stage” models for  $Pr(D_i = 1 \mid Z_i = z) = g(z; \theta)$

- With enough variation in  $Z_i$ , the parameter vector  $\theta$  (and thus ATE) can be identified from these moment restrictions

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

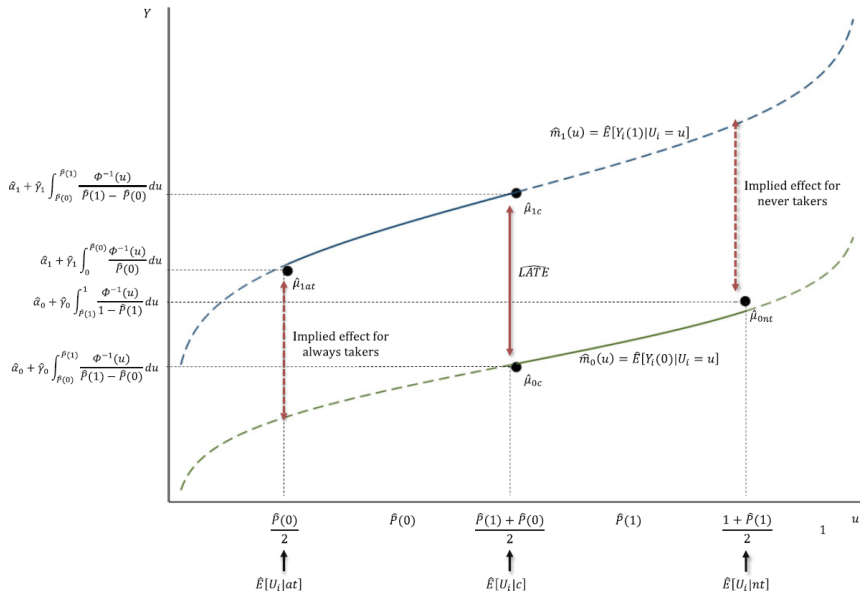
Kline and Walters (2019) formalize this extrapolation logic in the familiar Imbens and Angrist (1994) setup

- Key result: in simple binary  $Z_i$  / no controls setup, control function estimates of LATE are numerically identical to linear IV
- “Differences between structural and IV estimates therefore stem in canonical cases entirely from disagreements about the target parameter rather than from functional form assumptions” (p. 678)
- Functional form instead shapes the extrapolation to other parameters

They conclude with a nice point about validating “structural” models:

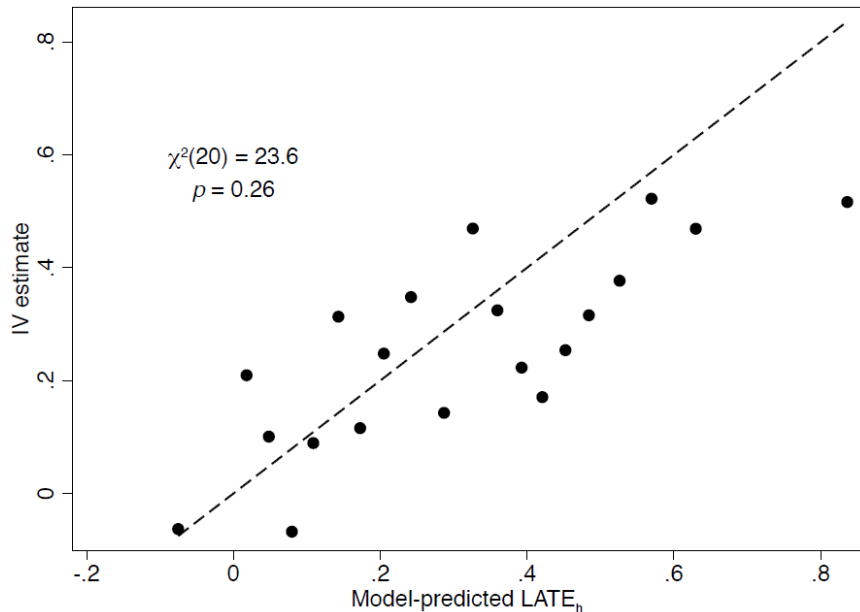
- “Comparing the model-based LATEs implied by structural estimators with unrestricted IV estimates provides a transparent assessment of how conclusions regarding a common set of behavioral parameters are influenced by the choice of estimator” (p. 678)

# Heckit Extrapolation of IV Moments



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

# Validating Structural Models: Kline and Walters (2016)



# Models for Identification

- So far, we've assumed we have as-good-as-randomly assigned and excludable “shocks” and shown how models can help us use them
  - No need to restrict how model unobservables (e.g. potential outcomes) relate to each other or observables (e.g. demographics), except if useful for extrapolating “local” variation to more policy-relevant parameters
- When we do not have such shocks, certain restrictions on unobservables can yield (relatively) transparent identification
  - Popular example: parallel trends. Sse in a panel of  $t \in \{0, 1\}$  we assume  $E[Y_{i1}(0) - Y_{i0}(0) \mid D_i = 1] = E[Y_{i1}(0) - Y_{i0}(0) \mid D_i = 0]$
  - Then in a canonical diff-in-diffs setup:

$$\begin{aligned} & E[Y_{i1} - Y_{i0} \mid D_i = 1] - E[Y_{i1} - Y_{i0} \mid D_i = 0] \\ &= E[Y_{i1}(1) - Y_{i0}(0) \mid D_i = 1] - E[Y_{i1}(0) - Y_{i0}(0) \mid D_i = 0] \\ &= E[Y_{i1}(1) - Y_{i0}(0) \mid D_i = 1] - E[Y_{i1}(0) - Y_{i0}(0) \mid D_i = 1] \\ &= E[Y_{i1}(1) - Y_{i1}(0) \mid D_i = 1] = ATT \end{aligned}$$



