Instrumental Variables

IV Frontiers

MIXTAPE SESSIONS

Roadmap

- Judge/Examiner IV
  - Approach
  - Cautions
- Shift-Share IV
  - Approach
  - Cautions
- Other Frontiers
  - Diff-in-Diff IV
  - Recentered IV

Approach

A judge (or examiner) IV design leverages the ideosyncratic assignment of individuals to a set of decision-makers

- Kling (2006): sentencing judges
- Doyle (2007): foster care investigators
- Mayestas et al. (2013): SSDI benefit examiners
- Doyle et al. (2015): ambulance companies

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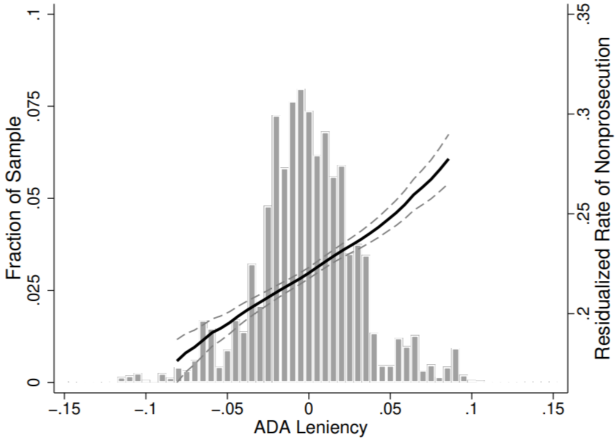
The typical approach is to IV a treatment D_i with a measure of the "leniency" $E[D_i \mid Z_i]$ of one's assigned judge $Z_i =\in \{1, \ldots, J\}$

- E.g. a leave-one-out average, $\hat{L}_i = \frac{1}{|i' \neq i, Z_{i'} = Z_i|} \sum_{i' \neq i, Z_{i'} = Z_i} D_i$

Agan et al. (2021) "Misdemeanor Prosecution"



Note: This figure shows the distribution of our leave-out mean measure of ADA "leniency," residualized by court-by-month and court-by-day-of-week. More lenient ADAs have higher rates of not prosecuting nonviolent misdemeanor cases. The solid line is a local linear regression of nonprosecution on ADA leniency, along with the 95% confidence interval, estimated from the 1st to 99th percentiles of ADA leniency—a local linear version of our first stage. A case assigned to a more lenient ADA (computed using all cases except the current case and other cases with the same defendant) has a higher likelihood of being not prosecuted.

Agan et al. (2021) “Misdemeanor Prosecution”

	(1) Nonprosecution	(2) ADA Leniency
Number Counts	-0.019***	-0.000
	(0.003)	(0.000)
Number Misdemeanor Counts	0.018***	0.000
	(0.004)	(0.001)
Number of Serious Misdemeanor Counts	-0.102***	-0.000
	(0.006)	(0.000)
Misd Conviction within Past Year	-0.068***	-0.001
	(0.005)	(0.000)
Felony Conviction within Past Year	-0.053***	-0.001
	(0.006)	(0.001)
Citizen	0.042***	-0.000
	(0.004)	(0.000)
Disorderly/Theft	-0.014*	-0.001
	(0.008)	(0.001)
Motor Vehicle	0.105***	-0.000
	(0.009)	(0.000)
Drug	-0.094***	-0.001
	(0.009)	(0.001)
Constant	0.224***	0.001
	(0.009)	(0.002)
Observations	67553	67553
Joint F-Test p-value	0	0.234

Note: This table reports regressions testing the random assignment of cases to arraigning ADAs. ADA leniency is estimated using data from other nonviolent misdemeanor cases assigned to an arraigning ADA following the procedure described in the text. Column (1) reports estimates from an OLS regression of nonprosecution on the variables listed and court-by-time fixed effects. Column (2) reports estimates from an OLS regression of ADA leniency on the variables listed and court-by-time fixed effects. Robust standard errors two-way clustered at the individual and ADA level are reported in parentheses. The p-value reported at the bottom of Columns (1) and (2) is for an F-test of the joint significance of the variables listed with standard errors two-way clustered at the individual and ADA level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Agan et al. (2021) “Misdemeanor Prosecution”

	OLS		IV	
	(1)	(2)	(3)	(4)
Panel A: Criminal Complaint Within 2 Years				
Not Prosecuted	-0.14***	-0.10***	-0.34***	-0.33***
	(0.01)	(0.01)	(0.10)	(0.11)
			[-0.55, -0.13]	[-0.54, -0.10]
Mean Dep Var Prosecuted	0.37			
Mean Dep Var Prosecuted Compliers	0.57			
Panel B: Misdemeanor Complaint Within 2 Years				
Not Prosecuted	-0.08***	-0.06***	-0.24***	-0.24***
	(0.00)	(0.00)	(0.09)	(0.09)
			[-0.42, -0.06]	[-0.43, -0.05]
Mean Dep Var Prosecuted	0.24			
Mean Dep Var Prosecuted Compliers	0.40			
Panel C: Felony Complaint Within 2 Years				
Not Prosecuted	-0.06***	-0.04***	-0.10*	-0.08
	(0.00)	(0.00)	(0.06)	(0.07)
			[-0.22, 0.03]	[-0.21, 0.06]
Mean Dep Var Prosecuted	0.13			
Mean Dep Var Prosecuted Compliers	0.17			
Observations	67553	67553	67553	67553
Court x Time FE	Yes	Yes	Yes	Yes
Case/Def Covariates	No	Yes	No	Yes

Note: This table reports OLS and two-stage least squares estimates of the impact of nonprosecution on the probability of a subsequent criminal complaint within two years. The regressions are estimated on the sample as described in the notes to Table 1. The dependent variables are identified in the panel headings. Each panel reports the mean of the dependent variable for all prosecuted defendants, and for prosecuted defendants within the set of compliers. See Appendix C.3 for details on the calculation of mean outcomes among prosecuted compliers. Two-stage least squares models instrument for nonprosecution using an ADA leniency measure that is estimated using data from other cases assigned to an arraigning ADA following the procedure described in the text. All specifications control for court-by-month and court-by-day-of-week fixed effects. Robust standard errors two-way clustered at the individual and ADA level are reported in parentheses in Columns (1)-(4). For the IV estimates, confidence intervals based on inversion of the Anderson-Rubin test are shown in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Caution 1: Monotonicity

“Strict” first-stage monotonicity requires judges to have a common ordering of individuals for treatment

- E.g. no differences in “skill” at identifying appropriate cases

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Imbens and Angrist (& Ridder) saw this coming in 1994!

EXAMPLE 2 (Administrative Screening):[5] Suppose applicants for a social program are screened by two officials. The two officials are likely to have different admission rates, even if the stated admission criteria are identical. Since the identity of the official is probably immaterial to the response, it seems plausible that Condition 1 is satisfied. The instrument is binary so Condition 3 is trivially satisfied. However, Condition 2 requires that if official A accepts applicants with probability $P(0)$, and official B accepts people with probability $P(1) > P(0)$, official B must accept *any* applicant who would have been accepted by official A. This is unlikely to hold if admission is based on a number of criteria. Therefore, in this example we *cannot* use Theorem 1 to identify a local average treatment effect nonparametrically despite the presence of an instrument satisfying Condition 1.

[5] This example was suggested to us by Geert Ridder.