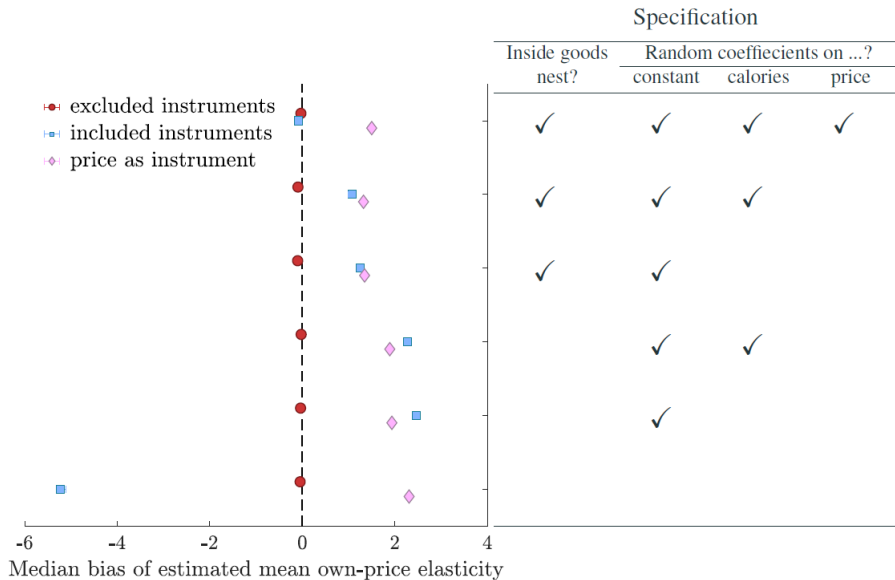
## “Model-Based” Identification

- Notice how  $E[Y_{i1}(0) - Y_{i0}(0) \mid D_i = 1] = E[Y_{i1}(0) - Y_{i0}(0) \mid D_i = 0]$  is a different flavor of identifying assumption: it models unobservables
  - As a result, DiD is highly sensitive to the chosen functional form of  $Y_i$  (Roth and Sant’Anna 2023)
  - This difference is also the source of the recent “negative weights” freakout for TWFE regression (Goldsmith-Pinkham et al. 2024)
- General “model-based” ID strategy: for causal model  $Y_i = \beta X_i + \varepsilon_i$ , sse  $E[\varepsilon_i \mid X_i, W_i] = W_i' \gamma$ . Then  $Y_i = \beta X_i + W_i' \gamma + v_i$  is a regression
  - E.g. if  $i = (j, t)$  indexes a panel with  $W_i$  including  $j$  and  $t$  FE,  $Y_i = \beta X_i + W_i' \gamma + v_i$  is TWFE and  $E[\varepsilon_i \mid W_i] = W_i' \gamma$  is “parallel trends”
  - This works just as well for IV: suppose  $E[\varepsilon_i \mid Z_i, W_i] = W_i' \gamma$ . Then regressing  $Y_i$  on  $X_i$  instrumenting w/ $Z_i$  and controlling for  $W_i$  ID’s  $\beta$
  - Works for fancier models too: e.g. BLP models for demand sometimes assume product unobservables are mean-zero conditional on observed characteristics  $\rightarrow$  “BLP instruments”

# The Fragility of “Internal” Instruments

- Andrews et al. (2023) show instruments constructed from restrictions like  $E[\varepsilon_i | Z_i, W_i] = W_i' \gamma$  are inherently sensitive to model structure
  - In contrast to “strongly excluded” instruments that are as-good-as-randomly assigned + excludable from potential outcomes
- Specifically, they show strong exclusion is sufficient and (essentially) necessary for structural IV to satisfy *sharp-zero consistency*:
  - If true causal effects are zero, the IV estimand reflects this
- Borusyak and Hull (in progress) show how “recentering” can ensure strong exclusion in structural models, given exogenous shocks
  - Stay tuned...

# Andrews et al. (2023) BLP Simulations



Note: simulations based on Miller and Weinberg (2017)

# Takeaways

- “Reduced-form” estimation is not “model-free”
  - At minimum, we need models to guide our choice of  $Y_i$  and  $X_i$ , and to assess when “exogeneity” of  $X_i$  or  $Z_i$  holds via specification of  $\varepsilon_i$
  - Given a well-specified model and natural experiment, we can identify key model parameters (e.g. by recentering)
- Models can help us extrapolate “local” estimands to more generalizable or policy-relevant parameters
  - Great to visualize/test the extrapolation, if possible
- Restrictions on model unobservables can substitute for clean “design” of exogenous observed shocks
  - But beware: estimates are more model-dependent and may be less robust to seemingly harmless deviations (e.g. heterogeneous effects)