Monotonicity Solutions

Frandsen et al. (2019) formalize a weaker “average monotonicity” condition: intuitively, that skill differences are uncorrelated with TEs

- Similar to de Chaisemartin (2017) “tolerating defiance”
- Also propose non-parametric tests of monotonicity + exclusion (similar to Kitagawa (2015), but with multiple IVs + controls)

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Other tests include checking whether leniency has the same first stage in different subgroups (Norris, 2021)

- Another solution is to parameterize variation in judge skill and estimate it jointly with TEs (Chan et al. 2021; Arnold et al. 2021)

Caution 2: Exclusion

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Like monotonicity, this can be weakened to an “on average” condition

- Kolesár et al. (2015): exclusion restriction violations are uncorrelated with leniency variation (see also Angrist et al. 2021)
- Need many judges for a “judge-level law of large numbers” to kick in

Adding Treatment Channels

Of course if multiple potential treatment channels are observed they can be included + instrumented by judges

- See Autor/Maestas/Mullen/Strand (2017), which adds a decision-time treatment to Maestas et al. (2013)
- Two instruments: examiner leniency and (leave-out) average examiner decision time

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- Two instruments: examiner leniency and (leave-out) average examiner decision time

Careful though: IV with multiple treatments can be difficult to interpret in a LATE framework (maybe OK as a robustness check)

- See e.g. Kirkeboen et al. (2016) and Kline and Walters (2016)

Caution 3: Leniency Estimation

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JIVE may be better with many judge instruments (as it can avoid 2SLS many-weak bias), but it is not bulletproof

- Kolesár (2013) shows many-weak bias can creep back in with many covariates (e.g. court-by-time FE, needed to make judges random)

State-of-the-Art: UJIVE

Kolesár (2013) also derives a solution to many-IV/many-control bias

- "Unbiased" Jackknife Instrumental Variables Estimation (UJIVE) adjusts the leave-out means for controls by (basically) leave-out-Frisch-Waugh-Lovell residualization

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Michal Kolesár, Paul Goldsmith-Pinkham, and I are currently working on a Stata package to implement UJIVE

- We hope to publish it and an accompanying R package soon! In the meantime you’ll be one of the first to beta-test our current code...

UJIVE Repo (In Progress)

`version 0.5.0 14Nov2021` | Installation | Usage | Examples | Compiling

Installation

From the command line:

```
git clone git@github.com:mcaceresb/stata-manyiv
```

(or download the code manually and unzip). From Stata:

```
cap noi net uninstall manyiv
net install manyiv, from(`c(pwd)'/stata-manyiv)
```

(Change `stata-manyiv` if you download the package to a different folder; e.g. `stata-manyiv-main`.) Note if the repo were public, this could be installed directly from Stata:

```
local github "https://raw.githubusercontent.com"
net install manyiv, from(`github'/mcaceresb/stata-manyiv/master/)
```

Usage

```
manyiv depvar (endogenous = instrument) [exogenous], options
help manyiv
```

Roadmap

Judge/Examiner IV

- Approach
- Cautions

Shift-Share IV

- Approach
- Cautions

Other Frontiers

- Diff-in-Diff IV
- Recentered IV

Approach

A shift-share instrument takes the form $Z_i = \sum_n s_{in} g_n$ for a set of *shocks* g_n and a set of *exposure shares* $s_{in} \geq 0$ (for each i)

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- Bartik (1991): national industry employment growth g_n, local industry employment shares s_{in} for regions i
- Autor et al. (2013): increase in (non-U.S.) Chinese import growth across manufacturing industries g_n, local employment shares s_{in}
- Card (2009): growth of immigrant inflows across origin countries g_n, local immigrant shares s_{in}

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The literature has taken two econometric approaches to such Z_i...

Exogenous Shares

Goldsmith-Pinkham et al. (2020) consider the shocks g_n as fixed numbers and consider the "exogeneity" of the shares: $E[s_{in}\varepsilon_i] = 0$

- Often regressions are run in first-differences, so this is like DD-IV
- The twist here is we have many instruments: In Autor et al. (2013) there are 398 industries n (and $1,444$ regional observations!)

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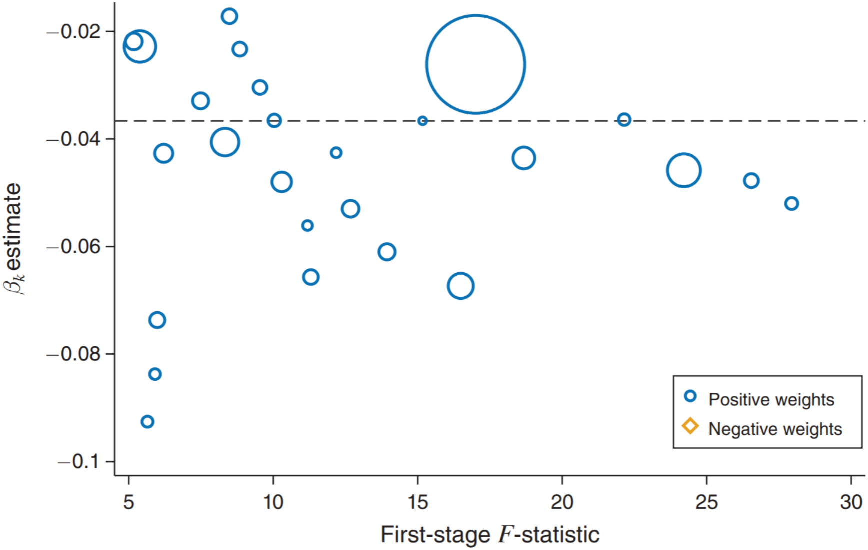
They propose tools to measure the “importance” of different share IVs (“Rotemberg weights”) and discuss other subtlties in estimation

- Kind of like judge IV, except with known “leniency” g_n
- Can check (many) overidentifying restrictions, pre-trends, etc

Rotemberg Weights for Card (2009) Exposure Shares



Source: Goldsmith-Pinkham et al. (2020)

Exogenous Shocks

Borusyak et al. (2022) consider the shocks g_n as exogenous, (quasi-randomly assigned + excludable), conditional on the shares

- E.g. different industries saw higher/lower import growth from China for reasons unrelated to local U.S. employment trends
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They propose tools to test for shock exogeneity (e.g. balance/ pre-trend checks) and quantify the extent of identifying variation

- No overidentifying restrictions: a single instrument g_n, as if we were running an “industry-level” IV regression
- Also show how to relax exogeneity to hold conditional on some observed shock-level confounders

Caution 1: Incomplete Shares

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Borusyak et al. (2022) show this can be a problem if you only want to leverage variation in the shocks and not also in S_i

- Intuitively, if $E[g_n|s] = \mu$ then $E[Z_i|s] = E\left[\sum_n s_{in} g_n|s\right] = \mu S_i$, so the "expected instrument" varies non-randomly across observations
- If S_i is correlated with ε_i, this non-random variation can create bias

Addressing Incomplete Shares

An easy fix to incomplete shares is to control for $S_i = \sum_n s_{in}$

- Alternatively, construct shares such that $S_i = 1$ for everyone
- The former may be more powerful if $X_i = \sum_n s_{in}\tilde{g}_{in}$ for $S_i \neq 1$

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If other controls are needed to make the shocks as-good-as- random (e.g. time dummies, to isolate within-period variation) then S_i needs to be added as an *interaction* with them

- In Autor et al. (2013), this means interacting the manufacturing sum-of-shares with period FE...

Sum-of-Share Controls in Autor et al. (2013)

Table 4: Shift-Share IV Estimates of the Effect of Chinese Imports on Manufacturing Employment

	(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)
<u>Regional controls</u>							
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
# of region-periods	1,444	1,444	1,444	1,444	1,444	1,444	1,444
# of industry-periods	796	794	794	794	794	794	794

Source: Borusyak et al. (2022)

Caution 2: Exposure Clustering

Adáo et al. (2019) show another problem with exogenous shocks: conventional robust/clustered SEs may be wrong

- Intuitively, the structure of $Z_i = \sum_n s_{in} g_n$ may make observations with similar $s_{i1} \dots s_{iN}$ correlated, even when otherwise "far apart"
- They derive non-standard central limit theorems to account for such "exposure clustering" (with R/Stata code)

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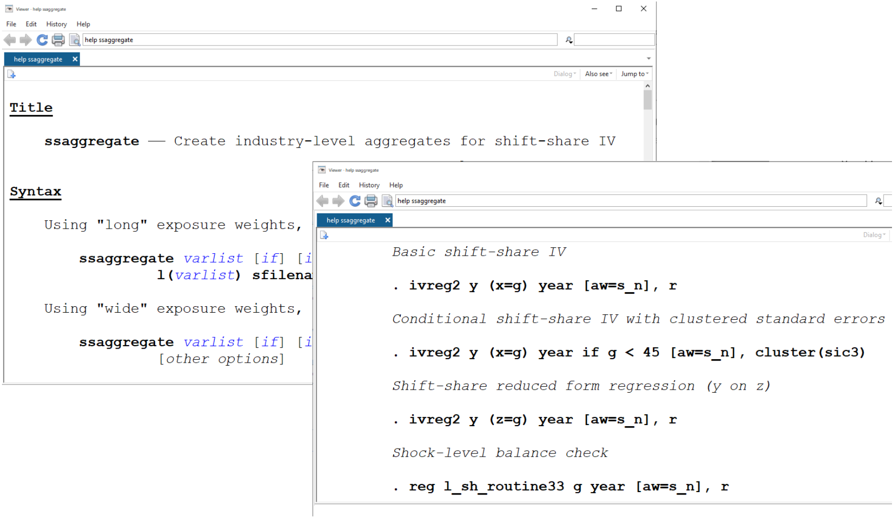
Borusyak et al. (2022) build on this theory to propose an alternative approach: estimate the IV at the level of identifying variation (shocks)

- Derive an equivalent regression where the g_n are used directly as the instrument for shock-level outcomes and treatments
- Standard robust SEs address the exposure clustering problem

Estimating Shock-Level SSIV Regressions



Install in Stata: `ssc install ssaggregate`

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Diff-in-Diff IV

Remember panel data IVs? We haven't talked about them in a heterogeneous-effects setup but Hudson et al. (2017) do just that

- Intuitively, a LATE interpretation requires parallel trends in both the outcome and the treatment and a subtle exclusion restriction: the IV can only affect outcomes in one period
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De Chaisemartin and D'Haultfoeuille propose an alternative "fuzzy difference-in-differences" approach which makes other assumptions

- Key question is whether you think the RF and FS diff-in-diffs are causal or not (if so, keep calm and `ivreg2` on!)

The Recent Diff-in-Diff Literature

You may have noticed there's been, uh, a lot going on with DD recently

- Goodman-Bacon, Sun and Abraham, Callaway and Sant'Anna, Borusyak/Jaravel/Spiess, de Chaisemartin and D'Haultfoeuille ...
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My guess is these problems only get worse with IV (work to be done!)

- But presumably if you can use any of these approaches to estimate the RF & FS, LATE goes through á la Hudson et al. (2017)
- I don't really have anything smarter to say about that for now...

Recentered IV

Remember the "expected instrument" in shift-share IV? It turns out the incomplete shares problem may generalize to related settings

- Network spillover IVs (e.g. Miguel and Kremer 2004)
- Transportation upgrade IVs (e.g. Donaldson and Hornbeck 2016)
- Simulated instruments (e.g. Currie and Gruber 1996)
- Nonlinear shift-share (e.g. Chodorow-Reich and Wieland 2020)

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Borusyak and Hull (2021) develop a general identification framework for IVs combining multiple sources of variation, w/only some random

- Propose "recentering" to avoid bias from non-random "exposure"

The Borusyak and Hull (2021) Proposal

Consider a instrument $Z_i = f_i(g; s)$ for some known mapping $f_i(\cdot)$ of exogenous shocks g and non-random exposure s

- BH show that the *expected instrument* $\mu_i = E[f_i(g; s) \mid s]$ is the sole source of bias and the *recentered instrument* $Z_i - \mu_i$ is free of bias

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2. Recompute $Z_i^{(1)}, \ldots, Z_i^{(K)}$ for each observation i: $Z_i^{(k)} = f_i(\tilde{g}^{(k)}; s)$
3. Average the counterfactual instruments for each i: $\mu_i = \frac{1}{K} \sum_k Z_i^{(k)}$

The Borusyak and Hull (2021) Proposal

Consider a instrument $Z_i = f_i(g; s)$ for some known mapping $f_i(\cdot)$ of exogenous shocks g and non-random exposure s

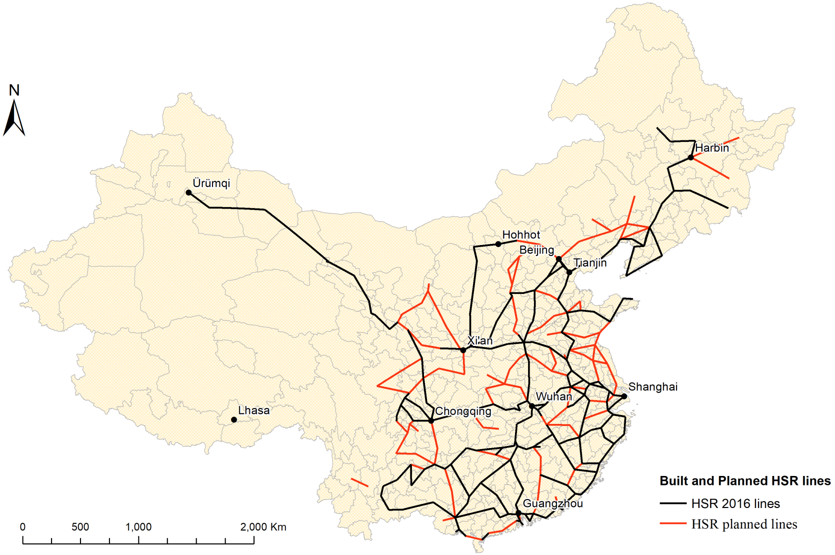
- BH show that the *expected instrument* $\mu_i = E[f_i(g; s) \mid s]$ is the sole source of bias and the *recentered instrument* $Z_i - \mu_i$ is free of bias

μ_i is measured by taking a stand on the shock *assignment process*

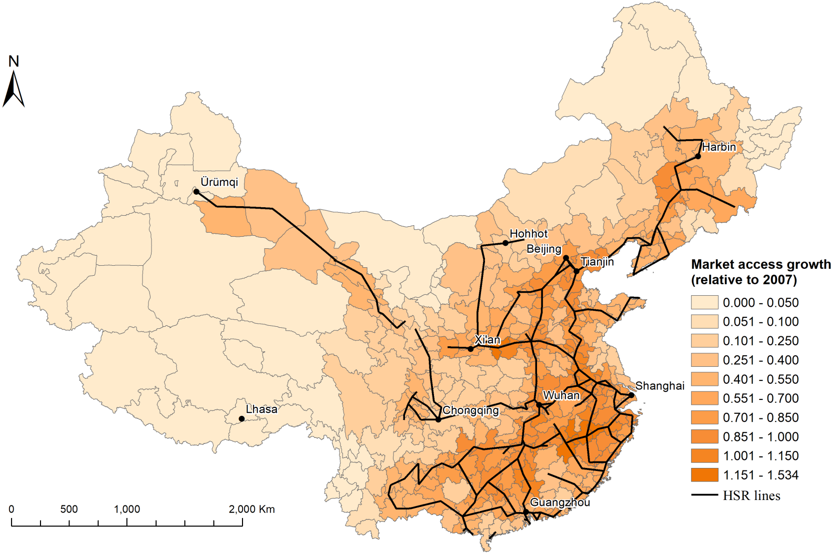
1. Specify *counterfactual* shocks $\tilde{g}^{(1)}, \ldots, \tilde{g}^{(K)}$ which were as likely to have occured (by, e.g., permuting the rows of g)
2. Recompute $Z_i^{(1)}, \ldots, Z_i^{(K)}$ for each observation i: $Z_i^{(k)} = f_i(\tilde{g}^{(k)}; s)$
3. Average the counterfactual instruments for each i: $\mu_i = \frac{1}{K} \sum_k Z_i^{(k)}$

Besides recentering, μ_i can also be controlled for with the original Z_i

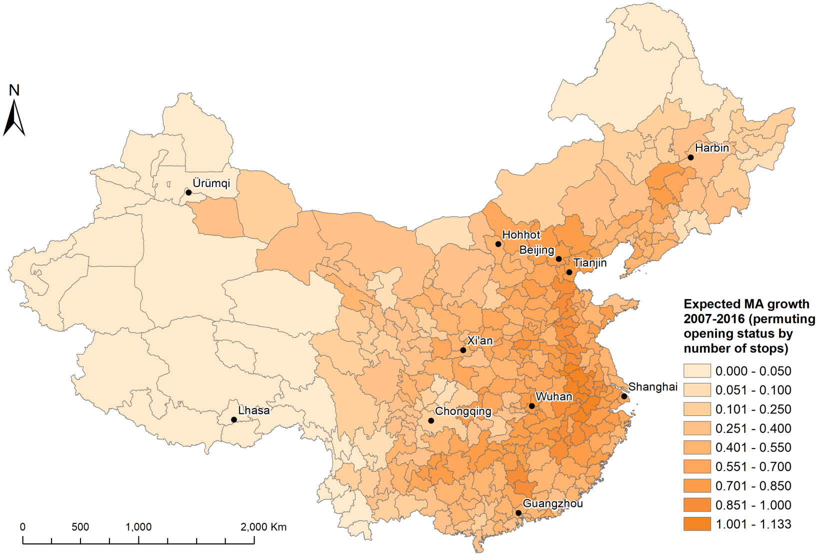
Illustration: High-Speed Rail in China, 2007-2016



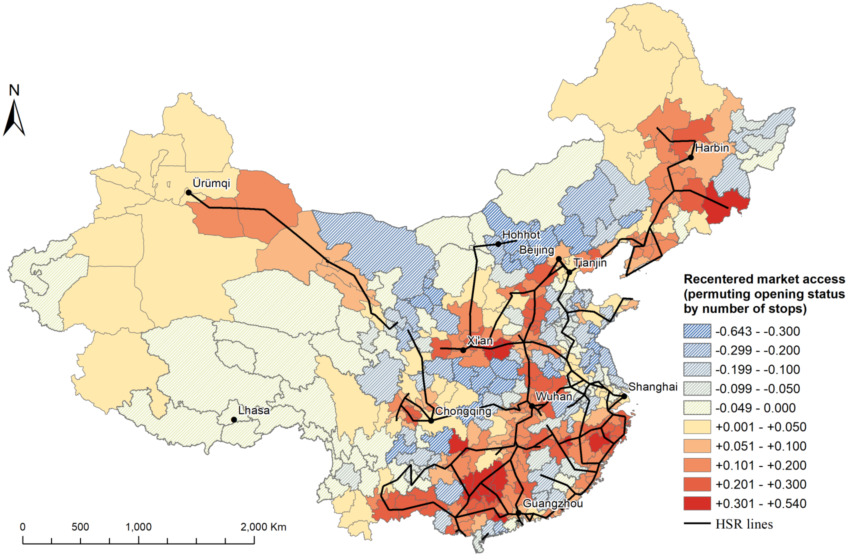
Market Access Growth, Computed from Rail Growth



Expected MA Growth, Assuming Random Rail Timing



Recentered Market Access Growth = Actual - Expected



Recentering Can Matter a Lot Empirically!

	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

Source: Borusyak and Hull (2021)